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TRANSACTIONS OF THE AMERICAN GEOPHYSICAL UNION

NINETEENTH ANNUAL MEETING
APRIL 27 TO 30, 1938
WASHINGTON, D. C.

REGIONAL MEETINGS

DECEMBER 28 TO 29, 1937, SPOKANE, WASHINGTON
JANUARY 7 TO 8, 1938, DAVIS, CALIFORNIA
JUNE 21, 1937, DENVER, COLORADO (ADDENDUM)

PART I

REPORTS AND PAPERS, WASHINGTON, D. C.

GENERAL ASSEMBLY AND
SECTIONS OF GEODESY, SEISMOLOGY, METEOROLOGY,
TERRESTRIAL MAGNETISM AND ELECTRICITY,
OCEANOGRAPHY, VOLCANOLOGY, AND HYDROLOGY

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PART II

REPORTS AND PAPERS

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WESTERN INTERSTATE SNOW-SURVEY CONFERENCE, DAVIS, CALIFORNIA
JANUARY 8, 1938

[Part II includes papers before Section of Hydrology at Spokane in 1937, at Davis in 1938, one at Denver in 1937, and before Western Interstate Snow-Survey Conference at Davis in 1938]

INTRODUCTION

The American Geophysical Union was established in 1919 as the American Committee of the International Union of Geodesy and Geophysics, and its Executive Committee is the Committee on Geophysics of the National Research Council. The objects of the Union are to promote the study of problems concerned with the figure and physics of the Earth, to initiate and coordinate researches which depend upon international and national cooperation, and to provide for their scientific discussion and publication. In the accomplishment of these objects, the Union is divided into sections following the plan of organization of the International Union of Geodesy and Geophysics. There are now seven sections, namely, (a) Geodesy, (b) Seismology, (c) Meteorology, (d) Terrestrial Magnetism and Electricity, (e) Oceanography, (f) Volcanology, and (g) Hydrology. A Section of Geophysical Chemistry was discontinued May 31, 1924, as the International Union had failed to provide such a Section; the Section of Hydrology was established November 15, 1930--matters pertaining to scientific hydrology referred to the American Geophysical Union had been previously looked after by special committees on Hydrology.

Annual assemblies of the Union and annual meetings of its sections for the discussion of reports and for symposia are held each year. The first 13 assemblies and meetings--those in each of the years from 1920 to 1932--were held in Washington, D. C. Besides the annual general assemblies and meetings of each of the seven sections held in Washington, D. C., meetings of the Section of Hydrology were held at Berkeley, California (1933 and 1934), at Pasadena, California (1936), at Denver, Colorado (1937), at Spokane, Washington (1937), and at Davis, California (1938). The transactions of annual assemblies and meetings through the ninth were published in the Bulletin series of the National Research Council as follows: Second, 1921, Bulletin No. 17; fourth, 1923, Bulletin No. 41; sixth, 1925, Bulletin No. 53; seventh, 1926, Bulletin No. 56; eighth, 1927, Bulletin No. 61; and ninth, 1928, Bulletin No. 68. Transactions of the tenth and eleventh annual meetings held in 1929 and 1930 were published in one volume in June 1930, and those of the twelfth, thirteenth, fourteenth, fifteenth, sixteenth, seventeenth, and eighteenth annual meetings were published in June 1931, June 1932, June 1933, June 1934, August 1935, July 1936, and July 1937, respectively, in the series of miscellaneous publications of the National Research Council. The present volume, also one of the miscellaneous publications of the Council, includes the transactions of the nineteenth annual meetings held at Washington, D. C., April 27 to 30, 1938, and of the meetings of the Section of Hydrology at Spokane, Washington, December 28 to 29, 1937, and at Davis, California, January 7 to 8, 1938. (There is also included in the present Transactions one paper presented at the Denver meeting of 1937, manuscript for which was not received in time for publication in the volumes for 1937.) At the first, third, and fifth annual meetings in 1920, 1922, and 1924, respectively, there were no scientific papers presented; therefore, the transactions for these were not printed but were mimeographed for limited distribution only.

Because of the importance of the minutes of the General Assembly and of those of the sections, with their appendices, in the general view of the development of the Union and of their interest to all of its members, they have been included, for the first time, as a part of these Transactions. The total attendance of members and guests--534 at Washington--marks the character of the meetings in 1938 and the growing import of the Union as a factor in the progress of geophysics in the United States. The attendance at the regional meetings also emphasizes this progress.

Included in these minutes are the reports of officers and of standing, special, and research committees. Especially noteworthy are those of the newly-formed Planning and Project Committee and of the two committees on Geophysical and Geological Study of Oceanic Basins and on Geophysical and Geological Study of Continents. These show the many problems calling for intensive theoretical research and potential possibilities of their applications in profound problems of Earth physics.

Four of the six resolutions adopted at the General Assembly relate to magnetic survey of the land- and water-areas of the United States, geophysical survey of the Central Atlantic States, location of meteorological stations, and meteorological research-projects. The other two resolutions express thanks for privileges extended at the Washington meetings by the Smithsonian Institution and the United States Geological Survey.

Preparations are well advanced for the seventh triennial assembly of the International Union of Geodesy and Geophysics to be held in September 1939 at Washington, D. C. During the nineteenth annual meeting of the American Geophysical Union, one evening was devoted to a smoker to discuss these preparations and to formulate further plans. This will be the first assembly of the International Union in the United States since its formation in 1919. It will afford opportunity to reciprocate the courtesies shown us at the six assemblies in Europe and to inform our foreign colleagues of the extent of geophysical research and interests in the United States. It is hoped especially that all the nations of South and North America will avail themselves of the opportunity to take active part in the Washington assembly.

The scientific session of the General Assembly in 1938 was devoted to a symposium on the physics of volcanic processes. Five formal papers presented were: Introductory--applying physics to volcanoes; surface-manifestations of volcanic activity; structural development of volcanic cones; rock-structures associated with some ancient volcanoes; the roots of volcanoes. Following a summary of the symposium by L. H. Adams, there was an extended and searching discussion.

In the Section of Geodesy 15 papers and reports were presented at two sessions. Five of these dealt with progress and development of geodetic operations and instruments in Canada, Central America, Mexico, and the United States; seven related to gravimetric surveys, apparatus, interpretations, and relation of gravity-anomalies and geologic structure; one each related to improvement of time-service and broadcasting, to sea-level datum, and to the mathematical treatment of accidental and other errors.

The Section of Seismology held one session. The 12 communications may be classified as follows: Theoretical interpretations and analysis, 4; individual earthquakes and seismic measurements, 2; seismic instruments, 3; microseisms, 1; geophysical investigations of geologic structure, 1; progress-report for the United States, 1.

The Section of Meteorology heard six papers at one session. These concerned ultra-violet solar radiation (2), velocity of sound-waves (1), compilation of observations (1), expeditions (1), and meteorological aspects of radio-transmission phenomena (1).

Sixteen communications were presented at the session of the Section of Terrestrial Magnetism and Electricity. These related to instruments and technicue (3), ionosphere and magnetic correlations (2), cosmic radiation (2), solar relations (1), fossil magnetization as determined from ocean-bottom core-samples (2), secular variation (1), auroral-zone currents (1), possible seismic and magnetic relations (1), and terrestrial electricity (3). The Secretary submitted brief summaries of progress-reports dealing with magnetic and electric researches received from five organizations on work in Peru, Western Australia, and the United States, including Alaska, Hawaii, and Puerto Rico.

Ten papers before the meeting of the Section of Oceanography concerned progress during the year of four governmental and private organizations doing oceanographic work in the United States. One paper each related to the Gulf Stream System, deep-sea measurements without cables, chemical oceanography, an international program for collecting samples of ocean-water, and methods of representing suboceanic relief on maps. A report from the International Commission on Continental and Oceanic Structure gave important features relating to an international program arranged for cooperative gravimetric work at sea.

At two sessions of the Section of Volcanology 12 papers dealing with igneous rocks and effects of temperature and pressure were read and discussed.

One of the important features of the nineteenth annual meeting was a joint meeting of the sections of Meteorology and Oceanography. The 13 papers presented at the two sessions of this joint meeting were devoted to a symposium on atmospheric and oceanic circulation.

There were four sessions of the Section of Hydrology for the presentation of reports and papers. Twenty-four papers, with discussions, may be roughly grouped as follows: Rainfall, infiltration, and ground-water, 15, and one appendix; stream-flow and flood-control, 6; and one each on evaporation, on glaciology, and on contributions of the Water Resources Committee to hydrological investigations. The annual reports of permanent research committees were received and discussed as follows: (1) Snow; (2) glaciers; (3) evaporation from water-surfaces; (4) runoff; (5) physics of soil-moisture; (6) underground waters, with three appendices; (7) dynamics of streams; (8) chemistry of natural waters; (9) rainfall. Reports were also presented by newly-formed special committees as follows: (1) Soil Conservation Service; (2) flood-waves, with six appendices; (3) density-currents. The total of 45 reports and papers affords evidence of the activity of the Section.

The regional meetings of the Section of Hydrology at Spokane, Washington, and Davis, California, were arranged by special committees of which W. A. Lamb and Morrough P. O'Brien were chairmen, respectively. Only three formal hydrological papers were presented before the North

Continental Divide Regional Meeting at Spokane, the time being largely devoted to conferences and discussions; this meeting was held in affiliation with the Northwest Scientific Association.

The South Pacific Coast Regional Meeting at Davis, California, was held jointly with the Western Interstate Snow-Survey Conference. Sixteen formal hydrological papers were presented at three sessions, January 7 and 8, 1938. These related to hydrometeorology, precipitation and vegetation, evaporation, transpiration, forest and range hydrology (in relation to water-supply, forest management, livestock industry, and flood-control), floods, flood-control, evaporation and runoff from snow, and studies in quality of irrigation-water. Nineteen reports and papers were presented and discussed at the Snow-Survey Conference on development of snow-surveying, methods of forecasting, economic aspects of snow-surveying, improvement of snow-survey equipment, and winter sports. This meeting was concluded with a round-table conference and dinner.

The General Secretary edited the manuscripts for publication after some preliminary editing by the secretaries of sections and by the chairmen and Dr. J. E. Church for the regional meetings. The present Transactions include, either in full or in abstract, 140 papers and reports presented during the nineteenth annual meeting at Washington and some 38 papers and reports (not elsewhere published) with discussions presented during the meetings of the Section of Hydrology and of the Western Interstate Snow-Survey Conference at Spokane and Davis. The cost of publication was met through allotment by the National Research Council for maintenance of the Union, annual dues of members of the Union, and contributions of money and of services from the Carnegie Institution of Washington, from the University of Nevada, from members of the Union, and especially from hydrologists both members and non-members. W. E. Scott proof-read the typed copy, and the authors, except for a few who could not be reached, also revised their matter as prepared for publication. The final copy was prepared by J. J. Capello with the assistance of R. O. Alpher, and was planographed by the Columbia Planography Company of Washington. Besides its obligation to authors, to members of committees, and to our colleagues in Central America and Mexico, who contributed to the nineteenth annual meeting, the Union is indebted to W. C. Hendrix for preparing many drawings of which the originals submitted were not suited for planographic reproduction and to P. G. Ledig for photographic work.

These Transactions afford further evidence of the scientific and economic value of geophysical research and of the contribution of the Union through its coordination of many agencies. Further substantial progress may be confidently expected through the activities of the special committees of the Union and of its sections, including the newly-created Planning and Project Committee.

Jno. A. Fleming, General Secretary,
American Geophysical Union

AMERICAN GEOPHYSICAL UNION

Nineteenth Annual Meeting

GENERAL ASSEMBLY

Washington, D. C.
April 29, 1938

Reports and papers

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SYMPOSIUM ON THE PHYSICS OF VOLCANIC PROCESSES

INTRODUCTORY--APPLYING PHYSICS TO VOLCANOES

Arthur L. Day

In introducing a constructive discussion of the physics of volcanic processes it will suffice to provide a very brief historic background against which the distinguished students of this subject, who will follow me, may present the current status of such studies and the outlook for the future.

In a volume entitled "Das Magma und seine Produkte" [I. Teil, Leipzig, 1937], which is just off the press, Dr. Paul Niggli has given a most interesting résumé of the long record of experimental efforts to elucidate these processes. The speculations of the early Greeks, who sought to explain the activity of Etna, and their Italian successors beginning with Pliny's description of the eruption of Vesuvius in 79 A.D., have brought us little actual evidence that is useful today. They do not go beyond the effort to reconcile the supposedly incompatible activities of the primitive elements air and fire and water, and the earlier accounts are rather highly flavored with instances of arbitrary interference by the Gods.

To Spallanzani, and two contemporaries in the eighteenth century, Niggli attributes the first systematic approach to experimental geology and the definite abandonment of the idea that volcanic outbreaks are the result of ordinary combustion. Spallanzani pointed out clearly that the outpourings from volcanoes do not burn upon reaching the air, as they must do if the energy was supplied by burning sulphur, for example. He and his contemporaries, Hall and de Saussure, appear to have been the first to try to remelt the lavas in glass furnaces and to study the products. Of course, the products when quickly cooled were glasses and appeared uninstructive. Slow cooling produced notable differences, but they were wrongly interpreted. Nevertheless these early experimenters did not fail to notice the porosity of surface-lava and the obvious escape of gases during eruption, and so recorded their belief that volatile ingredients participated effectively in the eruptive process. Neither did Spallanzani fail to recognize that these volatile ingredients were original constituents of the discharging magma, where others sought to discover some mechanism whereby water or oxygen might be absorbed into the magma at the surface.

Notwithstanding some limitations of interpretation, the point of view at that time was so intelligently thought out that de Saussure set down categorically, for students of the subject, 48 specific subjects of inquiry which might today serve as starting points for a program of volcano-investigation.

Spallanzani and de Saussure both died in 1799, but the record of their activities provides a definite and constructive starting point for the efforts which have followed. Indeed, if de Saussure's 48 points could have been nailed to the wall and followed as a program of investigation thereafter much of the fog which enveloped the efforts of the following half century could hardly have arisen. The quarrels of the plutonists and neptunists have little interest for us today, nor need we consider the long discussion whether or not basalt is a volcanic rock and granite original crust. Though there are good descriptions of numerous eruptions of Vesuvius in the first half of the nineteenth century, and even earlier, they do not contribute materially to the analysis of the volcano-problem.

The name which stands out most conspicuously in establishing the next constructive landmark in this field is Poulett-Scrope, who appears to have been the first to sift out logically the wheat from the chaff in the vast mass of description and speculation through the century immediately preceding. He recognized clearly the fact that under pressure water would be a most effective agent in accounting both for the fluidity of the lava and the gaseous phases of volcanic eruption, and that all the reactions and relations necessary to such explosive outbreaks must develop below ground, the explosive activity merely indicating the release of these forces following the weakening and eventual rupture of the overlying crust. Of course, all the details have been greatly clarified by subsequent observation and analysis, but the first clear picture of the mechanism of a volcano and the origin and progress of the forces released in the surface-explosion appears to be that of Poulett-Scrope [1825 and 1862].

There was energetic opposition to these views, particularly in regard to the role and the identity of the volatile ingredients, in which the name of Lyell appears in the forefront, all of which culminated in our own century in the publication of Albert Brun's work "l'Exhalaison Volcanique" [1911]. Brun sought to prove by studies in the field and by laboratory-investigation, which included the chemical analysis of personally collected gas-samples, that volcanic explosions were strictly anhydrous. Brun took no account of the great body of opposing field- and laboratory-observations which had made up Poulett-Scrope's evidence and had determined his conclusions therefrom. Nor did he associate with the eruptions the fact recorded by so many observers through the previous century and a half, that volcanic outbreaks were almost always followed by showers of rain which could represent nothing other than the condensation of escaping steam. He collected his gases at the volcano-sources and determined their content chemically. Chief reliance appears to have been placed upon his collections at Kilauea, where activity was continuously accessible without threat of violent explosive features.

It was this book of Brun which brought the Geophysical Laboratory into the picture and initiated my personal contact with the volcano-problem. In the summer of 1912 we followed Brun to Kilauea and after a rather thorough reconnaissance succeeded in collecting gases directly from the liquid lava without contamination by air, which Brun had not attempted to do. Without canvassing the details it is probably sufficient to say that we collected nearly a pint of water in a small condensing system in a few moments under conditions quite above reproach and several liters of associated gaseous products, all of which have been described in detail elsewhere. We were also able to show that the gases contained sufficient sulphuric acid to account for the apparently anhydrous character of the volatiles at the point where Brun's collection was made (in the open air some 200 feet distant from the point of emergence). This seemed to settle for all time one of the most controversial points in the long history of volcano-investigation, namely, the general participation of magmatic water (and other volatiles) in all volcanic activity.

Our interest thus aroused did not stop with establishment of the fact that the role of water was the most critical single factor to be studied in elucidation of the volcano-process. There were other factors of prime importance such for example as the participation of hydrogen sulphide, of chlorine, of fluorine. There was also the vast problem of the relations between the mineral components of the magma during the process of solidification and the effect upon this process of greater or smaller quantities of volatile components present with them which together have formed a major part of the program of the Geophysical Laboratory since that time.

There have also been the wonderful field-observations which have been carried out in Italy and in the West Indian Islands by Mr. Perret as Research Associate of the Carnegie Institution of Washington, attached to the staff of the Geophysical Laboratory. It is the work of Perret, and the fact that he was personally invited to participate in this Symposium and at the last moment was unable to leave Martinique, which brings out opportunely my particular text in introducing this discussion, namely, the considerable number of effective physical tools recently developed and generally available for the more competent study of volcano-phenomena in the field. A number of these simple instruments have been developed by Perret himself, notably his field-microphone through which he has been able to analyze the effective progress of the activity within the shell of an active volcano. With this instrument, in forms adapted to conditions and places, Perret succeeded in a confident forecast of the progress of the latest eruption of Mont Pelée in Martinique, which enabled neighboring inhabitants to return to their crops after being evacuated by Government order. With this simple instrument and others, which he had planned to describe more fully, if he had been able to join us on this occasion, he has given us one of the most complete and intimate records of field-observations of a single volcanic outbreak which is available to us [Frank A. Perret, The eruption of Mt. Pelée 1929-32, Pub. Carnegie Inst. Washington, No. 458, 1935).

It is probably not in my power to describe the uses of these instruments as he would have done if personally present with us, but it is pertinent that I point out to you, as he wished me to do, the potential importance of their use in the field in any competent analysis of the details of the progress of an eruption. He has contrived in this way to remain out of the danger zone and at the same time intimately in contact with the development and release of power at the center of activity.

It is also pertinent in introducing this discussion that I mention the successful use of portable seismographs which both Perret and Dr. Jaggar have developed in an effort to forecast the approaching culmination of the turbulent activity below ground. I think the seismograph has already proved itself to be indispensable to such studies as Perret and Jaggar have made and through which our knowledge of the entire phenomenon of volcanic eruption has been enormously clarified.

There is also the possibility of finding application for a considerable number of instruments and processes developed mainly for the discovery of petroleum-deposits which might find profitable application in the study and delimitation of bodies of magma where they approach close to the surface and perhaps threaten danger to life and property. These developments, depending variously upon the elastic, electrical, or magnetic properties of the Earth's crust, or even upon the distribution of gravitational differences, have hardly been tried out in volcano-regions, nor have they found adequate application in the study of structural geology generally. Nevertheless, in the field of petroleum-exploration the extent of their application, the sensitiveness of their measurements, and their general competence for subsurface explorations appear to have been completely demonstrated. Why then may we not look forward to the successful application of these and other competent instruments in the field to give us a real insight into the mechanism of our volcanoes, and with them to supplement the extensive and detailed investigation of the components of the magma, both volatile and other, in the laboratory.

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SURFACE-MANIFESTATIONS OF VOLCANIC ACTIVITY

E. G. Zies

The phenomenon of volcanic activity has occupied the attention of observers throughout the ages and will continue to do so if for no other reason than to try to satisfy our curiosity as to the cause of this outward expression of the forces beneath the Earth's crust and therefore removed from direct observation. The extrusion of hot materials either solid or fluid is our only direct evidence that what we call igneous rocks were once molten. It seems obvious, therefore, that if the study of the extruded lava and its related products can be coordinated with laboratory-investigations we shall have taken a step in the direction of finding that much-sought-for logical generalization as to the cause of volcanic activity.

It is perhaps not amiss to point out that a number of volcanic areas are located in such densely populated and fertile regions as the Lesser Antilles, the Pacific Coast area of Central America, Japan, the Netherlands East Indies, Italy, and Greece. It is not at all unlikely that there may be a growing demand for prediction of volcanic activity in order to mitigate the effects of a disastrous eruption. Somewhat more than fair success has been attained in this direction, but we still do not have the empirical background which will permit even reasonably accurate prognostication. This lack of knowledge is in itself an incentive to further study.

It is not, however, the purpose of this paper to discuss probable causes of volcanic activity; we are here concerned only with a review of the salient features of the surface-manifestations. Scrope [see 1 of "References" at end of paper] has condensed our knowledge concerning these manifestations into the simple and clear statement that volcanoes "are characterized by 'Eruptions'; that is, the forcible expulsion of heated matters, gaseous, fluid, or solid (usually all three together), from the interior of the Earth upon its surface accompanied or preceded by earthquakes of a minor or local character." It will be noted that no mention is made of the obvious feature of a volcanic cone which most of us have in mind whenever we think of volcanic activity. If the eruptive products can accumulate over a long period of time, a volcanic cone will be built up, but it is not at all a necessary consequence of volcanic activity. The great eruption of Laki [2] on Iceland in 1783 opened a fissure in a relatively flat field and from it issued a flow that covered an area of some 560 square kilometers with about 12-1/3 cubic kilometers of a fairly mobile basaltic lava. Another example is the flow of rhyolitic pumice that took place in 1912 in the Valley of Ten Thousand Smokes [3]. Fenner [4] estimates that about a cubic mile of this rhyolitic fragmental material was blown through the floor of the old valley and covered an area of 48 square miles to a depth of at least a hundred feet. The actual orifices are covered by the ejected materials.

It is a matter of considerable interest that Scrope in his definition of an eruption placed gaseous matters first in his list of ejected materials. He was led to this by the obvious condensation of water and the obvious presence of hydrogen sulfide and carbon dioxide during volcanic activity. He also inferred the presence of gaseous materials from the porosity and vesiculation found in many lavas, and also from the fact that the analysis of igneous rocks revealed the presence of water. Ste. Claire Deville [5] in his pioneer work had shown that over 99 per cent of the volcanic gas is water in the form of steam. Scrope argued that in all probability the ascensive property of the lava was due to the water which it contained, as he states, interstitially. The quantitative basis for the idea that within the constraining mantle the water could be in solution in the liquid lava and that enormous pressures would be available when crystallization had progressed was not developed until recent times [6]. It is true that Brun [7] denied that water was present in lavas and especially in the lavas of Kilauea, yet it has been since proved by Day and Shepherd that his deductions were in error [8]. Many other gases are present in the lava, but Shepherd has shown that in the rather fluid basaltic lavas of Kilauea about 70 per cent of the gas given off is steam. It is quite difficult to obtain gas-samples directly from most lava-flows because it is physically impossible to obtain them during an explosive eruption, which is the dominant type of activity at the present time. Nevertheless, the accumulated evidence obtained by many observers in the field, the evidence furnished by the

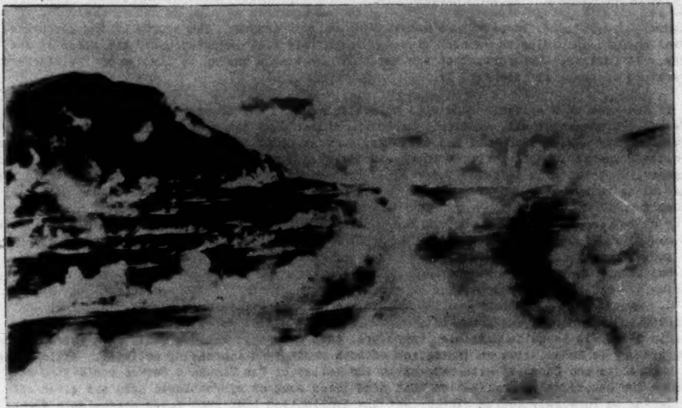

FIG. 1—MAIN ARM OF THE VALLEY OF TEN THOUSAND SMOKES
(SUPERHEATED STEAM WAS ACTIVELY EMITTED IN 1919; SEVEN YEARS AFTER ERUPTION)
PHOTOGRAPH BY R. F. GRIGGS—
COURTESY NATIONAL GEOGRAPHIC SOCIETY

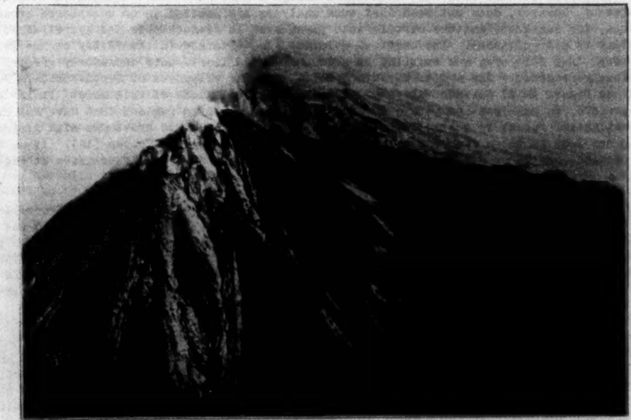

FIG. 2—THE VOLCANIC DOME, 1928, OF MERAPI IN MIDDLE JAVA
(THE DOME WAS CORRODED BY ACTION OF SUPERHEATED STEAM AND ACID GASES)
PHOTOGRAPH FROM N. J. M. TAVERNE [23]

analysis of the emanations given off after an eruption and by the analysis of igneous rocks, the laboratory-experimentation under controlled conditions of temperature and pressure of the effect of water on igneous materials, all show that even though the weight percentage of gases in rocks is low, yet they are an important factor in maintaining fluidity and in providing, at least in part, the ascensive properties observed during an eruption.

Gases of various kinds, especially water-vapor and hydrogen sulfide, often continue to escape from igneous materials for many years after extrusion and give rise to what is called the solfatara stage of activity. The eruption in the Valley of Ten Thousand Smokes took place in 1912, yet in 1919 active expulsion of gases with temperatures as high as 640° C was still in evidence [9]. In 1923, however, the activity had almost ceased. The source of the gases and thermal energy, according to Fenner, lies in a rhyolitic sill injected under the floor of the Valley. It follows, therefore, that the amount of available energy is limited by the size and temperature of the sill (see Fig. 1).

In the case of a volcanic cone with an active hearth, the sequence of events may be quite different. One example will suffice. Merapi in Middle Java has been intermittently explosively active for hundreds of years and has exhibited persistent solfataric phenomena during the interval between the periods of the more intense activity. Similar conditions prevail at many other volcanoes, especially at Vesuvius. Obviously the energy in the hearth of the volcano is in some manner being rejuvenated. Serious attempts are being made at Vesuvius, Kilauea, and at Merapi to keep a continuous record of this type of activity, but in many cases it is well nigh impossible to do so, for the eruptive periods are too far apart in time to justify such intensive work. Even casual visits, however, can be useful. The writer, through the courtesy of the Netherlands East Indies Survey, was able to make the ascent of Merapi in 1928 (see Fig. 2). The volcanic dome had become distinctly more active than was indicated by previous records--there was copious emission of acid gases such as sulfur dioxide and hydrochloric acid at temperatures as high as 650° C. On the basis of this increased activity and the previous record of Merapi eruptions, it was believed that Merapi was going to stage another paroxysmal outburst, which actually took place in 1930. It is quite possible, therefore, that such observations will, in a volcanic area whose history is known, give an indication of both waning and increasing activity. While it is true that Allen and Zies [9] with respect to the Valley of Ten Thousand Smokes could find no evidence for Deville's generalization that acid gases such as hydrochloric acid are given off only during an intensely active period, yet the implication that such gases predominate during the period leading up to an eruption seems to be well founded. It is well to bear in mind before closing this discussion of gases given off as a surface-manifestation to emphasize the importance of continuity of observation with respect to temperature and percentage amount of gas given off. It is becoming more obvious [10] that occasional analyses have but little meaning in themselves. This statement, however, does not mean that such analyses are useless in an unexplored area. We do not know, for instance, whether certain acid gases such as hydrochloric and hydrofluoric acid are peculiar to all volcanoes. The chemical evidence [11] obtained in the Valley of Ten Thousand Smokes showed that this area was emitting an amount of hydrofluoric acid enormously greater than any previously recorded. The writer has since found that the volcanoes of Santa Maria in Guatemala and San Miguel in El Salvador are also giving off large amounts of this acid. It is desirable, therefore, to make gas-analyses even if only isolated ones in regions that have not as yet been investigated, for it is quite possible that there may be magmatic provinces with respect to certain gases just as there are magmatic provinces for various types of rocks [12]. If we hope to make predictions, however, then it will be necessary to analyze the emitted gases at regular intervals.

The second part of Scrope's definition of an eruption calls attention to the obvious and dominating feature of volcanic activity, namely, the forcible expulsion of heated solid and fluid materials. We have seen above that when the physical-chemical environment no longer permits the gases to remain in solution in the lava they play an important part in bringing the heated material to the surface. The manner in which they are ejected and deposited depends on the fluidity, silica-content, temperature, and gas-content of the lava. Perret is inclined to believe that the degree of openness of the channel is also important.

The most obvious surface-manifestation of an eruption is the pillar of cloud that often accompanies it. When these clouds are forcibly ejected they are an awe-inspiring sight and assume many different shapes that appeal to the imagination. Many volcanic cones such as Mt. Martin in Alaska, Santa Maria in Guatemala, Merapi in Middle Java, and Vesuvius in Italy, even when not in eruption are almost continuously capped by a cloud derived from the condensation of the emitted steam. The many changes that are wrought in the appearance of these clouds depend on the temperature and velocity of the emitted steam and other gases, the number of points of nucleation that are present, and on the meteorological conditions of the atmosphere. Enormous volumes of

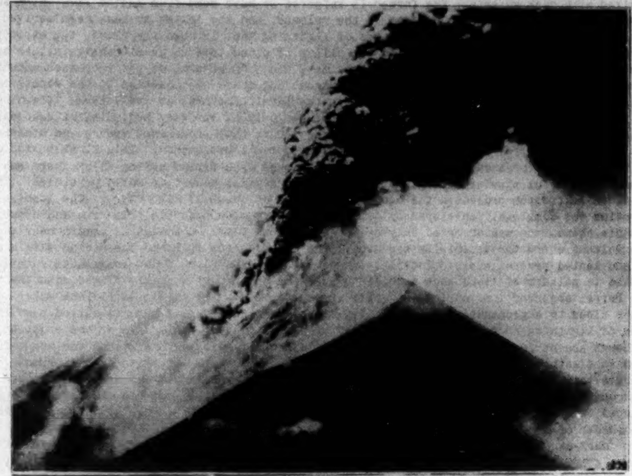

FIG. 3—PILLAR OF CLOUD ON FUEGO IN GUATEMALA
(ERUPTION OF 1932)

PHOTOGRAPH BY HUPKE—
COURTESY BIENER STUDIO, GUATEMALA CITY

gases are constantly emitted during intervals of repose from Santa Maria at temperatures ranging from 100° to 450°. When the moisture-content of the atmosphere is low, as it is in the dry season, there will be periods of several hours each day during which no cloud is apparent when the volcano is viewed from a distance of a few miles.

The steam-cloud is also a conspicuous feature of many areas not immediately related to volcanic activity. In these places, steam is issuing from cracks and holes in the ground and gives rise to what are called fumaroles. Boiling springs, geysers, and mud pots are not uncommon in these steamy areas. One of the best examples of such a form of surface-manifestation is the Valley of Ten Thousand Smokes referred to above. Other areas are to be found on the Dieng Plateau in Java, at Zuñil in Guatemala, Ahuachapán and Chinameca in El Salvador, the well-known Lardarello region in Italy, and the great Yellowstone Park area.

Steam is not always an obvious feature even during an eruption. Izalco in El Salvador, for instance, exhibits for a period of several years a type of eruption coming at more or less regular intervals of half an hour apart during which mostly dust and fragmental material are ejected. Any steam that may be present is quite promptly dissipated in the atmosphere.

The relatively quiet periods preceding or following an eruption are only now beginning to receive the attention they merit. It is at just such times that the activity is amenable to study and, as has been shown by the Katmai studies carried out by the Geophysical Laboratory, can furnish much information concerning the chemical activity of the emanations.

Many of the older accounts of volcanic eruptions are filled with vivid descriptions of the pillar of cloud (Fig. 3) which accompanies them; they make entertaining reading but provide very little information of scientific value. The work of Lacroix [13] at Pelée and the work of Perret [14] at Vesuvius in 1906 are, on the other hand, beautiful examples of what competent observers can do when they have the opportunity to study this surface-manifestation. Perret has not only given us a clear account of these clouds but has also made a serious effort to correlate their behavior with the lava from which they were born. The paroxysmal eruption of Vesuvius in 1906 was characterized by three periods of repeated extrusions of pillars of cloud. The clouds emitted during the first period ascended to great heights and contained glowing fused particles intermixed with dark material; the former is inferred to have come from molten lava which actually also issued as a flow on the surface of the volcano, and the latter to have been derived from detrital material of the old cone. Flashes of lightning shot through the cloud, the explosive force of the gases increased and carried the pillar of cloud to even greater heights than before. As the eruption progressed, the cloud became almost black on account of the increased amount of old fragmental material it brought up from the crumbling cone. In addition to the electrical display, Perret also noted and photographed for the first time what he aptly terms "flashing arcs" [14]. These luminous circles of light appeared only at the very beginning of each explosion and were repeated at intervals of about two seconds; they progressed upward and disappeared. Perret gives reasons for believing that they are an optical phenomenon. This first period of activity continued for about three days and was followed by a period during which there was continuous and powerful expulsion of gases carrying only a small amount of solid particles. The force of the extrusion projected the cloud to a height of about 13 kilometers. The lower part of Vesuvius was completely enveloped by dust and ordinary clouds during this time and hence this remarkable phenomenon was observed only by those who were fearless enough to remain near the top of the volcano where the visibility was good. The third period followed the second within a few hours and lasted several weeks. Intermittent paroxysmal expulsion of hot fragmental materials gave rise to pillars of cloud that were often so dense that they literally flowed down the mountain. Perret observed that during the early stages of this third period sufficient water condensed from the cloud to agglomerate the finer material and form the rounded pellets called pisolites. Flashes of lightning were shooting through the clouds during the first days of the eruption, but even though absent in the later stage, nevertheless the clouds that flowed down the mountain carried an electrical charge opposite to that of the Earth. This was shown by the brush-discharges that developed from the metallic buttons worn by attendants who were bold enough to walk through the cloud. The amount of fragmental material ejected into the atmosphere and down the slopes of the volcano was enormous, but irregularity of terrain makes it difficult to obtain even an order of magnitude. Perret attributed the intermittent and paroxysmal character of this last or "dark-ash phase" of the eruption to the fact that the lava was deep within the hearth, which was gradually becoming choked up, and that explosions occurred only when sufficient pressure had accumulated to initiate them.

The clouds of hot fragmental material that flowed down the slopes of Vesuvius remind one of the type of cloud observed by Lacroix at Pelée on the Island of Martinique (see Fig. 4). Here the amount of fragmental material in the cloud reached such proportions that in spite of a strong explosive force, gravity promptly asserted itself and the glowing cloud called nuée ardente by Lacroix moved with appalling swiftness down the slope of the mountain and within a few minutes engulfed the town of St. Piérre. These glowing clouds are given a directive force because they so often issue from the base of a dome or plug of more or less solidified lava. This plugging of the exit-channel permits accumulation of great pressures within the hearth. The lavas extruded by glowing clouds are as a rule viscous and of the andesitic or rhyolitic type. This combination of circumstances does not permit a sheet of lava to flow out, but instead forcibly ejects a high concentration of hot fragmental materials literally cushioned with gas. This phenomenon, first described by Lacroix, has since been observed at other volcanoes, especially at La Soufrière on St. Vincent Island, Santa Maria in Guatemala, Merapi in Middle Java, and Colima in Mexico. The great pumiceous flow in the Valley of Ten Thousand Smokes is in all probability the result of a similar form of activity. The remarkably clear description [15] of the eruption of Jorullo in Mexico leaves no doubt in one's mind that this eruption, too, was attended by the emission of such directive glowing clouds.

The finer material contained within the pillar of cloud is carried off by the winds, often to the distance of hundreds of miles. When the force of the explosion is sufficient to project the finer material into the upper atmosphere, it may be held in suspension for several years and carried around the world. This world-wide distribution of the fine particles in the upper atmosphere gives rise to unusually beautiful sunsets and also causes a diminution in the solar energy that reaches the Earth [16]. Well-known examples of this type of surface-manifestation are the eruption of Krakatau in 1883 and Katmai in 1912. The outstanding characteristic, however, of the flows of clastic material, both of the directive type such as the glowing clouds of Pelée as well as of those that flow over the rim of the crater, is the complete lack of sorting; huge boulders and fine particles are intermingled in a haphazard fashion. The flow of old, hot fragmental material from Fuego [17] in 1932 is an example of the latter type (see Fig. 5). It is also worth mentioning that in the highlands of Guatemala, to the east of the volcanic chain, are large deposits of unsorted angular fragmental material (usually pumice) where the relationship to a volcanic center is not at all obvious.

The coarse particles, however, in the pillar of cloud are promptly precipitated and obvious sorting with respect to particle-size and weight takes place. Boulders several feet in diameter

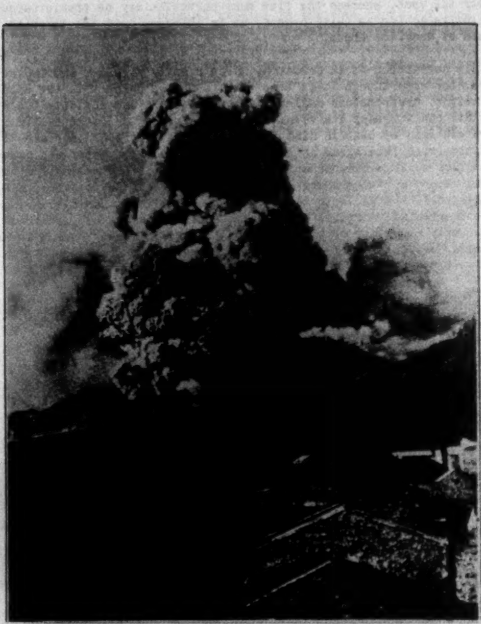

FIG. 4—GLOWING CLOUD (NUÉE ARDENTE) FLOWING DOWN SLOPE OF MT. PELÉE ON MARTINIQUE

PHOTOGRAPH BY FRANK A. PERRET—
CARNEGIE INSTITUTION OF WASHINGTON,
PUBLICATION 458

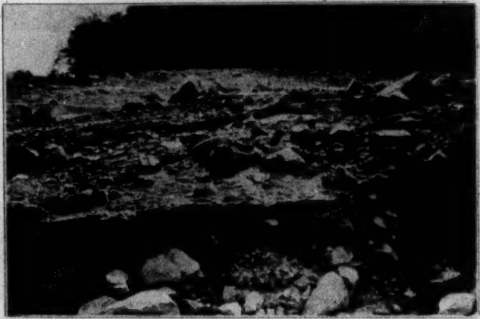

FIG. 5—FLOW OF UNSORTED OLD FRAGMENTAL MATERIAL EJECTED DURING ERUPTION OF FUEGO IN 1932

PHOTOGRAPH BY E. G. ZIES

will be found near the vent, whereas the fine dust-particles may be precipitated many miles away. Intermittent explosive action is quite common, as is inferred from the layering of coarse particles on the slopes of many volcanoes.

Scrope, in his definition of an eruption, quite rightly stressed the importance of the gas-content of a lava, for even though it is true that it may be present to the extent of only about one per cent by weight, nevertheless during the period of extreme activity the gas that was in solution in the lava can increase its volume a thousandfold. The potentiality of this increase is readily understood when we bear in mind that liquid water at 100° increases its volume 1600 times when it is converted into steam at 100°. The great mass of material, however, that is brought to the surface during volcanic activity is composed of the silicate minerals. They form the more permanent record of the surface-manifestation of volcanic activity (see Fig. 6). There are 92 elements listed in the periodic table of chemical elements and all may be present in the extruded lava and fragmental material, but the statistical studies of Clarke and Washington [18] have shown us that only about ten of the oxides of the common elements such as silica, iron, alumina, the alkalis, etc., form the bulk of all igneous rocks. The possible changes with respect to mineral species is very great even if we consider only the ten oxides; therefore, for the purpose of this paper attention will be directed to only a few of the dominant types of volcanic lavas.

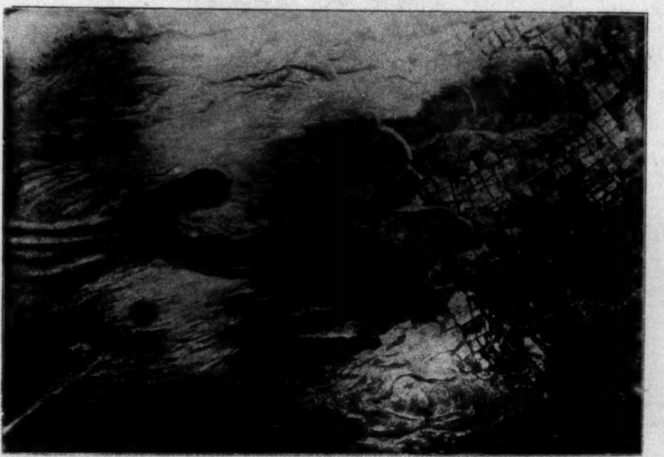

FIG. 6—AEROVIEW OF SERIES OF LAVA-FLOWS ON GOENTOER VOLCANO IN JAVA (LAVA FORMS THE PERMANENT RECORD OF SURFACE-MANIFESTATION OF VOLCANIC ACTIVITY)

PHOTOGRAPH FROM N. J. M. TAVERNE [23]

While it is true that the dominant type of volcanic activity at the present time is explosive in nature, nevertheless one of the best-known volcanoes does not belong in this category, namely, Mauna Loa, together with Kilauea. Explosions certainly have occurred and have brought fragmental material to the surface, but the usual surface-manifestation is the relatively quiet effusion of a fluid lava carrying mineral species developed before extrusion. They are characterized by a temperature of about 1100° C and a high iron-content, especially ferrous iron, which together with the high temperature is believed to be the cause of the high degree of mobility possessed by these basaltic lavas. The fluidity is so great that the gases dissolved in the lava can escape fairly easily, and therefore the eruptions do not as a rule exhibit the paroxysmal explosions so characteristic of the more siliceous lavas such as andesite and rhyolite. The high degree of fluidity enables the lava to flow great distances, and, if the available supply of material is large, these flows will cover the large areas exemplified by the ancient plateau basalts of the Columbia River and the Deccan, and the more recent flow at Laki on Iceland. We have seen above that no cone is developed during the extrusion of such flows. If the flows are intermittent and the feeder-channel remains in the same place for long periods of time, as is the case at Mauna Loa, then sheet after sheet of basaltic material is piled up around the orifice and gives rise to what is called a shield-volcano. Such volcanoes have gentle slopes varying from 2° to 10°, yet if the effusion of the lava is intermittent and varies in volume and speed of consolidation, it can build up enormous edifices of which Mauna Loa is an example. The gentle slope and smooth contour of these volcanoes is characteristic only when they are viewed from the distance, because erosion often sculptures the mass to a considerable extent. It should also be kept in mind that during extrusion of the lava-flow, the escape of volatiles, the

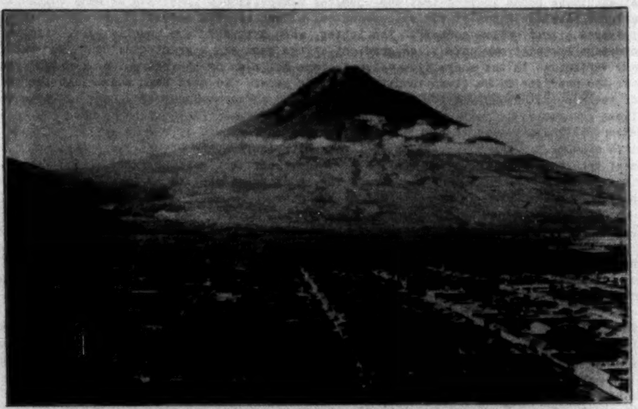

FIG. 7—AGUA VOLCANO IN GUATEMALA
(FINE EXAMPLE OF STEEP AND SYMMETRICAL CONE)
PHOTOGRAPH BY J. W. GREIG

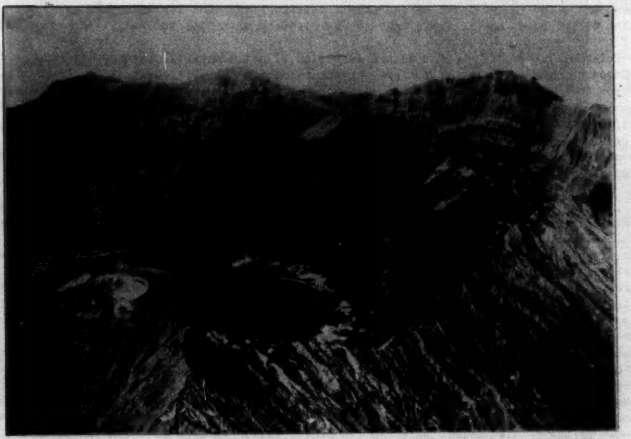

FIG. 8—RAOENG VOLCANO IN JAVA
(WELL-CONSOLIDATED MATERIAL OF CRATER-WALL PERMITS MAINTENANCE OF STEEP WALLS)
PHOTOGRAPH FROM N. J. M. TAVERNE [23]

accumulation of debris as the lava-front advances, and the tendency of this lava-wall to turn over, often give the flow an extremely rugged appearance when viewed close by. The slopes of the basaltic volcanoes could maintain a very low gradient if the lava would always come from the central orifice, but flank eruptions often alternate with eruptions from the crater and therefore pile up material below the upper part of the cone and alter what might be called the normal gradient. Then, too, the chemical composition of the lavas extruded by Mauna Loa and Kilauea is remarkably uniform.

It should be borne in mind that the chemical composition of the lava at many volcanoes is not uniform; basaltic flows of low silica-content alternate with andesitic and rhyolitic lava-flows of much higher silica-content. The latter, when extruded, are usually quite viscous and the cone that is built may have a low gradient at the base and a gradient of 32° near the top. The many variations in the shape of a volcanic cone built up by the extrusion of mixed types of lava are too numerous to be given in detail in this paper. We have seen above that the extrusion of the more siliceous materials is often attended by paroxysmal explosions during which no lava-flows are extruded, but instead either old or new fragmental material or both are brought to the surface and deposited on the cone at just about the angle of repose, which for such material is about 32°. The volcanoes of Guatemala with their steep symmetrical cones are fine examples of this type (see Fig. 7). Erosion often develops deep gulches in the loosely consolidated material, but there is also a tendency for later fragmental flows to fill these gulches and thus once more smooth out the contours of the cone.

Many of the inner walls of the funnel-shaped craters in the active steep volcanic cones of the andesitic type have steep gradients, 35° or more. The walls are built of well-consolidated lavas, and while considerable evisceration may take place during an explosive eruption, yet the strength of the well-consolidated material makes the steepness of the inner wall possible. Fuego in Guatemala and Raoeng in Java are excellent examples (see Fig. 8).

The steepness of the outer slope of the andesitic cones is maintained at many volcanoes by repeated dome-formation in the crater. The term repeated is used because in the typical example, Merapi in Middle Java, volcanic domes have been repeatedly built and destroyed over intervals of roughly 50 years. These domes are made up of hot consolidated but fairly plastic lava and are slowly extruded to heights of 1000 feet or more and for a short time maintain almost vertical cliffs. Huge blocks of angular material are broken off during the extrusion and distribute themselves around the base of the dome at their angle of repose, which at Merapi is about 35°. These domes may attain a height of 800 feet or more within a period of three years; such was the case certainly at Santa Maria (see Fig. 9). They are not to be confused with what are called spines, which are, as at Santa Maria, only a detail of the dome itself. They are formed when a portion of the lava, more plastic than the surrounding material, suddenly finds a path of egress.

Before closing this section on the extrusion of solid materials, the writer wishes to direct attention to the mixed type of activity at the volcano of Batoer [19] on Bali. Here a paroxysmal eruption at some remote period developed a huge elliptical crater whose axes are approximately 14 and 10 kilometers. Such immense craters are called calderas. This eruption at Batoer was accompanied by the extrusion of rhyolitic pumice with which is intermingled some rhyolitic glass. The present activity is confined within the caldera and is characterized by the repeated extrusion of fairly mobile basaltic lavas which are gradually building up a cone whose slope has the low gradient characteristic of basaltic volcanoes (see Fig. 10).

It is obvious from what we have discussed in this review that the solid material extruded during volcanic activity may be either a fluid magma, more or less mobile, or may be fragmental material. Much of the latter is exploded into the air and, together with gaseous emanations, forms the pillar of cloud. The fragments may be either congealed fresh lava or pumice derived by the extremely rapid escape of the gases from the liquid siliceous lava which thereupon promptly freezes. The fragments may also be nothing but old material derived from the wall and throat of the crater, as was observed at Fuego. The water-content of these materials plays an important part in bringing about the extrusion; nevertheless, it is low when reckoned on a weight-basis. There is another type of activity, however, in which the surface-manifestation is characterized by the transport of materials with high water-content, namely, the mud flows. A word of caution is here not amiss. For instance, observers have occasionally been deceived by the extraordinarily rapid motion of hot clastic flows and have called them mud flows. The type of mud flow to which attention will now be directed is characteristic of the volcanic activity at the Kloet in Java (see Fig. 11). Considerable fumarolic activity is present in this volcano, as shown by the roiling of the water in its crater lake. In 1919 an explosion of frightful intensity carried over the rim of the crater a large portion of the water which flowed down the channels on the outer slope. The resulting mud flow rapidly engulfed a large area and killed over 5000 people. The Dutch authorities have now pierced the volcanic edifice with a tunnel and maintain the lake quite low so that the subsequent and expected eruption will carry only a relatively small amount of water over the top of the crater. The amount of water that can be drained off is limited by physical difficulties peculiar to the volcano. This is probably the first deliberate attempt that has been made towards mitigating the effects of volcanic activity. In this connection, mention should also be made of the success of Jaggar's efforts in diverting and breaking up the lava-flow from Mauna Loa [20]. Mud flows are not uncommon attendants upon volcanic activity. Their source is not always as obvious as in the case of the Kloet. At times enough water is condensed from the pillar of cloud (Vesuvius) to yield a mud flow. The hot materials also may come in contact with snow and ice (Katmai [3] and Cotopaxi [21]), and by their commingling produce mud flows. Then, too, such flows can arise when clastic flows come in contact with a stream, and also when the force of an explosion has opened fissures in the lower part of a volcanic cone and thereby permitted egress of ground-water.

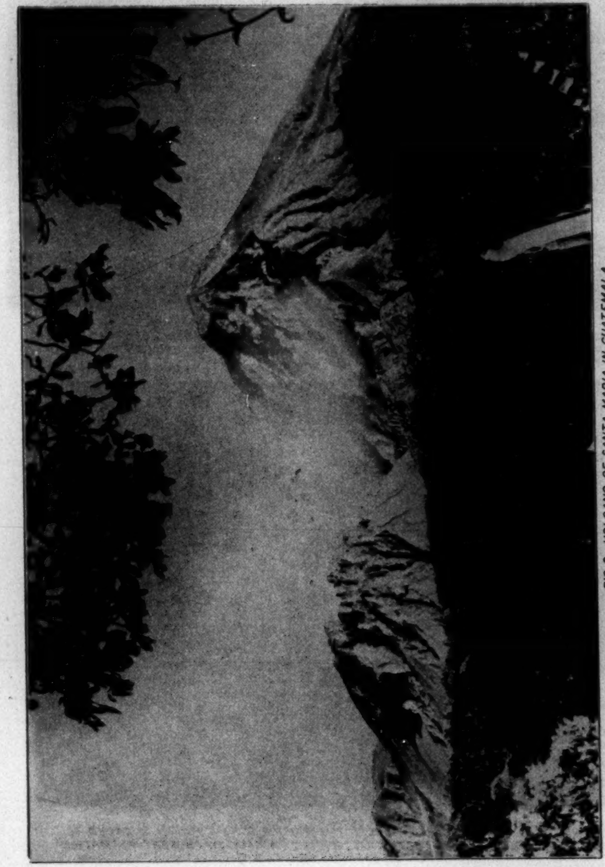

FIG. 9—VOLCANO OF SANTA MARIA IN GUATEMALA
(ACTIVE DOME TO LEFT WAS BUILT UP WITHIN A PERIOD OF THREE YEARS, 1922-25; STEEP WALL OF OLD VOLCANO, TO RIGHT, WAS FORMED WHEN EXPLOSION OF 1902 BLEW OUT A CRATER IN ANCIENT VOLCANIC EDIFICE)
PHOTOGRAPH BY J.W. GREIG

The bottoms of many volcanic craters are tight enough to hold water and permit the formation of a crater lake such as in the case of the Kloet. These lakes are, of course, not an immediate result of volcanic activity, but their presence in many volcanoes that are in the solfatara stage of activity does, as we have just seen, modify the usual course of an eruption during which relatively speaking dry materials are extruded. Furthermore, the crater-lakes of the Kloet and Kawah Idjen in Java, and Santa Ana in El Salvador are typical examples of still another

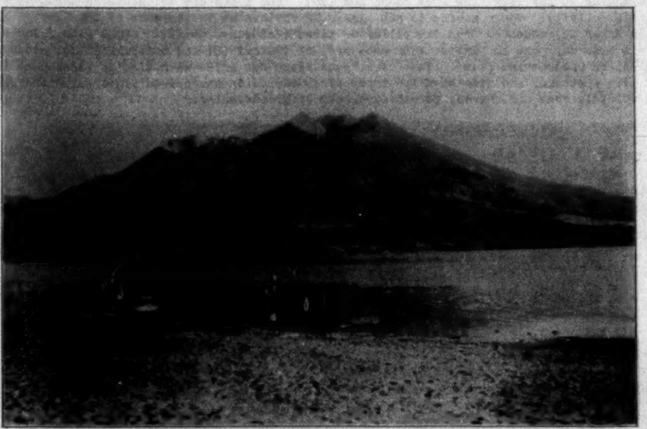

FIG. 10—BATOER VOLCANO IN BALI
(FLOWS OF BASALTIC LAVA ARE GRADUALLY BUILDING A CONE WHOSE SLOPE HAS GENTLE GRADIENT CHARACTERISTIC OF SHIELD-VOLCANOES; BASALTIC MATERIALS FLOW EASILY AND THEIR CHEMICAL COMPOSITION IS FAIRLY UNIFORM) PHOTOGRAPH BY E. G. ZIES

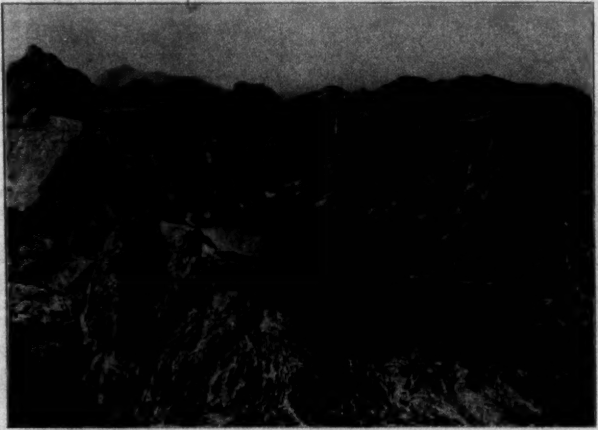

FIG. 11—CRATER AND LAKE OF THE KLOET IN JAVA
(THE DUTCH GOVERNMENT HAS PIERCED THE EDIFICE WITH A TUNNEL IN ORDER TO KEEP DOWN VOLUME OF WATER IN LAKE, HOPING THEREBY TO PREVENT FORMATION OF DISASTROUS MUD FLOWS)
PHOTOGRAPH FROM N. J. M. TAVERNE [23]

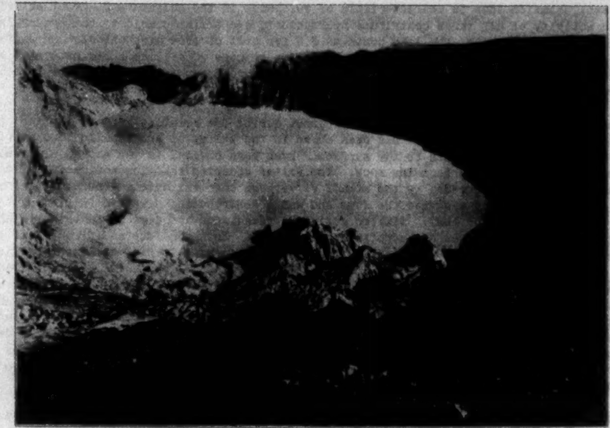

FIG. 12—CRATER LAKE OF KAWAH IDJEN IN JAVA
(COLOR OF THE WATER IN THIS BEAUTIFUL LAKE IS A BLUISH GREEN; ABOUT 3.5 PER CENT OF FREE ACID (HYDROCHLORIC AND SULFURIC ACIDS) IS PRESENT IN WATER)
PHOTOGRAPH FROM N. J. M. TAVERNE [23]

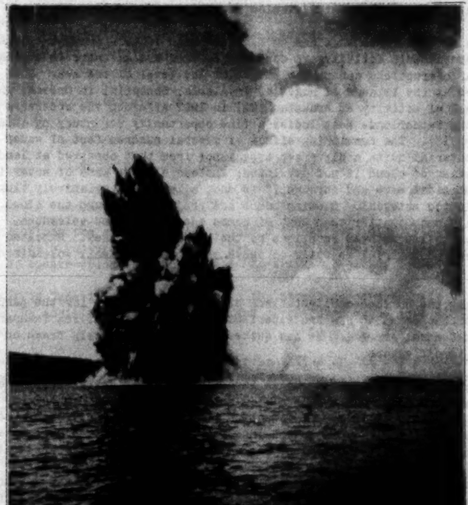

FIG. 13—SUBMARINE EXPLOSION, 1928, AT KRAKATAU, NEAR JAVA
(BLUNT PILLAR OF CLOUD IS CHARACTERISTIC OF SUBMARINE ACTIVITY)
PHOTOGRAPH BY CH. STEHN

surface-manifestation that is characteristic of volcanoes in the solfatara stage of activity. Fumarolic activity at all three contribute hydrochloric and sulfuric acid to the crater-lake. At Kawah Idjen, for example, the lake contains 3.5 per cent of free hydrochloric and sulfuric acids (see Fig. 12). The total amount of these acids in some 20 million cubic meters of water is obviously enormous. It is not surprising, therefore, to learn that these acid waters have produced extensive alteration in the wall of the crater.

Chemical activity is present in many volcanoes during the relatively quiet periods between eruptions. Steam and gases can, as has been shown in the Valley of Ten Thousand Smokes [11], bring many volatile constituents to the surface. Even more obvious is the action of aqueous solutions of acids on the surrounding rock. The writer observed that the entire top of the Merapi dome was in 1928 corroded by the attack of various chemical agents and believes that this fumarolic activity greatly weakened the dome and was a contributing cause in initiating the violently explosive period of 1930. Similar activity prevails on the dome of Santa Maria and is here augmented by the presence of hydrofluoric acid [17]. Both the face and fissures of the crater-wall may be exposed for long periods to the steam and acid fumes. At Kawah Kamodjang [17] in Java this activity has produced considerable alteration of the rocks and made possible the formation of the mineral alunite which fills the vesicles in an andesitic rock. The slumping of a portion of the crater-wall has revealed thousands of tons of rock in which this mineral is present. At the basaltic volcano of Batoer on Bali the acid gas is mostly hydrogen sulfide which oxidizes to sulfuric acid. This, when dissolved in water, permeates the lava and develops in the caves, peculiar to one of the craters, stalactites and stalagmites of the sulfates of magnesium, potassium, manganese, and iron; titanium as rutile, and copper and vanadium in some unrecognizable combination are also present [17].

A steam explosion in a cultivated field on the Dieng Plateau in Java revealed the fact that even though no activity is obvious on the surface, yet the ground may be permeated with steam, carbon dioxide, and hydrogen sulfide. Analyses were made of the gases that issued from a fissure developed by the explosion in a hill close by, and about ten per cent of carbon dioxide together with hydrogen sulfide was found. The reader may judge for himself as to the potentiality of such a concentration of these chemicals below the surface.

It might be inferred from what has so far been said in this presentation that surface-manifestations of volcanic activity are confined to land-areas; there is abundant evidence, however, that volcanic eruptions can take place below the level of the sea. Such submarine activity has been observed at the islands of Bogoslof in Alaska, Santorini in Greece, and Krakatau near Java. The renewal of activity at Krakatau [22] in 1927 afforded the observers of the Vulcanological Survey in the Netherlands East Indies a fine opportunity for studying this subaqueous phenomenon (see Fig. 13). The cushioning effect of several hundred feet of water gives the violently ejected material quite a different appearance from that observed at land-volcanoes. The tall, slender pillar of cloud is not developed; instead a large mass of water is elevated as a kind of incipient tidal wave and through it is shot a large but relatively blunt column of steam and finely comminuted material. Glowing bombs are projected through the cloud, and their trajectory is usually much flatter than that of bombs ejected by land-volcanoes. The surface of the sea is constantly roiled between eruptions by the emission of gases. Repeated eruptions have now brought up enough material to build up above sea-level the small volcanic cone called Anak Krakatau.

In portraying the surface-manifestations of volcanic activity for the purposes of this symposium the writer has drawn on his own experiences in the Valley of Ten Thousand Smokes, in Java and Bali, and in Central America. He has quite obviously also freely drawn on the published accounts of other investigators.

The study of surface-manifestations is important, but the writer wishes to emphasize the fact that such a study alone can hardly give a satisfactory explanation of the causes of volcanic activity. We must obtain more information concerning the behavior of the lava within the hearth of the volcano. The hope is entertained that the study of the surface-manifestations, the study of ancient dissected volcanoes, the application of modern geophysical methods, and laboratory-studies may all contribute in helping to obtain the much-sought-for logical generalizations as to the cause of volcanism. Finally, it is well always to keep in mind what seems to be an obvious fact: That an eruption is but the culmination of a long sequence of events that took place within the hearth of the volcano.

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STRUCTURAL DEVELOPMENT OF VOLCANIC CONES

T. A. Jaggar

A geologist describes the structure of volcanic heapings for interpretation of history. A geophysicist, describing structural development, has in view the basis of experience for forecasting evolution of construction that is to come, in a living volcanic edifice.

A volcanologist in Hawaii observes changes in the structure of a volcano by direct experience. He watches the addition of gushes of gas and of extruded magma from within the so-called crust of the Earth.

We may believe as Dana did that Hawaii is representative of the unity of volcanic process, in construction by external layering, and also by internal intrusion. Both processes construct a change in making the local topography of the Earth higher in a place of added matter, but lower elsewhere by subtraction.

The quarrel between Von Buch and his opponents on the subject of "craters of elevation" has been decided in Japan and Hawaii in this century, by measurement through levelling and tiltmeters, in favor of Von Buch's doctrine that volcanoes swell and shrink from within. A volcano-observatory is looking inward at the lithosphere and its gas at an excessively mobile place.

Lithosphere and atmosphere

My point of view is experimental and may be illustrated by the current intense interest in, and use of, the gas called atmosphere. That interest is wholly based on its action, rather than its history. My interest in the Earth's surface is based on its action, as exhibiting latent powers and products useful for mankind.

The present study of "free air" is experimental for purposes that would have seemed absurd in 1880. Clusters of balloons send telegrams from 15,000 meters up; men sacrifice themselves for the stratosphere; tens of thousands of figures are accumulated and printed; the electric properties are all-important; air-vehicles are using all the data; and physics is mostly concerned with gases.

All of this started with some advertisements of British prizes, posted in the universities about the nineties, in the days of Ramsay, Rayleigh, and Thomson, asking for studies on the constitution of the atmosphere.

The result is a reconstructed world. All human commerce is using flying, stream-lining, compressed air, insulation, air-conditioning, nitrates, predicted weather, and "listening on the air," because of a science of the common substance air. The studies are made all over the globe for weather, agriculture, radio, stellar and solar radiation, circulation of the oceans, navigation, ship-design, and the intercourse of peoples in a hundred different contacts.

Fast vehicles and the threat of air-warfare have started distributing people away from congested centers. Mining machinery is carried on wings. Bird men draw together the nations. The amount of expenditure of human energy that has gone into the measurement of the resources of the air was never dreamed of in 1895.

Even since 1895 I have been convinced that distributed study of the powers of the lithosphere would accomplish still greater results for mankind. The American Geophysical Union, ever extending its branches, is named after Earth physics. The powers and processes of land, ice, and sea-bottom, as affected by the gigantic hot chemical furnace of the interior of the globe, are not yet being experimented upon after the same fashion as water, life, and air.

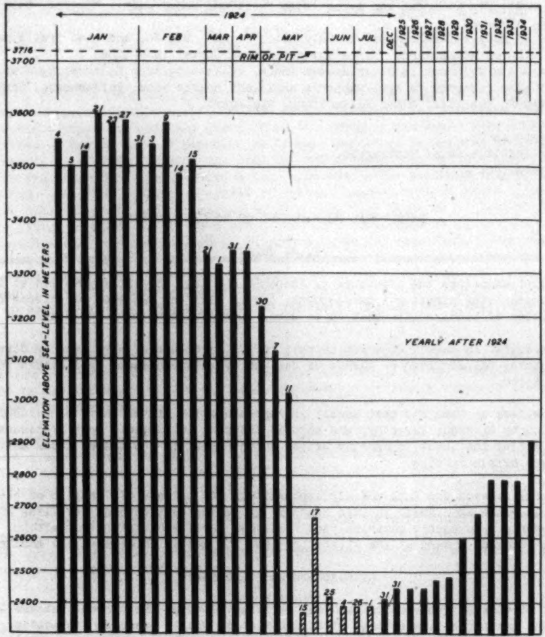

FIG. 1—DIAGRAM OF ACTUAL STANDS OF THE BOTTOM-LAVA IN HALEMAUMAU PIT 1924, AND YEARLY MAXIMA, 1925-34

Water, life, and air have proved more amenable because we are gregarious and science likes congestion and collection. So these interests have been brought to the service of us travelling animals and our food-supply, both made up of a chemistry of carbon, hydrogen, oxygen, and nitrogen.

It so happens that these, with sulphur added, are the constituents of volcanic gas and of protein, respectively. Probably protein-chemistry actuates laboratories just as much as the atmosphere. It requires no effort of the imagination to recognize the obvious inference, that these five intratelluric gaseous elements originally produced air and water and life, are probably producing all three now, and at volcanoes are the transporting agents of new raw material to the Earth's surface.

A quarter-century of Hawaiian volcanoes

With this excuse for experimenting with the exuding gases and radiations from within the Earth's crust, let us examine diagrammatically the rise and fall of basaltic foam expelled by gas in Hawaii (Fig. 1). These diagrams visualize measurement of Kilauea and Mauna Loa, 22 miles apart in Hawaii, with central craters respectively 3,700 feet and 13,700 feet above sea-level.

The rift crack athwart the craters and far down the mountain flank is just as active a vent in history as the pit on top. The shield-domes are built elongate to the rifts. I believe the active rift is a buried or open feature of every world volcano. Failure to recognize it may cause misunderstanding of the deeper facts of volcanic systems. The sink at the summit is an expression of weakness and down-faulting at a bend of the rift in plan. It is as though each center of radial construction of the edifice becomes controlled by deep cracking, of a larger tectonic significance.

The larger tectonic significance goes with the breakdown into calderas, in Post-Tertiary time, of most of the big volcanoes on Earth. These calderas take many different forms, such as Monte Somma at Vesuvius and the outer walls of Aniakchak, or Aso or Batoer in Bali.

The Post-Tertiary decline of volcanism is just as definite a world-wide condition as the post-glacial change in shore-lines that followed the retention of water in the great ice-sheets. Stübel's statement that the underlying dome of many of the steep South American cones was a shield-volcano of basaltic habit, may express another world-wide feature. This would account for a hotter liquid-lava Mauna Loa phase in the Tertiary, for many of the peaked heapings like Fujiyama, that in more recent time became complicated with development of viscous intrusive andesites. This process led to retrogressive engulfments, and heating of inflowing ground-water that made steam-blast upbuildings of cinder-cones.

The sink-craters of Kilauea and Mauna Loa are such calderas with active lava-pits inside. The decade 1914-24 is a single cycle of 11.1 years with lava rising in Kilauea to 1919, spurting and lowering with each eruption of Mauna Loa, then sinking and recovering with a series of flank outflows of Kilauea, all within the decade. The outflows were from the rift-belt on both sides of the Kilauea summit-crater. They ended with a voluminous rising in 1923, then a tremendous lowering of 1924 and cracking open of the eastern rift-belt to the seashore and beyond. At the crater steam-blast explosion accompanied the engulfment. The volume of matter ejected was one two-hundred-and-fiftieth part of the volumetric enlargement of the lava-pit. The conclusion was justified that lava escaped seaward, engulfment was the principal crater-maker, and ground-water entered the sub-crateral void.

The great sea of lava that had filled the pit by millions of cubic meters during the year 1923 had receded in such enormous volumes that only submarine outflow will geometrically account for it. The presence of the broken rift at sea-level, the submarine mountain slope extending to 3,000 fathoms in 40 miles; and at the crater the invasion of ground-water, evidenced by the blasts of pure steam that accompanied the engulfment of wall-rock during the explosive eruption, all justified the submarine-outflow theory. Note that submarine lava-flow was never suspected at explosions of Vesuvius, Pelée, or Krakatoa.

No explosive eruption of Kilauea crater prior to this one had occurred since about 1790. Increasing volumes of lava were pumped out of the Hawaiian volcanic system from 1790 to 1855, and decreasing volumes, accompanied by notable crater collapses, gave occupation to the system during the 67 years from 1856 to 1924 (Fig. 2).

In other words, the nineteenth century and the beginning of the twentieth showed the Hawaiian volcanoes executing a greater culmination achieved in 1855, in a series of 11-year peaks of lava-rising, each similar to the decade we have studied in detail.

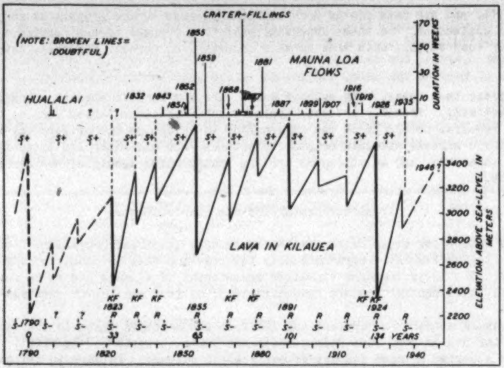

FIG. 2—DIAGRAM OF MAXIMUM AND MINIMUM STANDS OF LIVE BOTTOM-LAVA IN HALEMAUMAU PIT YEARS OF CRITICAL BREAKDOWN AND OF DURATION IN WEEKS OF MAUNA LOA AND HUALALAI OUTFLOWS AND CRATERAL INFLOWS (DATED ONES ARE OUTFLOWS)
[S+=SUNSPOT-MAXIMA, S-=SUNSPOT MINIMA, KF=KILAUEA FLANK-OUTFLOW, R=KILAUEA REPOSE]

Like our 1913-24 decade, so our 134-year greater cycle was punctuated during its receding last half by a series of cataclysmal sudden lowerings for Kilauea in 1855, 1891, and 1924. While the 1919 decade was exceptional in bringing to an end the supercycle by explosion, its other features of gradual rising, spasmodic lowering, and Mauna Loa coincidences, are indicative of an 11-year habit worth examining for rhythm.

Two special charts of 1914-16 showed Kilauea risings progressively greater approximately nine months apart. The first and third are accompanied by Mauna Loa outbreaks, the second by a Mauna Loa swarm of earthquakes. The two Mauna Loa eruptions culminate on solstice, the seismic swarm on equinox.

The seismic frequency-chart for this period shows similar peaks. Each began with epicenters at Mauna Loa distances from the seismographs, and ended with earthquake-sources at Kilauea distances.

For intensity, the time of lower lava-level and lower earthquake-frequency in 1914 coincided with occurrence of strongest shocks, as though these were measurably controlled by lava-confinement.

It is the symmetry of such measurements as these that lead to the suspicion that we are dealing with tidal release of a uniform force. In day-to-day record, the results by E. W. Brown on Jaggar's data for the lava-tide measurement of the summer of 1919 suggest a luni-solar release. Perret and Wood have worked on seismo-volcanic curves in the semiannual group, and on astronomic controls, and have found strong correspondences for equinox and solstice. Dr. Douglass has examined our curves in comparison with his "tree-growth and climatic cycles," and writes me that he strongly suspects the presence of the half sunspot-cycle. This makes it of interest to turn to the curve of Kilauea and Mauna Loa for the nineteenth century.

Kilauea and Mauna Loa in the nineteenth century

The only way known to me for interpreting what numerous authors say, without reliable quantitative measurements, for Hawaiian activity after 1790, is to group the known facts and tabulate them, interspersing in the tabulation such measurements as have been reported. We get the following:

1788-93--Kilauea flows before and after 1790 explosion; crater-lava elevation before and after 1790 collapse; contour of pit-lava elevation 3,500 down to 2,200 feet, estimated.
1794-99--Vancouver report; all probably quiet.

1800-05--Two lava-flows from Hualalai, and Molokini west of Haleakala reported smoking; Kilauea drop 1800, estimated 2800 down to 2500 feet.

1806-15--Unknown decade; Kilauea drop 1810, estimated 3000 down to 2800 feet.

1816-23--Tremendous building of Kilauea floor; unusual large eruption Kilauea with outflow to the sea southwest 1823 explosive cracks southwest; Kilauea drop 1823 (Hitchcock), 3180 down to 2457 feet.

1824--Probable quiet year.

1825-32--Kilauea and Mauna Loa lava-floods culminating 1832; Kilauea drop 1832 (Hitchcock), 3365 down to 2937 feet.

1833-34--No recorded mention; probably quiet.

1835-43--Building up of Kilauea to a great eastern outflow 1840; tremendous northern Mauna Loa eruption 1843; Kilauea drop 1840 (Hitchcock), 3430 down to 3127 feet.

1843-47--Quiet period.

1848-55--Maximum volume of Kilauea and Mauna Loa lavas in the century; Kilauea drop 1849, 3730 to 3127; Kilauea greater drop 1855 (Weld and Hitchcock), 3730 to 2700 feet.

1856-58--Relapse to quiet.

1859-68--Three eruptions Mauna Loa, including first southern outflow; Kilauea rising and overflowing.

1868-69--Greatest earthquake of history; collapse Kilauea Crater; period of quiet; Kilauea drop 1868 (Hitchcock), 3480 down to 2957 feet.

1870-77--Kilauea rising; continued summit lava-eruptions of Mauna Loa; Mauna Loa outflow Kealakekua Bay 1877.

1878-79--Kilauea crateral activity leading to breakdown; Mauna Loa quiet; Kilauea drop 1879 (Hitchcock), 3620 down to 3320 feet.

1880-87--Kilauea building up crater; Mauna Loa two big outflows.

1887-91--Kilauea rather quiet, Mauna Loa dormant; Kilauea breakdown 1891; Kilauea drop 1891 (Hitchcock), pit-lava elevation 3745 down to 3055 feet.

1892-99--Three eruptions Mauna Loa; Kilauea crateral overflows and collapse 1894; Kilauea drop 1894 (Hitchcock), 3678 down to 3433 feet.

1900-02--Very quiet; Kilauea drop 1902 (Hitchcock), 3430 down to 3240 feet.

1903-12--Two eruptions Mauna Loa; Kilauea recovers to violent effervescence 1910 and 1912; Kilauea drop 1908 (Hitchcock), 3583 down to 3160 feet.

1913--Quiet year; Kilauea drop (Jaggar) 1913, 3350 down to 3075 feet.

1914-19--Three eruptions Mauna Loa; Kilauea overflows crater; Kilauea drop 1919 (Jaggar), 3740 down to 3140 feet.

1920-25--Mauna Loa quiet; Kilauea three outflows and explosive collapse 1924; Kilauea drop 1924 (Jaggar), 3580 down to 2350 feet.

1934--Kilauea range (Jaggar), 3200 down to 2900 feet.

The only data for comparative volumes of lava-eruptions must be based on duration as expressed in days of average flowing. This is only satisfactory for outflows, or for crateral overflows. Mere activity within the pit is made up of so many recessions that it means nothing as to added volume.

The number of flow-days in Hawaii was: 1790-1820, very few; 1820-1830, about 7; 1832-1843, 132; 1851-1855, 411; 1859-1868, 307; 1870-1877, 207; 1880-1887, 247; 1892-1899, 37; 1903-1907, 28; 1914-1919, 71.

It is rather striking that sunspot-maxima show: 1790-1820, low values; 1820-1830, increasing values; 1830-1880, very high values; 1880-1920, declining values.

Let us turn to a plat summarizing this kind of information, based for elevations of lava in Halemaumau pit of Kilauea Volcano on Hitchcock's valuable table (p. 272); and for duration in weeks of Mauna Loa eruptions, expressed as columns, on a chart made by E. G. Wingate, my associate at the Observatory. On the chart the symbol S+ is inserted for the years of maxima of sunspots (after Stetson), and S- for the years of sunspot-minima. The symbol R stands for the year of beginning of a marked Kilauea repose-period and this is identical with a conspicuous measured lowering of lava in the crater. For 1790 and 30 years thereafter an imaginary estimated curve of broken lines has been drawn based on inadequate data. Similarly a broken line has been inserted as a forecast, from 1934 to 1946, based on a resemblance of 1935 to 1843 in location.

It will be observed that the Kilauea curve is based entirely on the critical years of what Dana, Green, and Hitchcock called eruptions. By "eruption" is meant a crateral breakdown. It is apt to be preceded by strong effervescent crateral fountaining and overflow and by Mauna Loa flank outflow. All workers have recognized the probability of an elastic limit occasioning these breakdowns, and to me this means a preceding measurable tumefaction, and a succeeding shrinkage, as definitely proved by tilt and levelling before and after the 1924 collapse of Kilauea. The recognition of a repose-period following collapse as the vital punctuation-mark for cycles of volcanism is well known in Europe, where Mercalli spoke of "sprofondamento," a deepening, as the index of eruption. The interval between repose-periods has cyclical significance. The interval between mere risings, spectacular outflows, or spectacular sinkings with sudden recovery, is generally not significant.

A crateral downbreak at Kilauea was in the conspicuous cases of 1823, 1840, and 1868 accompanied by Kilauea outflow from the rift in the mountain flank (KF on the diagram). The collapse of 1832 went with an outflow in the adjacent deeper Kilauea Iki pit, so that this also may be labelled KF. The year 1877 produced a welling up of lava in the adjacent Keanakakoi Crater, precedent to the 1879 collapse. The years 1920, 1922, and 1923 showed visible flank outflows on the Kilauea rift, and I have stated the reason for the certainty that submarine Kilauea flow undermined the pit in 1924.

All of these facts make it unthinkable that the great downbreaks of 1790, 1855, and 1891-94 had no flank outflow, when we consider that the rift-belts extend out under the ocean to depths of 2000 to 3000 fathoms, for scores of miles radially along the flank of the same edifice, but concealed and masked by great pressures of sea-water.

The emergent facts from the Kilauea curve are a general rising from 1790 to 1855, a high level from 1855 to 1891, ended by decline and collapse to 1924. Mauna Loa (quite unrecorded 1790 to 1832) showed in its crater a building up of the bottom, marked by continuous action at the apex of the curve between 1871 and 1877. Otherwise the duration of its outflows, which were scattered at intervals of from one to 12 years, reached a maximum in 1855 and declined rather steadily thereafter. The year 1855 was critical as combining for both volcanoes the evidence of maximum lava-pressure, and the two curves are strikingly sympathetic, just as they were for the single cycle that we have examined in detail, namely, 1913-24.

If we consider volcanism under the Hawaiian system as magma continuously stressing upward, and released to effervescence of its self-heating fixed gases by the yielding of the edifice, it is apparent that the Hawaiian ridge yielded to magmatic tumescence from 1840 to 1868, with a breakage that made the most voluminous surface-floods of history 1855-59, and at the same time (1855) made an unknown submarine flood of lava from Kilauea.

When these great flows congealed, the tensional opening of the numerous rift-faults spread to the more brittle and long-cooled southwestern extensions of the rift-belts of both Mauna Loa and Kilauea, as evidenced by the terrific earthquakes of 1868 there. This breakage released southern outflows of Mauna Loa for the first time, and initiated alternation of Mauna Loa flows north and south during the remainder of the nineteenth century. Kilauea had been alternating between southwest and northeast flanks, and continued to do so. It was this 1868 breakage and release of pressure that maintained the high levels of the two curves long past the middle of the century. This exhausted the energy of the system by gas-release for that larger unit of weight and structure that controls the supercycles of 134 years; the weight had been enormously added to by the new veneer of lava-extrusion on the whole Hawaiian ridge, and the release of magma declined strikingly from 1881 to 1926.

Then along with a new building up of extremely forceful pumiceous lava-fountains in Kilauea Crater, the year 1929 produced a new earthquake crisis northwest of Mauna Loa, indicating underground shift of lava to the north, and this was confirmed by the northern outflow that threatened Hilo in 1935, which in location and volume was almost a duplicate of the Mauna Loa flow of 1843. The broken line that projects the Kilauea curve to 1946 is based on years around 1843, and if we project a series of Mauna Loa columns into the next 20 years, we find great danger of a repetition of the lava-flows of 1852 and 1855 which terrorized Hilo.

These projected curves are the basis for the engineering project for barriers to defend this, the most important harbor of the Island, from being filled with lava, an event that geologically is unavoidable otherwise, opposite the valley between Mauna Loa and Mauna Kea, the only important drowned valley on Hawaii.

The extreme low points of the Kilauea curve are five in number, namely, 1790, 1823, 1855, 1891, and 1924. These were the greater collapses of Kilauea Crater, and the 1823 event was accompanied by some steam-blast explosion. There is numerical basis of measurement for the depths of these lava-depressions in the region of Halemaumau pit for all except 1790 and the quarter-century following. As 1790 was a bigger explosion and engulfment than 1924, I have tentatively set its limit of crater-bottom at 2200 feet, 150 feet lower than the bottom of 1924. The height of pit-rim before 1790 collapse is set at 3500 feet arbitrarily. The pit-rim of the nineteenth century was about that, and was built up to more than 3700 feet above sea-level during the century. The evidence of the early missionaries and the measurements by officers of the Blonde in 1825 indicate that construction of 800 or 1000 feet of Kilauea Crater floor by inflow of lava must have occurred during the 30 years that followed the 1790 downbreak, in order to make the marginal floor, spoken of as the "black ledge" by the explorers of the nineteenth century, and left by the collapses of 1823 and later.

Emergence of sunspot-correlation

The curve resolves itself, then, into four divisions separated by big depressions; and within the last three measured divisions from 1823 to 1924 each division is broken up by two principal minor depressions or eruptions making intervals of from eight to 15 years. The five great crises are almost exactly 33 years apart; treated as calendar years the intervals come out respectively 33, 32, 36, and 33 years. The mean is 33.5 years, which divided in three makes the mean of the minor cycles 11.16 years.

This checks with the single cycle 1913-24 studied in our first chart, and the seemingly accidental identity of this with the sunspot-cycle makes the suggestion of sunspot-correlation at least worthy of platting. Minima of sunspots (S-) correspond somewhat with Kilauea repose-periods (R); with S- lagging behind R prior to 1855, and R lagging behind S- thereafter until 1924 when they are coincident. The true repose-epoch usually _followed_ R.

Sunspot-maxima, shown by S+ across the upper part of the Kilauea curve, invariably occur on rising lava when both Kilauea and Mauna Loa were engaged in an 11-year effort of recovery. In order to discover the relation of Mauna Loa outflows to the Kilauea minor cycles, vertical dotted lines are drawn to the upper columnar diagram. The dated Mauna Loa eruptions shown are all outflows. The others are Mauna Loa summit-crater gushings. It will be seen that usually, within the Kilauea cycle, Mauna Loa Crater eruptions occur in the first part of the period, and the outflows come toward or at the end, as though the tumescent effort of the cycle finally ruptured the rift-belts in one or more different years, just as in our type cycle of 1913-24. It seems likely that the bend of the rift-belt at the crater is the gash that opens widest, over dike-tumefaction of rising magma occupying the rift-crevasses in the depths.

Can sunspots affect volcanism?

I have only speculative hypothesis to offer as to why the lift of intra-telluric magma should respond to the times of maximum of mean numbers of sunspot-eruptions, and why the greater and lesser withdrawals of magma occur at average intervals respectively of 134, 33.5, and 11.1 years; and the departures from the mean of the 11-year interval correspond rather well in some cases, for eruption and sunspots, as in the long interval immediately preceding 1855.

It may be noted that Professors Nagaoka and Ikebe (Imp. Acad. Tokyo, v. 13, p. 30, 1937) have discovered response to volcanic rumblings at Asama Volcano, in a magnetograph giving the value of the time-rate of variation of magnetic disturbances. They attribute this to changes in magnetic attractiveness for liquid lava underground, on occasions of the crystallizing of iron oxides, or on their liquefying amid glassy silicates.

If the gas-action of underground magma can itself show some response in its solution phases, to the stimulus of solar electromagnetic control, on the crystallizing or melting of billions of crystals, possibly as a pressure-phenomenon, it is conceivable that the well-known magnetic effects of the gigantic solar eruptions may penetrate the highly sensitive physical chemistry of magma.

We know all too little of the supposed pressure-control of gas in solution at high temperatures in a silicate slag. There are no physical tables for viscosity, temperature, solution, and pressure of basaltic melt containing dissolved hydrogen. To me Dr. Nagaoka's experiment came as something of a boomerang.

All of our Hawaiian differentiation of the stirred aa or sprouting lava, and the glassy-topped pahoehoe or skin-lava, centers around the oxidation of the iron to billions upon billions of iron-oxide crystals. The surface of the sprouting lava is stimulated by stirring, to crystallize the ferrous iron to magnetite, and this oxidizes to hematite in the presence of air. The surface of the vitreous lava skins over, when glass floats to the top and the crystallizing melt is confined beneath.

The heating and stirring agents appear to be gases escaping from solution, and their retention in solution is itself controlled by the intricate phenomena of interaction of pressure, bubbles, convection, crystallization, density, motion, and viscosity.

We know by Chevalier's tests that mass-polarity arranges the crystals of basalt in accordance with the Earth's local declination. If solar violence can produce magnetic storms and temporarily change local declination, it is not unthinkable that it may produce magmatic storms, or at least exert a trigger force on microlitic crystals. In 1913 J. W. Green proved that a handful of vitreous basaltic fibers--"Pele's hair"--will deflect a magnetometer-needle. The Sun achieves magnetic fields at very high temperatures in vapors of iron, calcium, and hydrogen. We are ignorant of the localized magnetic susceptibility of these elements, at supposed high temperature, in thermoelectric earth-crust conditions, amid considerable oxygen. But they are all ingredients of magma.

Review of Hawaiian volcanism

Reviewing the structural development of the Hawaiian volcano-system in the light of experiment, we find a quarter-century of unbiased measurement revealing construction of the great slag-heap mountains by both internal tumefaction and extrusion along rifts. The construction is by gas-heating and gushing of silica-iron foam through 11.1 years. This is followed by sharp collapse terminating the period. The collapses of minor phase occur throughout the cycle and correspond to alternate, mutual exchanges between craters large and small. Lava-lowering is just as frequent as lava-rising. Seismic indication goes with both movements. Tests by gas collection have left no doubt that the volatile products are air, hydrogen, carbon, and sulphur and their several oxides. Tests by platting successive surveys at craters have shown sympathy between Mauna Loa and Kilauea, and rhythms that are seasonal, and tidal effects definite. The sympathy is not hydrodynamic, it is hot gas-foaming release. Temperature-measurement has shown liquid lava in a pit hotter from below upward and hotter with increased effervescence of gas. Soundings in lava have shown hotter glassy pools and wells, within cooler but mobile crystalline paste; extrusive basaltic lava is duplex. All facts point to continuity of intrusive pressure of magma initially glassy, triggered by edifice-yielding and tidal controls.

Review of the nineteenth century in the light of twentieth century observatory-discovery develops the importance of periodic collapse, alternating with 11-year accumulations of inflow and outflow. Both Kilauea and Mauna Loa have worked together. Kilauea has been better measured. Its measured recessions of lava show four 33-year greater collapses. Measured volumes of output from both Kilauea and Mauna Loa rose to a maximum in the middle of a 134-year period, then declined. Sunspot-numbers rose and declined in the same period. The collapse-times accorded with 12 sunspot-minima. The larger 134-year period begins and ends with a major breakdown accompanied by steam-blast eruption. As a whole the century of scientific observation left both craters more filled than before by a few cubic kilometers.

The volcanic process

In conclusion, I want to sketch the volcanic process that appears to be at work, an application of this to human service, and the educational bearings of the work of the Volcano Observatory.

The volcanic process observed in Hawaii is destructive of scientific creeds. We have to give up textbook convictions that explosive eruption is of the essence of terrestrial volcanism; or that volcanism is superficial; that oxidation of gases is not important; that crystallized rocks, after losing their gases, are magma; that Hawaiian lava-fountaining and explosive eruption, respectively, are identical phases of volcanism on differing scales; that major earthquakes have nothing to do with magma; and finally, that we have ever studied by experimental measurement the most important aspect of volcanism, namely, intrusion. If we scientists were as wealthy as oil-corporations, we would be measuring present-day intrusion.

The volcanic process is today a retarded release of shelled-over iron-silica glass. In the glass are dissolved solar gases. The solar matter makes our sphere. The process has succeeded in associating oxygen, hydrogen, nitrogen, carbon, and sulphur--incidentally the ingredients of protein--within a glass of viscous silica, iron, calcium, aluminum, magnesium, and the alkalies.

These gases in high-temperature condition actuate expansive motion when release of pressure is achieved or applied. The release is upcracks in the shell, tectonically controlled. The gaseous elements earlier made air and water.

At extended cracks called volcanic systems they are pumping out lava while adjacent shell presses down. At extended cracks, called epeirogenic belts, they are pumping up intrusives, assimilating sediments, and warming ore-deposits and hot springs. Here the extrusions are andesitic and complicated with faulting coastal edifices, that permit engulfment and explosion of ground-water.

In Hawaii there is no visible evidence that the intrusion breaks--underlying extrusion--are anything more than clustered rift-dikes. What the form or condition of the glass is deeper down no one knows. Since the discovery of deep earthquakes I am disposed to be cautious about using the word "substratum." We have no map of soundings to the seismic discontinuity. Suggested Atlantic and Pacific differences do not make a map.

All of this picture might apply to the Sun after it congealed to restrain the calcium-layer magnetic whirls and the hydrogen-layer prominences. With rapid loss of atmosphere, the picture might resemble the Moon's giant calderas of circular engulfment and her pits aligned within cracks. With accumulated low-temperature atmosphere and hydrosphere it is easy to get a solidified shell and terrestrial sediment restraining gas-release.

There are left pits and domes along circular cracks; see the extraordinary circular curve of Andaman-Sumatra-Java, extended to the Ladrones, all lines of volcanoes.

In time, it is not difficult to envisage the rotational, orbital, and tidal restraints of Sun, Moon, and Earth, and even magnetic fields progressing through the ages to affect magmatic release. The larger shell-units control longer cycles. The smaller edifices themselves control, by their periodic yielding, the pulsations of gas-pumping.

So in Hawaii we get cycles and seasons of volcanism. Our tilts are seasonal. Our major crateral gushings have come at winter solstice. And our 11-year, 33-year, 66-year, and 132-year suggestions of rhythm are certainly useful for expectancy.

Useful applications

Let me illustrate. In 1918 we published expectation of 130 years from 1790, at Kilauea, as a dangerous time; the explosive eruption came in 1924 and was observed throughout with Government control, and only one life lost.

We called attention to the great Hualalai earthquakes and eruption of 1800, ten years after 1790. Six years after 1924 a Hualalai crisis of damaging earthquakes occurred.

In 1935 it was evident that the lava-sources had successively moved up the southwest rift of Mauna Loa since 1855, beginning in 1868. The 1933 eruption was at the summit crater, and the next eruption, according to the law of cycles and commonest time-intervals, should be in about two years and farther north, endangering Hilo. This was published in March 1934 under the title "The coming lava-flow," with a history of dates and intervals explaining the expectancy.

Studies had been made at Kilauea of how a lava-source making a crusted flow in a tunnel might after prolonged gushing be deflected artificially by breaking the tunnel-roof, and so cooling the slag, forming a new source-channel, and so deferring any danger at the former front by robbing it of lava-supply. Air-bombing had been suggested.

The expected flow came in 1935, entered the Hilo Valley, was bombed at the high source and stopped. It fulfilled the expectancy quite satisfactorily. This was the great effervescence of the end of the cycle 1924-35, 11 years. It came where 1843 had poured forth a similar flood. Kilauea has shown lowered elevation and inward tilting.

This furnishes a clue to the next 20 years, correlating location and structure with cycles. It makes 1957 a next major collapse of the Hawaiian system, with 1946 a minor one, and the Mauna Loa northeast rift open and ready to erupt at a source about ten km nearer Hilo than the 1935 vent. This was the location of source of the three most dangerous Hilo flows of the past, 1852, 1855, and 1881. If the 1935 eruption otherwise resembled 1843, it forecasts the most voluminous lava gushing of the twentieth century in the next 20 years.

The Hawaiian Volcano Observatory has therefore recommended, with the approval of the Department of the Interior, that large earth-works three to seven miles long and 15 to 20 feet high, be constructed in three strategic places to deflect coming lava-flows from Hilo Harbor. The aim is to use existing valleys and deflect obliquely. This is what Nature does with old lava-ridges that deflect new flows with 15-foot embankments. The cost will be great. The value of the harbor exposed is a hundred times greater.

Educational needs

The educational bearing of the volcano-observatories of Martinique, Italy, Java, Japan, and Hawaii can now be compared with the study of the air. We are studying volcanic gas, the air-maker. Every day we listen in on the air, use air-planes, air-drills, air-mail, air-conditioning, and air stream-liners. A dozen engineering sciences have grown up. And even more does the chemical laboratory study the thing called life--the protein-gases--and the thing called electromagnetism.

The air is coming out of volcanoes, the constituents of protein are coming out of volcanoes, and volcanoes, like the atmosphere, show blood-kinship to the atom and the Sun. We need skilled men, adequately financed, at all the volcano-stations. And it is by such men that populations may be guided to new industry and new raw materials, away from congestion and unemployment.

Although I am a servant of the Department of the Interior, it must be remembered that the wild places of Earth are not only for conservation but for power. I know of nothing the World needs so much as prizes and subsidies to find and use new materials and new powers in every county of each Nation, with the resources of industry, and the labors of men of science, of engineers, and of workers.

Hawaii has vast sea-bottoms covered with unmapped lava-flows. Hawaii has lava close to the surface, ready to be outlined by the geophysical prospector. Hawaii has, as I have tried to show, a magnetic problem that may equal any of those solved on the Sun by Professor Hale. Volcanology needs instruments and inventors just as brilliant. It stands to reason that the lithosphere will yield a richer harvest than the atmosphere.

Any young research-worker who will go to live in Iceland, the New Hebrides, Salvador, or Tonga can make himself the center of a new science, if he has the will to plan, the apparatus to do and the staying power that is unrelenting and for life.

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ROCK-STRUCTURES ASSOCIATED WITH SOME ANCIENT VOLCANOES

Chas. B. Hunt

(Published with the approval of the Director, U. S. Geological Survey)

The sites of volcanoes or of volcanic regions are not restricted to any single type of local or regional geologic structure. It is true that many volcanoes are distributed along linear belts where the Earth's crust has yielded to great crustal stresses, as for example in Iceland, central France, our own Cascade Range, and along the Gulf Coast. The ancient volcanoes of the Gulf Coast were located along a belt of folded sedimentary rocks, known as the Ouachita Belt [see 1 of "References" at end of paper], presumably a belt of weakness, that served as a hinge line during downwarping of the Gulf Coast in Upper Cretaceous time. In parts of the Great Basin of Nevada and California the great faults that broke the crust into huge basin- and range-blocks exerted some control over the location of the volcanic activity, cones having been built along some of the faults. Yet such faults are only a local factor in the location of volcanoes because other volcanoes drilled their way through the massive granite of the Sierra Nevada independently of faults and erupted at much greater elevations than along the faults [2].

Nor have faults controlled the location of volcanic vents in the Mount Taylor volcanic field, New Mexico, which is located near the edge of an area of very simple structure [3]. The country east of the volcanic field is broken with abundant block-faults and the volcanoes erupted while movement along some of the faults was still going on. One would suppose that such fractures would be sought by molten rock rising through the crust, but the broken area contains relatively few volcanoes; most of the lava came up instead through the little faulted strata a few miles to the west.

The variety of structural settings in which volcanoes are found suggests that the general regional structure of the rocks is not a direct factor controlling the location of volcanic activity although the forces that produce the regional structure may be related to the forces that caused the vulcanism.

The structure of individual volcanoes is about as varied as the general structural setting

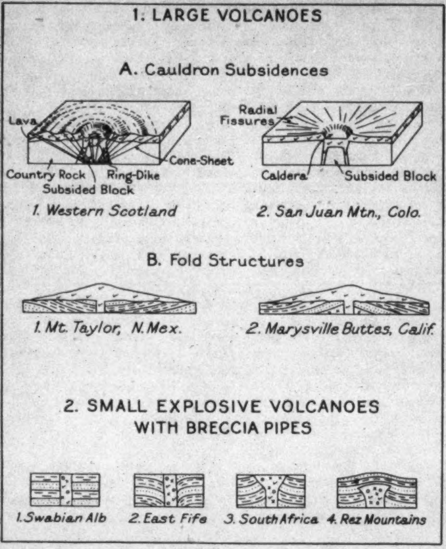

FIG. 1—

Diagrammatic illustrations of eight examples of structures associated with volcanoes

of the activity. One of the best-known types, known as the cauldron-subsidence-type (Fig. 1), is represented by several of the Tertiary volcanoes in the western islands of Scotland [4]. It involves a steeply dipping circular, or ring, fracture which permitted subsidence of a central cylindrical block a few miles in diameter. Lava rose along the fracture in ring-dikes, flooded the topographic depression at the top of the subsided block, and spread over the surrounding country. Younger ring-dikes, in general, are located inside (concave side) of the earlier ones. These younger intrusions are thought to be due chiefly to further subsidence of the central block, but in part they may also have been forcefully injected because the earlier lavas are thrown into gentle ring-folds. In the last stage the subsided block and rocks around it were broken by abundant fractures along cones apexing at depth, and lava was injected upward and outward along them to form cone-sheets.

At several localities in the United States cauldron subsidence-structures with radial fissures have been recognized, but cone-sheets have not been observed. In the Silverton Caldera in the San Juan Mountains, Colorado, radial fissures with dikes or veins in the country-rock are abundant outside the subsided block (Fig. 1) [5]. In the Ossipee Mountains, New Hampshire, a few radial faults cut the subsided block itself [6]. In the ancient Cripple Creek Volcano, Colorado, there was also cauldron-subsidence and erupting vents were located along the peripheral fissure of subsidence but radial fissures have not been found in the older crystalline rocks either within or outside the peripheral ring of volcanic vents [7].

Subsidence of a central block is not so evident in the Mount Taylor Volcano although it is located near the center of a structural trough or basin whose width is practically the same as the volcanic cone. But the Volcano is younger than the down-folding because the earliest lavas truncate the eroded edges of the strata dipping into the basin. The vulcanism and down-folding may, however, be related at depth and the interval of time represented by the erosion may be the time required for the lava to drill its way through the down-folded but non-faulted rocks to the surface. This volcano began erupting from a central vent but the latest eruptions were from fissures that radiate from the vent [3].

Large volcanoes are not always accompanied by subsidence of the adjoining rocks. In fact, the volcano at the Marysville Buttes in California [8] broke out at the top of a structural dome (Fig. 1) produced by the intrusion of a large mass of molten rock prior to the vulcanism. The Valles Volcano, New Mexico, is also at the top of a dome [9].

Most of the volcanoes thus far mentioned erupted vast quantities of lava and ash, but small volcanoes that transferred little or no lava through the Earth's crust may also deform adjacent rocks. The pipes of these volcanoes are filled with rock-fragments torn from the walls (Fig. 1). Some of these pipes may leave adjacent strata undisturbed, as along the Swabian Alb [10] and in the Hopi Buttes, Arizona [11], but in others the adjacent strata may be turned down along the walls of the pipes, as in east Fife [12], or they may be turned up along the walls of the pipes, as at a few localities in South Africa [13].

Pipes not opened all the way to the surface have been described with the strata arched over the top of the pipe and turned up along the sides, as in the Rez Mountains, Roumania [14].

Most breccia-filled necks have been interpreted as the result of violent explosions blasting out a pipe to the surface. But some of the breccia necks in the Mount Taylor field, structurally like the Alb-type, apparently are not due to violent explosions. At one locality, 1,500 feet below the surface of eruption, a bed of sandstone can be traced almost continuously from the wall nearly to the center of the breccia neck. At the center the fragments are small and well rounded, but near the walls they are large and angular. Clearly the fragments have been moved only a few feet from their original position. Through the field as a whole the rock-fragments are mostly from adjacent formations, and fragments of deeply buried formations and plutonic materials are uncommon. The breccia does not occur with the cones and flows of lava at the surface of eruption, but is restricted to the pipes below. The development of the breccia within the pipes was followed by a rise of fluid basaltic lavas through them. I infer that the breccia was formed by some such process as follows. At any given level the first effect of the rising intrusion would be baking that would joint the overlying rocks. Ring-faults around this fractured mass could permit it to settle downward and the upper, probably frothy, part of the lava-column could be injected along the fractures converting the breccia to à plastic mass. Irregular stringers of basalt injected into such a mass could be churned in with the breccia to provide inclusions of relatively pure basalt.

While such a process, as just outlined, might produce a breccia chimney, it is difficult to explain how basalt subsequently rose through this breccia. Probably several processes contributed. First, some large blocks of the breccia must have settled to be physically mixed or chemically combined with the magma at great depth. Secondly, some of the breccia certainly fused in place and may have been incorporated immediately into the basalt. Thirdly, some increase in the volume of the pipe may have been provided by horizontal compaction of the adjacent weak shale in response to forceful injection of the basalt into the breccia. Fourthly, when the basalt finally reached the surface, it may have enlarged its channels by floating off small fragments of the breccia walls.

Only a few of the many volcanic structures have been cited. Although in general the volcanoes that erupt similar lavas within any one field are apt to deform adjacent strata alike, similar volcanoes in another field will behave very differently. Volcanic structures thus tend to be as provincial as the structures and architecture created by man, and the controlling factors are probably equally variable.

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THE ROOTS OF VOLCANOES

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Although explosion may initiate volcanism of the type represented at central vents, it is not essential for continued activity. Nor is such violent action necessary even for the opening of central vents, as shown by the pit-craters of Hawaii. These are genuine volcanic vents and yet are not rimmed with ash, tuff, or breccia, the normal products of explosion. On the other hand, the pit-craters, like Kilauea and many other craters on islands and continents, were opened and kept open by fluxing magmatic gas, rising quietly. Quiet emanation of gas is the essential part of the mechanism that gives prolonged activity. But emanation from what? This is the ineluctable, chief problem of volcanology. The root of the volcano is the root of the matter.

The limited size and life of every volcano suggests that at each center fluxing gas rises from an isolated chamber of liquid rock. Why is the volume of gas-charged magma so limited? Only two of the possible answers need be considered. First, one might assume the volume of gas-feeding magma to be limited because the requisite heat is only locally supplied in an Earth which is almost entirely crystalline and solid; or, second, assume the feeding melt to be a localized injection, risen from an eruptible earth-shell into the more superficial, solid rock of the planet.

The first hypothesis is not adequately supported by field-observations or by facts known about the interior of the globe. The frequent appeal to local relief of pressure as an inciting cause for the molten state has little force, when it is once realized how trivial must be the maximum effect of relaxed pressure on the melting temperature of underground rock. Nor is there any better, proved basis for belief in sufficient, specially localized heat developed by radioactivity or otherwise.

According to the second hypothesis injection is the preliminary to all volcanicity. This is manifest at many localities where the lava and gas issuing at cone and crater have been delivered from clearly exotic bodies of magma, thrust into a fractured crust. Although such intimate connection between plutonism and volcanism may be pictured in different ways, only one will now be discussed. The thesis will be presented that the roots of volcanoes are best regarded as intracrustal, molten injections which stem from a world-circling couche of vitreous basalt, beneath the crystalline crust. In fact, the principle can be stated in a still more comprehensive form, namely, all igneous action whatever is the result of abyssolithic injection, that is, local penetration of the crust by molten masses which initially had no floors of crystalline rock. The root of a volcano is an abyssolith.

The postulate of crust and substratum has a world of implications outside the field of volcanology. If correct in principle, it should satisfy tests supplied by the facts of plutonism, petrogenesis, orogeny, epeirogeny, isostasy, and seismology. Because volcanism is a planetary problem and one most intimately associated with so many other fundamental questions of physical geology, a solution to that problem will be facilitated by putting into competition all reasonable, imaginable models for the Earth. Elsewhere the writer has discussed the crust-substratum model in relation to: The origin of mountains; crust-warping; isostatic adjustment; the latent heat of magma; the diversity, consanguinity, and eruptive sequences of igneous rocks; the structural relations and emplacement of batholiths and other intrusive bodies; the ophiolitic injection of mountain-structures; the large-scale basining of the Earth's crust under the wide fields of plateau-basalt and under vast gabbroic intrusives; the distribution of central volcanoes, whether aligned or scattered in irregular networks; the continuance of activity, periodicity, and final extinction of these vents [see 1 of "References" at end of paper]. On the present occasion attention will be concentrated on particular tests of the suggested earth-model, tests derived from new data.

(1) First, the model should provide the conditions for the isostatic balance of the lithosphere. In fact, the required weakness of the asthenosphere is a natural characteristic of a hot, vitreous substratum, even though this layer may be able to carry minute shearing stress for millions of years. It is assumed that the thickness of the crust varies with the efficiency of the radioactive furnace, the Sial being more radioactive than the basic rock of the Sima. Hence the crust of the model, when in thermal equilibrium, is thinner under a Sialic continent than under the ocean, where the Sial is thin or quite absent, and thinner still under the extra Sial of the mountain roots. Here the convenient word "root" means of course something quite different from what has been called a volcanic root. A continent or mountainous belt owes its isostatic support not only to a root of light, Sialic rock above, but also to what may be called an "anti-root" of relatively light, vitreous rock below (Fig. 1). Thus in the model the densities of the rock are arranged according to a combination of the ideas of Airy, Heiskanen, Pratt, and Hay-

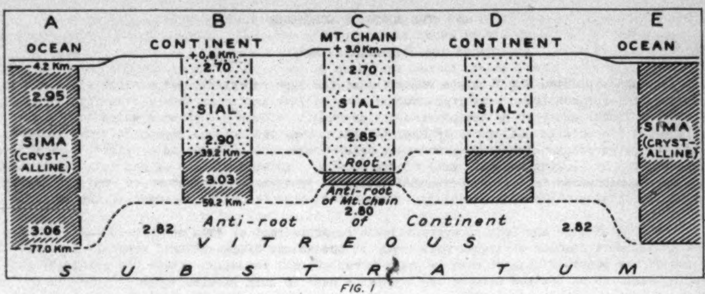

FIG. 1

ford, plus the conception of the anti-root.

This new scheme gives a center of gravity of isostatic compensation for continent, high plateau, or low basin at nearly the same level as the center of gravity of compensation computed according to the assumptions of any of the older schemes. Hence in a general way the crust-substratum model seems to pass the examinations set by plumb-line and gravity-pendulum.

(2) What Dana called "ascensive force" for eruption is provided by the model, because it demands for the crust an average density which is higher than the average density of a molten, basaltic abyssolith. The eruptive pressure is a special by-product of isostatic adjustment at a through-going fracture of the crust. Where the crust presses with its full weight at the foot of a rising abyssolith, this molten column becomes isostatically balanced only after its top is a little more than three km (continent) to about four km (ocean) above sea-level. That is the range of maximum heights which, according to the proposed earth-model, can be reached by the liquid, bubble-free column of basalt. It may be recalled that the approximate heights of continental Mount Etna and oceanic Mauna Loa (lava-lake) are 3,300 and 4,000 meters respectively. Vesiculation of lava-columns and possibly also changes of volume due to chemical reactions are supplementary conditions for the ascent of lava.

(3) New and extraordinarily important evidence for the extreme weakness of the asthenosphere has been published by Hirvonen [2]. He reports the free-air and Bouguer anomalies of gravity at 202 stations in the eastern half of the glaciated tract of northwestern Europe. The mean for all 202 free-air anomalies with regard to sign is -7.5 milligals, and the mean for the Bouguer anomalies is -16 milligals. The corresponding mean Hayford anomaly for all 202 stations may be closely estimated at -10 milligals. The mean Hayford anomaly for 40 stations in the central area of the glaciated tract, 300 km in radius, is approximately -17 milligals. If the last figure means deficiency of mass in the central area, this deficiency is equivalent to about 150 meters of granite. The glaciated tract as a whole appears to show a deficiency nearly equal to 90 meters of granite.

Now Witting has proved that almost all of the glaciated tract is being upwarped, the maximum rate of rise, at the center, being a little more than one meter per century [3]. The upwarping is conditioned by stress-differences in the material under the flexible crust. From Timoshenko's analysis of a similar problem it follows that the maximum stress-difference under the center of the glaciated tract, roughly circular and 1,500 km in diameter, is now located at the depth of about 600 km and there amounts to some 12 kg per square cm [4]. At the depth of 100 km the stress-difference comes out at only about 2.5 kg per square cm.

The isostatic recoil in Fennoscandia still continues; hence these relatively small stress-differences are above the strength of the asthenosphere. After the lapse of a few more thousands of years but before upwarping has quite ceased, the stress-differences will be much smaller than at present. Because the crust has some stiffness we should expect small "permanent" stress-difference to remain within it, while either comparatively minute stress-difference or none at all should remain in the asthenosphere (substratum).

With a maximum strength of considerably less than 2.5 kg per square cm for the asthenosphere, at the sample depth of 100 km, may be compared the older estimates of Barrell--200 to 300 kg/cm^2--and of Jeffreys--about 100 kg/cm^2. Unless the data of Hirvonen and Witting have been wrongly interpreted, Nature's great experiment on the distribution of strength under Fennoscandia indicates, more clearly than ever before, the rule of isostasy and the exceedingly small strength of the asthenosphere. Such weakness goes with hot glass better than with crystallized rock, and once again the existence of a vitreous substratum under a true crust is suggested by field-observations.

It is significant that the mean Hayford anomaly of the 27 United States stations within the glaciated tract of North America is -11 milligals or not far from the mean characterizing an analogous belt of the European tract. Evidently comprehensive study of tide-gage records around Hudson Bay and James Bay and also measurement of gravity at many new stations in eastern Canada are much needed, for purposes of comparison. Meantime the Fennoscandian test of our earth-model is eloquent.

(4) The reality of a true, thin crust resting on a vitreous earth-shell is doubted by some mathematical physicists. One of the chief grounds for their scepticism is the result of computing the internal distribution of heat in the Earth. Their results have seemed to show our planet to be too cool, and to have been too cool since an early Archean epoch. This conclusion is based on a number of unproved assumptions regarding the relative proportion of original heat and heat of radioactivity, the efficiency of convection, the right values to be assigned to the thermal gradient, coefficients of thermal conductivity, and other quantities necessary for the calculation of existing temperatures at depth. Among all these uncertainties only one, the amount of Earth's heat due to radioactivity, will here be emphasized, and again the meaning of relevant, comparatively recent discoveries is to be noted.

At the present time something like one-third of the heat generated in the Sial by radioactivity is due to K_{40}. Because this isotope has a comparatively short life, its thermal contribution in early Archean time was about twice as rapid as now [6]. Similarly the actinouranium of the infant Earth was a more efficient heater than at present. Calculations of the existing thermal gradient at depth have been made without allowance for these two causes of a greater rate of production of heat when the Earth was young. But the allowance may be quite trivial in comparison with one that a philosophical physicist might suggest as necessary. Specialists on the problem of atomic constitution have for some time been asking themselves whether there were originally, deep in the body of the Earth at the beginning of its long history, many radioactive elements or isotopes, each with half-life equalling a few thousands, millions, or scores of millions of years. If so, such heat-producing particles added a great store of heat to the interior of the planet and yet have left no discoverable trace of themselves. This speculative query has become all the more justified on account of the fact that four new, short-lived elements, with atomic numbers of 93 to 96 inclusive (uranium at 92) have been artificially created. What limit, then, can be set to the number and total mass of short-lived elements inherited by the Earth from the parent Sun and trapped at great depth in the planet after it was rapidly condensed into a ball? Be the answer what it may, it seems clear that the idea of an existing vitreous substratum can not be dismissed because it fails to give any of the thermal gradients so far deduced by mathematics.

(5) A common objection to any hypothesis involving a non-crystalline substratum (not undercooled) is that rock-glass above the temperature of melting can not transmit shear waves of the type recorded on seismograms. That argument is seen to be greatly weakened when account is taken of Ide's remarkable experiments on various glasses [7]. He found these glasses, at one atmosphere of pressure, to transmit sound-waves even after the temperature had been raised well up into the annealing range. Still more recently Volarovich has reported effective rigidity in molten basalt at a temperature as high as 1,100° C [8]. It is reasonable to expect that at the minimum ruling pressure of 17,000 atmospheres the glassy substratum should retain "restoring force" at temperatures somewhat above 1,100°. Here the question is not merely one concerning the relation of high pressure to viscosity, when the viscosity is measured by application of large increments of stress. The strains used in all the classical methods of testing the rigidity of the earth-shells, whether seismological or gravitational, are, like the corresponding stresses, extremely small, so that the rate of decay of the rigidity characterizing a vitreous layer at depth may be almost immeasurably small.

In summary, there seems as yet to be no certain reason, derived from seismological or other geophysical evidence, for rejecting the postulate of a vitreous substratum.

(6) Some geophysicists have concluded that a thick, world-circling layer of crystallized dunite or other kind of peridotite begins at a depth less than 50 km or even 40 km below the Earth's surface. This view is hardly reconcilable with a model embodying a vitreous, basaltic substratum that begins at depths between 60 and 80 km. The preference for the crystallized,

ultra-basic shell so near the surface (the lower part of the vitreous layer may be ultra-basic) is based on computed values for the velocities of earthquake-waves just below the first major discontinuity in the Earth's substance, nearly 40 km from the continental surface. It is supposed that those velocities, because so high, can characterize rock at the level stated, only if that rock be ultra-basic in composition and also crystallized.

The argument rests on the older measurements of the elastic coefficients belonging to the standard types of rocks. The later and somewhat more accurate measurements by Birch and his colleagues at the Harvard Geophysical Laboratory have given coefficients which in general are slightly higher. It now appears that the velocity of the longitudinal wave, just below the discontinuity near the 40-km level, is only about seven per cent larger than the velocity expected in unmetamorphosed diabase or gabbro (crystallized basalt) at the same depth [9]. This difference would be diminished if the layer in question is basaltic but crystallized in a garnet-bearing or other high-pressure phase, with both bulk-modulus and rigidity above those of ordinary diabase or gabbro.

Moreover, we may well doubt that any specimen of rock from quarry or ledge has yet given any experimenter the coefficients of elasticity which the same material would have at the actual pressure and temperature ruling just below the discontinuity. For, excepting quartz, the minerals of any specimen of accessible rock are crowded with actual, open cleavages which are not entirely closed even at the great pressures (and low temperatures) hitherto employed in the laboratory [10]. The cleavages are not quite closed because, at the high pressures, the constituent crystals have great strength or else much increased viscosity. Such residual cracks in the material increase the compressibility of the rock and decrease its rigidity; hence the calculated wave-velocities are lower than those expected if the minerals of the same rock were quite devoid of open cleavages. But open cleavages are hardly conceivable in rock permanently exposed to the temperature and pressure corresponding to the depth of 40 km or more. It seems clear that this contrast between the accessible and inaccessible samples of the same kind of rock must be considered before an attempt is made to diagnose the lower parts of the crust by comparing actual and computed wave-velocities.

For more than one reason, then, the writer is inclined to regard the material under the continents and between the depths of 40 and 60 km as crystallized but "gabbroic," basaltic in chemical composition, rather than peridotitic.

(7) If the lower third of the continental crust is "gabbroic," one can rather easily credit a layer of vitreous basalt, beginning in continental areas at the depth of approximately 60 km. The conditions for the actual break at this level are: First, a sufficient thickness for the Earth's basaltic shell as a whole; and, second, a thermal gradient steep enough to give the temperature of melting below the interface between frozen and unfrozen basalt. But at that interface there should be a moderate discontinuity for the transmission of seismic waves; the wave-velocities in the vitreous sub-layer should probably be a little smaller than the velocities in the overlying "gabbroic" sub-layer. It is, therefore, significant that in a forthcoming book on the Earth's interior, to be published by the National Research Council, Washington, Gutenberg will reiterate his statement, that there appears to be registered in seismograms a shadow-zone of the kind expected if there is a change from crystalline rock to vitreous rock of the same composition at the depth of 60 to 70 km [11].

Time fails for a full discussion of two more objections to the suggested earth-model, neither of which, however, seems cogent.

(8) One of them is founded on the reality of earthquakes with deep foci. This argument can not be regarded as valid until it can be shown that sudden ruptures in the vitreous material of the substratum are impossible during isostatic adjustments. This saving proviso has never been demonstrated, and it is hard to see how its proof can be obtained.

(9) The other objection is based on the apparent evidence for the triaxiality of the figure of the Earth [12]. All that need now be said is that, if the triaxiality is ultimately proved, it would not necessarily dispose of the idea of a weak or even liquevitreous substratum with moderate thickness. For the inequality in the distribution of mass causing the triaxiality and the corresponding stress-differences might well be restricted to a pressure-solid, centrospheric shell, leaving the relatively thin substratum nearly or quite free from permanent stress-differences.

Conclusions

As we review the data of geophysics they seem to permit, if not actually to compel, faith in the value of the crust-substratum hypothesis. But at least as strong a sanction for that faith comes from observations in the more purely geological field. In the writer's opinion no other earth-model yet published has as much practicality when we try for reasonable explanation and correlation of the numberless facts of physical geology, including those of volcanology. So by a long but necessary detour we come back to our subject, the roots of volcanoes. According to systematic deduction from a host of facts the feeders of volcanoes appear to be abyssoliths, molten masses injected upward into the Earth's crust from a vitreous, gas-charged substratum of basalt. The injected melt may or may not be contaminated with the material of the invaded crust. The suggested mechanism as a whole enables us to understand the opening of volcanic vents, their duration and periodicity of action, their final extinction, the varied composition of their lavas, and their geographical relations, whether the vents be aligned (along through-going fissures) or irregularly scattered (gas-fluxed vents above sheet-like offshoots of abyssoliths).

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[10] P. W. Bridgman's stoppered, capillary tube of glass did not collapse at the hydrostatic pressure of 24,000 kg/cm^2 (see Phil. Mag., p. 72, July 1912). He found that a cylindrical cavity in andesite, about 3.5 mm in diameter, did not collapse though the material was suffering a compressive stress of 15,000 kg/cm^2 (Amer. J. Sci., v. 45, p. 260, 1918).

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SUMMARY OF SYMPOSIUM ON THE PHYSICS OF VOLCANIC PROCESSES

L. H. Adams

I am glad to avail myself of the opportunity afforded by your Committee on Meetings to make a brief summary of the program. This Symposium has provided a splendid outlook on the problem of volcanology and, without doubt, has clarified the subject in the minds of those who have been fortunate enough to be present this afternoon. In the time available it is probably not possible to make an adequate summary, but I should like to make a few general comments at this time and to recall the scope of the program.

Quantitative measurement is the outstanding requirement for successful research in volcanology as well as in other sciences. This was emphasized in the paper by Dr. Day, who suggested that we should have, with speculation, a liberal mixture of experimentation. In spite of the physical difficulties, which often are quite formidable, we need additional measurements of many varieties.

Dr. Zies has given us a picture of the inspiring phenomena of volcanism that are presented to those fortunate enough to be in the vicinity of a volcano at a favorable time. It is evident that a further study of fumaroles, especially in the initial stages preceding a volcanic outburst, would be of great value and should advance materially our understanding of volcanic phenomena.

An interesting point of view has been presented by Dr. Jaggar, who described the development of volcanic cones, especially in relation to magmatic pressure and to volcanic cycles. These considerations allow us to correlate many phases of a volcano's history, and appear to be valuable not only scientifically but economically by providing an estimate of future behavior of volcanic centers. The general occurrence of side rifts associated with volcanic cones is a feature of great significance.

The petrological and structural features of volcanoes, especially in relation to caldera-subsidence and other great subsiding areas, as outlined by Dr. Hunt, clearly indicate the complexity of the problems awaiting solution. It is worthy of note that the phenomena and underlying causes of subsidence, either on a large or small scale, appear to present one of the most baffling problems in geophysics.

In his contribution dealing with the roots of volcanoes Dr. Daly has pointed out the impossibility of separating volcanism from the other major problems of physical geology, and has presented for us a simple picture of a mobile substratum which readily accounts for many geological phenomena including volcanism. Lack of time prevents an adequate discussion, on this occasion, of all the implications of Dr. Daly's contribution. It is encouraging that steady progress is being made in correlating the diverse aspects of the intrusion and extrusion of magmas. On the other hand, it appears to be the rule rather than the exception for processes and conditions that readily explain geologic phenomena to offer great difficulties from the standpoint of physics and chemistry.

Finally, it is becoming increasingly apparent that, in order to further our knowledge of volcanism and related problems in geophysics, we must extend our studies still deeper in the Earth and still farther back toward the beginning of our planet.

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DISCUSSION

A. C. LANE (Tufts College)--Mr. Chairman, as Dr. Daly asked me to say what I thought about potassium, I will say this. In figuring the age of the Earth, I begin now and work back, and the further back you get the more uncertain you are. As to just how much potassium is doing, we want a lot more facts. There are old white micas with a lot of potassium in them, and they don't seem to be affected. Farther back than two billion years we may have had potassium that is a lot more active than at present. Today we want to know a lot more about the rate of decay of potassium.

R. A. DALY--My statement that about 1,500,000,000 years ago, in Archean time, K_{40} was delivering heat twice as fast as at present, is based on an estimate by an expert in this field of study, namely, Professor Evans of the Massachusetts Institute of Technology.

A. C. LANE--If I may reply, is Evans here? He ought to be. It would be rather interesting to find out at what rate K_{40} is changing and how much product of decay there is in our oldest micas. It is supposed to be Ca_{40}.

H. F. REID (Johns Hopkins University)--Mr. Chairman, I should like to comment on another paper in our program. Mr. Hunt has called attention to certain extinct volcanoes in Scotland characterized by radial dikes and peripheral faults. I think we can understand in a general way how these dikes and faults could have developed. Gilbert, in his famous memoir on the "Geology of the Henry Mountains," showed how radial tensional cracks could be formed by the upward pressure of underlying molten lava; and Dr. Jaggar has actually measured vertical movements about Kilauea presumably due to such pressures. Lava would pour into these cracks and solidify on cooling into radial dikes. The liquid lava would probably take advantage of one of these cracks, perhaps the largest, to rise to the surface, and by melting off any rocks projecting into its main current would gradually develop a more or less circular vent. When the energy responsible for the volcanic activity is exhausted the lava would sink, leaving a great pit like that of Halemaumau, and naturally its walls would fall in. They might crumble in or they might slip in in larger masses. The landships in the chalk-cliffs of the southern coast of England seem to illustrate the latter method--they have all the appearance of normal faults. We cannot expect any regularity--each volcano would probably present certain characteristics peculiar to itself--but I think the ideas above offer, by and large, a reasonable explanation of the phenomena mentioned by Mr. Hunt.

W. D. URRY (Massachusetts Institute of Technology)--I would like to ask Dr. Daly why he requires this large excessive heat in the past. Would not a continuous excess of radioactively generated heat over the heat-loss answer the same purpose?

R. A. DALY--If the Earth, just after its formation, was heated by radioactivity much faster than at present, that fact would have to be considered during the calculation of the existing thermal gradient at depth. On the other hand, there is evidence that, since Archean time, the heat of radioactivity has not been increasing the temperature and therefore volume of the Earth. Such increase of volume should have stretched the continents, with the development of abyssolithic fissures and dikes. Yet an area of some two million square miles of eastern Canada seems to lack dikes erupted since the late Pre-Cambrian. The same is true of other extensive regions. Hence it seems preferable to assume that since Archean time the planet has been contracting, not expanding.

W. D. URRY--I put the question because all the hypotheses of the internal structure of the Earth as postulated, for example, by H. S. Washington or Dr. Daly, produce a radioactivity-distribution scheme by analogy with radioactive data for corresponding types of meteorites, generating heat at a rate 20 to 30 times the rate of loss. (Considering only the upper shells and assigning minimum radioactivity-contents, the heat produced in the first 100- to 200-km depth counterbalances the loss.)

R. A. DALY--The stated excess of heat is derived from a set of assumptions. Yet the facts of geology do not seem to square with those assumptions. May I go further and register a protest against the statement that rigorous mathematics forbid us to assign to the Earth's outer shells the temperatures postulated in the suggested earth-model? One can not doubt the soundness of the mathematical procedure, but he can as little doubt the uncertainties in the premises, to the number of half a dozen. For example, the values assumed for thermal conductivity may be considerably in error. Dr. Birch and Dr. H. Clark at Harvard University have succeeded in measuring the conductivities of many standard types of rock at all temperatures up to about 500° C, but only at atmospheric pressure. Until these and other data are published, computation of the existing thermal gradient can hardly give trustworthy results.

A. C. LANE--I want to say that I agree absolutely with Dr. Daly as to the great uncertainty about the conductivity and temperature of the Earth. As a matter of fact, it was about the potassium that I was concerned. We don't know what the rate of increase of temperature is under the Earth and our older figures for gradients don't work for Canada. There is still plenty of uncertainty about that yet.

L. L. NETTLETON (Gulf Research and Development Company)--I would like to ask Dr. Daly how would this low-density material act to keep a volcano, once started, from going on continuously. The thing is inherently unstable. When the flow starts up, why should it not continue until the supply is exhausted?

R. A. DALY--According to the suggested earth-model, extrusion from a basaltic abyssolith will go on until a superficial cone is built to a maximum height of three or four km above sea-level. When that height is reached, the hydrostatic pressure at the base of the abyssolith is equal to that due to the weight of the crust alongside the abyssolith. The duration of the volcanic activity is determined by the total volume of the abyssolith. If this is large, it takes a long time to freeze it and to stop its output of fluxing gas--hundreds of thousands of years. The cone containing the liquid column is a self-sealing structure (magma filling tensional cracks in the cone are quickly solidified), the crust is stable, and the volcano has a limited life, though, it may be, a long life.

S. HAMMER (Gulf Research and Development Company)--According to Dr. Daly's model, all the lava-lakes should be at equilibrium and should be at the same height.

R. A. DALY--Yes, provided that the crust presses with its full weight at the base of the abyssolith. But the crust has a certain bending strength. Because of this stiffness the crust does not press with its full weight at the base of the abyssolith, except under special conditions. In by far the majority of cases the abyssal fissures have been sealed, by chilling and solidification of the abyssolith, before the top of the molten column can rise to the theoretical maximum height.

S. HAMMER--What is the evidence on these old volcanic-lava lakes? Do they tend to be at the same elevation?

R. A. DALY--The heights of basaltic-lava lakes differ widely. The lakes at Mount Etna and Mauna Loa seem to be the only instances where the height reached is seen to be near the theoretical maximum.

M. FLEISCHER (Geophysical Laboratory)--How do you account for the intermittent action of volcanoes?

R. A. DALY--Assume an abyssolith with its thread of an active basaltic volcano. At the surface chilling is rapid; the column plugs itself at the top. Were it not for circulation of fluid at Kilauea, that Volcano would plug itself one kilometer deep every few days, if the loss of heat at the crater went on at the rate estimated in the year 1909. Convection in the lava-column and concentration of gas near its top are periodic. Hence at intervals the vent becomes plugged. Under each plug hot gas accumulates and ultimately in amount sufficient to blow the plug out or else melt it without any violent explosion. Quiet fluxing is the normal thing at Kilauea and Mauna Loa.

JOHN L. RICH (University of Cincinnati)--Going further with the question that Mr. Urry raised, do we not actually have evidence of a great deal of stretching of the Earth's crust and the escape of an enormous amount of hot rock that would periodically relieve excess heat that radioactivity-investigations indicate must be accumulating? I am thinking of such things as the Great Basin, the similar action that occurred in the Appalachian Region during Triassic time. Are there not evidences of a great deal of stretching in certain places at certain times?

R. A. DALY--On the whole, decidedly so, but it does not affect eastern Canada and large parts of Africa--stable areas. All the loss of heat that escaped from the planet when the great Columbia River lavas were erupted could not affect essentially the temperature of eastern Canada.

A. G. McNISH (Department of Terrestrial Magnetism)--Is this basaltic layer, which is capable of producing volcanological activity, something which encircles the entire Earth or is it merely capable of producing volcanic activity in a few restricted regions?

R. A. DALY--According to the proposed earth-model the vitreous basaltic layer is a world-circling shell with a moderate thickness, its base being perhaps no more than about 150 km below the surface of the Earth.

A. G. McNISH--Is that restraint of volcanic activity confined to local regions?

R. A. DALY--Volcanism results from the opening of abyssal fissures and the location of these depends on planetary stresses and on the whole dynamic history of the crust. These conditions do localize volcanism, but, again according to the model, eruption to the surface is potential at all points on the Earth's surface.

T. S. LOVERING (University of Michigan)--I would like to ask Dr. Zies if he can tell me, in connection with that flow in Guatemala which he showed on the screen, whether or not the broken-up rock-fragments might be confused with glacier debris left by the ice. Were any of the rocks striated or grooved?

E. G. ZIES--Those that I have seen in the flow are not.

T. S. LOVERING--I understand that in some cases those fragmentary volcanic breccia-deposits are scratched by one fragment moving over another one.

E. G. ZIES--If there is any scratching there, it is not noticeable.

L. C. GRATON (Harvard University)--Some of these points that have been receiving most discussion just now are decidedly speculative. May I refer to one or two questions that seem somewhat more direct? Several of the contributors placed emphasis on the mechanism of subsidence. There is subsidence, but is it net subsidence? If it were, would we see so many volcanoes, visible only because they are protuberances? Is not the subsidence an incident, which at any time may be conspicuous; but is not the dominating feature the building up of a rock-mass consisting of material transported from regions below?

We have heard a great deal of emphasis today upon gas. Many seem to have come to worship gas as the prime motive power in volcanism. I am much impressed by Dr. Daly's mechanism whereby the fundamental means of pumping rock-material to the surface does not involve the gas-phase. The manifestations of gas which make so profound an impression on the observer are probably relatively shallow phenomena. They are the inevitable consequences of moving volatile-containing materials to regions where the pressure is no longer sufficient to retain the volatiles in the liquid phase. Our observations are made at the very "top of the chimney"; here, the conspicuous emanation is often gas and it does work as such; but it seems to me more likely that the vastly dominating fraction of the total work was done at depths where probably the gas-phase was relatively subordinate and rarely could exist. Finally, may I inquire from Dr. Daly whether his proposed mechanism for volcanic extrusion is to apply only to basaltic volcanoes or whether he imagines the same essential process to apply to volcanoes which yield lavas of considerably different composition.

L. L. NETTLETON (added by mail)--I would like to suggest an answer to my own question. In a fluid mechanical hypothesis for the formation of salt domes (L. L. Nettleton, "Fluid mechanics of salt domes," Bull. Amer. Ass. Pet. Geol., v. 18, No. 9, pp. 1175-1204, September 1934] a process was suggested for a density-situation, of which the elements are similar to Professor Daly's model, by which the upward flow of lighter material would be cut off by the collapse of the overlying heavier material into a "peripheral sink." In fact, this theory shows that the continued flow of the lighter material requires a definite strength of the overburden to resist the collapse which would cut off the lateral flow of light material into the area from which it is flowing upward. The general principles of this theory were demonstrated by experiments with highly viscous liquids (a thick syrup with density 1.4, underlain by a very viscous crude oil with density 1.0). It seems reasonable to postulate a similar principle for the flow of volcanic material under the density-assumptions which Professor Daly has proposed. Under such a postulate the "peripheral sink" may be invoked to limit the amount of lighter volcanic material which would reach the surface.

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ADVANCED SURVEYING AND GEODESY IN THE CURRICULA OF ENGINEERING COLLEGES TODAY

P. H. Underwood

For some time I have been interested in a study of the place of surveying in its different branches in the curricula of American colleges and universities, particularly as it is taught to students in civil engineering. About two years ago, I examined the catalogues of a large number of the principal engineering schools and made a tabulation of the number of credit hours devoted to surveying.

It was found that there is a great variation in the names of the courses, in the subject-matter covered, and in the distribution of it throughout the curricula. In spite of this variation we may consider that most of the courses offered might have been entitled by one of the following designations, or by what I might call type-names for courses: Elementary surveying; advanced surveying; topographical drawing; route surveying; practical astronomy; least squares; geodesy; photographic surveying; and summer survey.

In tabulating credit-hours it was decided to group results under fewer headings than would have been required by using all of the titles mentioned. Accordingly a seven-column arrangement was used. The name of the institution was listed first and this was followed by columns devoted to elementary surveying, advanced surveying, route surveying, summer survey, miscellaneous, and total hours of credit in surveying. Data were tabulated on the semester-hour basis for 62 different colleges, technical schools, and universities to show the number of credit-hours devoted to surveying and geodesy in the curricula in civil engineering. In counting credits, it was considered that one credit-hour is in general equivalent to one recitation or lecture, or to a 2 1/2- to 3-hour field- or laboratory-period a week for a term of about 16 weeks. For those institutions that have a three-term system instead of the more common semester arrangement, the numbers of hours of credit were multiplied by two-thirds to reduce the data to a comparable basis. When hours of credit were not given for summer surveys, a week of camp was considered to be equivalent to an hour of credit.

The assembled data showed a variation of total credit-hours in surveying from a minimum of seven to a maximum of 28. The median number of hours per semester was about 12 1/2. If it is considered that the number of credit-hours required for graduation is on the average 140, the percentages of time devoted to surveying are about five per cent for the curricula with the minimum content of surveying to 20 per cent for that with the maximum, the median being about nine per cent.

The typical minimum surveying-content of a college curriculum in civil engineering might, in view of the data collected, be considered to consist of three courses, an elementary one of three hours, an advanced one of two hours, and a course of three hours in route surveying. The median course might be much the same except for the addition of a summer-survey course of about four weeks. This course in summer would be devoted chiefly to topographic surveying and triangulation but might include some route surveying. In increasing the content beyond the median, additional matter would be found offered either as expansions of the courses listed or in the form of separate additional courses in astronomy, least squares, geodesy, or topographical drawing.

In those curricula where practical astronomy is taught as a separate course, it is usually given as a two-hour one. More frequently, however, it is offered as a part of the course in advanced surveying. Geodesy, least squares, photographic surveying, and topographical drawing are given as separate courses in relatively few institutions. Thus, of the institutions studied, only five list geodesy as a required course for students in civil engineering, three require least squares, eight require topographical drawing, and two require photographic surveying. To be sure, the elements of some or all of these subjects may very likely be included in the advanced-surveying courses required of students in many of the institutions. No study of this was made.

Two or three institutions offer special curricula or options in surveying, geodesy, or geodetic engineering, and thus make it possible for a student to get more thorough instruction in geodesy than he would get in the regular course in civil engineering. The University of Michigan, for instance, offers a course leading to the degree of Bachelor of Science in Engineering (geodesy and surveying) with a total surveying- and geodesy-content of 24 credit-hours. Among the surveying courses included are: Geodesy, three hours; astronomy, two hours; least squares, two hours; and photographic surveying, two hours. The purpose of this special curriculum is to fit a student for engineering and geodetic surveying. At Syracuse University a special curriculum in photographic surveying is offered. Cornell University offers a special option in geodetic engineering. In the curriculum of this option, which leads to the same degree as the regular four-year course in civil engineering (B.C.E.), are included a high percentage of technical courses other than those in surveying and geodesy. It offers a good training for a student not only in surveying but also in other lines of engineering. The surveying content of this geodetic option is as follows: Elementary surveying, three hours; advanced surveying, three hours; field astronomy, two hours; route surveying, three hours; summer survey, five hours; mapping, two hours; topographic surveying, one hour; adjustments of observations, three hours; photographic and aerial surveying, three hours--a total of 25 credit-hours. Students electing this option are required to take six hours of electives, in which they may include additional courses in surveying if they wish.

Not many institutions in this country offer graduate work in surveying and geodesy. In so far as the writer knows, there is not much demand from American students for graduate work in geodetic engineering. Perhaps one reason that there is not more interest in the study of surveying and geodesy in this country, both as an undergraduate and a graduate study, is that surveying is not looked upon as leading to a career, or, if it is, it is still believed that it is one of those things that can be learned by experience. When this experience is obtained from organizations like the United States Coast and Geodetic Survey or the Geological Survey, which train their own personnel, perhaps it does not matter very much since these organizations do such a good job at it, but, for those who will later find their way into private practice in surveying, the results are not so fortunate. It has often seemed to me that the training of young men in preparation for undertaking work in surveying is not very well taken care of in our technical schools for the reason that surveying is so often considered to be simply one of the fundamentals and not studied thoroughly as a preparation for later work.

The teaching to non-civil-engineering students of the use of surveying instruments and the methods of surveying is considered by some as unfortunate since, too often, those with a mere smattering of surveying undertake to do surveying work for which they are not well trained. It has even been suggested that the teaching of the subject of surveying to other than civil-engineering students should be confined to a historical and descriptive course with the idea of making the training a cultural one rather than a technical one and thus not encouraging others than civil engineers to engage in the practice of surveying.

As for geodesy as a higher branch of surveying, it is often considered something too abstruse and too highly mathematical to be undertaken as a study by students in civil engineering. One great argument for the institution of plane-rectangular coordinates in this country is that thereby it will be made unnecessary for the ordinary surveyor to have any knowledge of geodetic positions in order that he may tie in his surveys to the fundamental control-system of the country. Thus it might not be considered an extreme statement to say that he is discouraged from getting more than an elementary knowledge of surveying--such a knowledge as might be included in a very elementary course in the subject. If this is so, it will not tend to raise the standards of instruction in surveying and geodesy. Those few engineers or geodesists who have to make the necessary computations to set up the coordinates of the fundamental control-points will need a more thorough grounding in higher surveying and geodesy. These, however, need be but few in number compared with the engineers and surveyors who, it is hoped, will make use of the systems.

We might also consider the trends in teaching of surveying. Years ago, when so many of our civil engineers found employment either in governmental service or in doing the surveying required in locating and constructing our vast railroad systems, surveying formed relatively a much more important part of the curricula. In those early days other fields for civil engineers had not been so fully developed as they have today. As conditions have changed, increasing stress has been laid upon those courses that are a preparation for the design of structures in steel and concrete, so that it has been necessary to find a place for many subjects, which, in the early days, if they existed at all in the curriculum, appeared only in a very elementary form. Thus for years there was a tendency to make way for other subjects in the curriculum by abbreviating the work in drawing, surveying, and perhaps some other subjects. That struggle is probably still going on, although, as I am about to mention, a recent study has shown that there is evidence that surveying is receiving increased attention.

While teachers of engineering are not as yet paying very much attention to psychology and pedagogical methods, they have an active organization in The Society for the Promotion of Engineering Education. That Society has a special Committee on Surveying and Geodesy. This Committee has been active during the last two or three years. It has sponsored special programs at annual meetings and last summer a special conference on surveying of two-weeks' duration was held under its auspices at the Summer Survey-Camp of the State College of Iowa in northern Minnesota.

At the annual meeting of the S.P.E.E. in Cambridge last June a paper on recent trends in surveying instruction by Professor W. H. Rayner was presented. This paper was based on a discussion of the replies to certain questions on the questionnaire sent to about 175 colleges conferring engineering degrees. There were 80 replies. Among the conclusions summarized by Professor Rayner was that surveying has made substantial gains, with the subject of horizontal control receiving the most attention because of the increasing use of plane coordinates and the growing demand for precision in cadastral surveys. As a result of this increased interest in horizontal control, a number of topics are receiving increased attention, such as base-line measurement, astronomy, transit-traverse, geodesy, and errors of observation. The subject of photographic surveying is also receiving greater recognition.

It was my privilege to be able to attend the Conference of Surveying Teachers last summer. There were in attendance about 31 teachers of surveying representing some 26 institutions. A number of different papers were presented and were used as the basis for round-table discussions. After rather thorough discussions of different phases of the subject, committees were appointed to draw up outlines for courses in elementary surveying, advanced surveying, route surveying, and photographic surveying. These reports have been printed in the December issue of the S.P.E.E.'s Journal of Engineering Education. I believe it is unnecessary to comment upon them here since their purpose was largely to give what should be included in minimum courses.

Two sessions of the Conference were also devoted to the discussion of surveying instruments with representatives of two leading instrument companies present. It was brought out that American instruments are made to meet the demand of American surveyors and that the instrument-makers are willing to meet the demands of the users and to provide the features and attachments requested. Simplicity of design and sturdiness seem to be popular features with American users.

I mention this Conference to show that engineering teachers are devoting attention not only to methods of teaching, because these were discussed at the Conference, but also to the scope and content of the courses. My own impression, after attending this Conference, is that geodesy in an advanced form is not likely to be taught very thoroughly in many institutions in this country, although the elements will be, with particular attention to base-line, triangulation, and rectangular-coordinate systems.

I have not made an attempt to investigate what might be called the collateral subjects such as geology, mathematics, and physics. Any one to become thoroughly grounded in geodesy will of course need a thorough training in the subjects mentioned.

In conclusion it may be said that the results of our investigation of civil-engineering curricula, in so far as they relate to geodesy, are largely negative, for the study shows that geodesy is not widely taught in an advanced form, although its elements are generally included in the courses in advanced surveying. There is some excuse for this state of affairs in the overcrowded condition of the four-year engineering course.

On the other hand, there are signs that surveying is becoming of more interest and that it will be better taught. The extension of the rectangular-coordinate systems in the States, the greater stress that is being laid upon precision in city and cadastral surveying, and a more extensive use that is being made of photographic surveying, are factors in promoting an interest in higher surveying and geodesy. The concerted action of teachers of surveying as evidenced by their recent and current activities should also do much to insure that surveying and geodesy receive their requisite share of attention in engineering curricula.

Cornell University,
Ithaca, New York

THE IMPROVEMENT IN TIME-SERVICE AND BROADCASTING

J. F. Hellweg

Eight years ago the Naval Observatory was broadcasting time three times daily, with an average error of from 0.03 to 0.05 second. Like all other time-determining observatories in the world, time was determined by visual observations of the transits of stars over the meridian of the Naval Observatory.

During the past eight years the Naval Observatory has gained a reputation for its accuracy and its cooperation. One of the European authorities, in an official report listing the various fundamental observatories in the world in accordance with their accuracy, rated the United States Observatory 100 per cent, while the various observatories in Europe and South America were tabulated with varying percentages of accuracy. The nearest competitor was Potsdam shown with 100 on its right-ascension work, but only 70 per cent on its declination-work. This institution received 100 per cent on both. About that time, the French Government also gave us an excellent rating on our time-service.

The symposium of the American Geophysical Union, held in 1931, is recalled very distinctly. During that symposium the various representatives expressed opinions all pointing to the necessity of more frequent and more accurate broadcasts. Believing that it was merely a matter of obtaining authority, I promised to increase the broadcasts and then found considerable difficulty in obtaining the authority. Without referring the matter to anyone, the broadcasts were jumped to six daily, but we could not go beyond that, inasmuch as a further increase would necessitate increasing the staff of astronomers, and the Navy did not have additional funds nor did it consider favorably increasing the staff. I then tried to get more young officers ordered here for duty in time-service.

In 1932, we were still determining time visually and were broadcasting time six times daily under the immediate supervision of the astronomer on watch. The immediate preparation for a broadcast consisted of a comparison of the broadcasting clock with the master-clock in the vault, a measurement of the difference, the determination of the duration of the application of an electrical current to a solenoid either to retard or accelerate the pendulum. This was followed by a second chronograph-run to determine how accurately the error had been absorbed, and, finally, the broadcast had to be made. This procedure usually occupied 16 to 17 minutes and the time for six broadcasts, plus the other numerous duties that the astronomers had to perform, gave them a full day.

The next improvement, after doubling the number of broadcasts, was the construction of a very large vault. During the construction of that vault a letter was received from a scientist in Europe stating that he had seen a notice in a foreign paper that the Naval Observatory was contemplating the construction of a large and more efficient type of vault for its master-clocks. This letter advised against too great optimism, stating that one European country had spent an enormous sum of money, and the vault was not a success. The letter ended with the advice not to expect too much and not to claim too much.

We proceeded with our plan, however. It may be interesting to you to learn that some of the details of that vault are a direct result of practices on shipboard and at powder-factories. In the Navy we have to guard against moisture in the lower parts of the ship, particularly the powder-magazines. The Navy has developed a special preparation which works very effectively.

When we built our outside wall, we used this hot preparation and painted the inside face of the first fleet of bricks and then slapped on regular builder's felt and again applied the Navy's preparation, thus giving us an insertion between the inner and outer fleet of bricks which has done much to insure the absolute dryness of the structure.

After having completed the entire outside structure, we stepped in 30 inches and built an entirely separate structure completely contained inside the first. This arrangement gave us a 30-inch dead air-space all around and over the inner structure.

Down through the roofs of both of these compartments we let a 11-inch pipe. The piers on which the clocks are mounted were all placed in what is known in powder-factory parlance as "dead angles of all adjacent structures," the idea being to reduce, if not eliminate, the possibility of the influence of one pendulum on the adjacent pendulums swinging in the same plane.

Down through the 11-inch pipe, let through the roofs of the two structures, we installed a periscope taken from one of our submarines. This periscope serves a double purpose. First, it permits the thousands of visitors, who come to the Observatory annually, to see the clocks which keep the Nation's time, and, second, and more important from our point of view, it makes it possible for all clocks to be observed daily.

From the time that vault was constructed, constant pressure and constant temperature have been maintained even under the varying conditions existing here in Washington. The vault has been in continuous operation since its construction.

Subsequent to the completion of this structure, several letters were received from the gentleman in Europe who had warned me not to be too optimistic. His letters were deliberately not answered, particularly those in which he requested detailed information of the vault and all apparatus. Finally, he asked for blue-prints, then for complete working drawings.

About four years after the structure was completed, he unexpectedly appeared in my office and introduced himself. Naturally, he was asked the reason for his visit and he replied, "To see your vault." He was taken to the building and actually put in the vault for about 30 minutes. When he was finally allowed to come out of the vault he apparently was deeply impressed. His only remark was, "'Strordinary." Since that day that gentleman has paid us one other visit and has again spoken in the highest terms of our vault and the ideal conditions under which the precision timepieces operate. As he put it, "Naturally, they operate perfectly under such perfect conditions."

During a public address this gentleman stated that the Naval Observatory was 150 years ahead of all the others in the world in the determination and maintenance of accurate time. He expressed a desire to be licensed as European representative for all our apparatus.

One of the enthusiastic members of our staff, Mr. Willis, completed his plans for the photographic determination of time. The Naval Observatory has long had a photographic zenith-tube which had previously been used for the determination of the difference in latitude. Mr. Willis made modifications which permitted the use of the instrument for time-determination, thereby eliminating the variable personal errors of the observing astronomer. Since that very efficient improvement, our time has taken another boost in accuracy.

Coincident with this improvement in determining time, we were developing an apparatus which would permit us to broadcast our time automatically. The scientists of this country were insisting on receiving more frequent and more accurate time. Our photographic determination was answering the second of these two requirements.

The present apparatus used in broadcasting time answers the first of these requirements and has a triple personality. First, it is a crystal-controlled clock; second, it is its own chronograph; and, third, it is an automatic time-broadcaster. The crystal-controlled oscillator was made for the Naval Observatory by the General Radio Company. The quartz-crystal has a frequency of 30,000 cycles per second. This quartz-crystal is enclosed in a double temperature-controlled compartment. The 30,000-cycle output is fed directly into two submultiple generators. One of these generates a frequency of 6,000 cycles; the other, 5,000. They are combined and fed into a detector-and-amplifier unit. This amplifier-unit takes the difference of the two frequencies--that is, 1,000 cycles--and amplifies it to the full output of three standard No. 41 tubes. The resulting magnified power is then used to operate a synchronous motor. The field-coils of that motor can be rotated. This permits the application of corrections to the device without interference with its operation. If the field-coils are rotated through 0.03 revolution, a correction of exactly 0.003 second of time will have been applied. The shaft of this synchronous motor is geared to the shaft which is made to rotate at exactly one revolution per second. Mounted securely on this shaft is a glass disc whose periphery is etched with 1,000 divisions. Inasmuch as the shaft with its glass disc revolves exactly one revolution a second, each of the divisions on the periphery of the glass disc therefore measures 0.001 second. Below this glass disc is carried the master-cam and the whole program-mechanism which produces the time-signal, beat by beat with its identification-signals, end of the minute-silences, etc.

There is a neon-light so placed adjacent to the revolving glass disc that the flashes of the light, which are caused by one of the standard clocks in the vault, can be read through the periscope. A clock-key with a long shank can be admitted through the side of the apparatus and the field-coils can be rocked right and left. The flashes of the neon-light can be seen through the revolving glass disc, showing the corrections as they are applied to the clock in increments of 0.001 second.

When the broadcasting clock has been set to the exact 75th meridian time based on 30 or 40 meridian-transits of stars obtained the night before, the apparatus is then ready to carry on, and the astronomer is free to go about his other duties. Every hour, the apparatus automatically signals to Arlington. The man on watch in radio central throws a switch which puts this automatic broadcaster in command of the broadcast. From that time on, the apparatus becomes an electrical robot. It sends out every audible signal, operates the identification-signals by means of the various silent-beats, and at the end of the broadcast makes the zero hour-signal and then cuts off; waits 55 minutes and then repeats, and does this 24 hours a day, 365 days a year.

One of the great advantages of this automatic broadcasting apparatus is that duplicates can be installed at distant points. The reception of signals by radio then operate the flashing of the neon-light instead of the beats of our standard clocks in the vault. The proper corrections can then be applied until the error due to radio transmission has been absorbed, and when that is complete, the distant broadcaster can then go on the air and rebroadcast simultaneously with the Naval Observatory the exact time with an accuracy of within 0.001 second of the standard time received over the radio from Washington.

Where before it required 16 or 17 minutes for an astronomer to set the broadcasting clock every time a broadcast of time-signals was made, now the astronomer on watch sets the clock at 8 a.m. in about five or six seconds. They check it at night just before they go home, and the apparatus runs automatically all the time.

Our original broadcaster was placed in commission on May 7, 1934, and we ran it for six-months' endurance-test without any public announcement.

Some rather funny occurrences developed. An amateur scientist, whose bedroom is fitted with all sorts of gadgets, snapped on his short-wave receiving set one morning just before eight o'clock and picked up an unknown signal. He became so excited he stopped shaving and devoted his time to checking the broadcast. The next day he was all set for it and checked it again. After several days he wrote a letter to Washington asking whether we were aware of this broadcast and, if so, did we know where it came from. Immediately upon receipt of my answer he appeared in Washington.

A couple of months later, one morning a very smart-looking Chinese gentleman appeared in my office and told me the following story. He said that some months before he had picked up an unknown signal in Nanking, China. Remember, Nanking is many miles above Shanghai, up in the interior of China. After having checked this signal for some time, he decided that such an invention must be a German one, so he packed his bags and proceeded from Nanking to Berlin. There he was unable to get any information about the invention. He then proceeded to Paris where he was equally unsuccessful, and then on to London. In London he was told that he would have to go to Washington, D. C. That gentleman remained with us for nine weeks. When he returned to China, it was his intention to have the Chinese Government install a complete duplicate of our set, including everything: Vault, P.Z.T. for photographic determination of time, and the automatic broadcaster.

Unfortunately, the Japanese invasion compelled the cancellation of all plans. It may be fortunate that his original plans never matured; otherwise, everything completed would have been destroyed in the bombardment of Nanking.

Our present situation is as follows: We can divide time and set our broadcaster much more accurately than we can determine time, which is the neck of our bottle. Until we can obtain sufficient additional funds to further develop our plans for increased accuracy, our present record must stand. Also, it will be necessary to improve the running of the Shortt clocks and the crystal oscillators before any marked improvement can be made. Our average error over a long period is 0.012 second. Previous to 1930 it was 0.038 second. Paris now checks us with an error of 0.009 second. I hope we will soon have signals with only 0.001 second going out 24 times a day 365 days a year. It is a long step from three times daily to 24 times daily and from approximately 0.04 second to 0.012 second, but we will not be satisfied till we get that error down to 0.001 second.

U. S. Naval Observatory,
Washington, D. C.

THE SEA-LEVEL DATUM OF 1929

Howard S. Rappleye

Precise or first-order leveling, as it is now called, was first undertaken by the United States Coast and Geodetic Survey in 1878 when field-work was begun on the line of levels of high precision which was to follow the general trend of the Transcontinental Arc, an arc of triangulation extending across the United States from Chesapeake Bay to the Golden Gate and following approximately the 39th parallel. From this single-line beginning, the control-leveling of the Survey has been extended until the fundamental leveling net now includes more than 260,000 miles of first- and second-order leveling. About two-fifths of this is first-order leveling, the remaining three-fifths being second-order or single-line leveling.

The first adjustment of the leveling net was undertaken in 1898. The results were published in Appendix 8, Report for 1898. This adjustment included not only the precise leveling by the United States Coast and Geodetic Survey but a large amount of leveling of similar character which had been run by the Corps of Engineers, the United States Geological Survey, and other organizations. At that time practically all of the leveling in the net was in the region lying east of the Mississippi and north of the 39th parallel, and in the Mississippi River and Missouri River valleys. Connections to tidal stations at Boston, Massachusetts; Sandy Hook, New Jersey; Old Point Comfort, Virginia; and Biloxi, Mississippi, had been made. These were the only points at which mean-sea-level determinations of sufficient accuracy to hold in the adjustment had been connected to the control-net. It is true that a line had been run from St. Augustine to Cedar Keys, across the Florida Peninsula, but since this line had not been connected by spirit-leveling with the rest of the net, it need not be considered at present.

About 1900, an improved type of leveling instrument, designed and constructed in the shops of the United States Coast and Geodetic Survey, was put into use. This instrument proved to be so efficient that it gave considerable impetus to the leveling activities of the Survey. Additional field-work was undertaken on an ever-increasing scale. By 1903 so many new lines had been run that a second adjustment of the control-leveling net was undertaken. The resulting elevations were published in Appendix 3, Report for 1903.

In 1907 the net had been so greatly extended that a third adjustment was undertaken. In this case, however, a considerable portion of the net, especially in the eastern half of the country, was held fixed as adjusted in 1903. The adjustment of 1907 cannot, strictly speaking, be called a general adjustment of the net.

By 1912 the net had again been so much expanded that another adjustment was deemed advisable. Again certain portions of the results of the adjustments in 1903 and 1907 were held fixed in that of 1912, and this adjustment should not be considered as a strictly general one.

At the time of the adjustment of 1912 the net included about 30,000 miles of leveling of high precision. Most of this leveling had been done by our own parties, but the net still included a large amount of leveling by other organizations. The leveling then extended from the Atlantic to the Pacific, and from the Gulf to the Canadian border. It was connected with tidal stations at Boston, Massachusetts; Sandy Hook, New Jersey; Annapolis and Baltimore, Maryland; Old Point Comfort, Virginia; Morehead City, North Carolina; Brunswick, Georgia; Biloxi, Mississippi; Galveston, Texas; San Diego, California; and Seattle, Washington. Still the line from Cedar Keys to St. Augustine need not be considered since this line had not yet been connected in any way with the rest of the net by spirit-leveling.

In 1927 we made a special adjustment of the net, as it then existed, for certain theoretical purposes which time will not permit us to discuss in detail.

By 1929 we had included in the net about 45,000 miles of first-order leveling mostly run by our own parties but still retaining the previously accepted work of other organizations. It was becoming increasingly difficult to fit the new leveling to the existing adjusted work. It sometimes became necessary to apply corrections to the new leveling which were all out of proportion to the accuracy of the field-observations.

A complete and general readjustment of the net was decided upon. In order to strengthen the net further it was desirable to include also the Canadian first-order leveling in the adjustment. The officials of the Geodetic Survey of Canada cooperated with us by placing at our disposal the necessary data to permit us to include, with the first-order leveling of our own net, another 20,000 miles of first-order leveling in the Dominion of Canada.

The 1929 General Adjustment, as it has been known from the time it was well under way, was a strictly general adjustment of the combined first-order level nets of the United States and Canada as they existed at that time, and included approximately 65,000 miles of first-order leveling. This net was connected to the observed mean-sea-level planes at 26 tide-stations on the Atlantic, Gulf, and Pacific coasts of the United States and Canada. Five of the tidal connections were in Canada--three on the Atlantic Coast and two on the Pacific Coast. The remainder of the mean-sea-level connections were at tide-stations on the coasts of this country.

It was hoped that the General Adjustment of 1929 would result in eliminating discrepancies in the elevations of bench-marks along the boundary between the United States and Canada, and thus do away with one of the troublesome features of our work. However, it so happened that the Geodetic Survey of Canada had only recently completed an adjustment of its net and had published its results. As the ink was hardly dry on an excellent series of nine publications, including the results of a rigid adjustment of approximately 20,000 miles of leveling, the Geodetic Survey of Canada could not see its way clear to upset all of the published results in order to reconcile the relatively slight discrepancies along the border. The elevations published by the Geodetic Survey of Canada and those resulting from our 1929 General Adjustment of the combined nets did not disagree, at any point along the international boundary, by as much as one-half foot.

We finally decided to adopt the results of the 1929 General Adjustment of the combined nets as standard for use in the United States, and the work of recomputing the elevations of all of the bench-marks in the fundamental net was begun. This decision made it necessary for the United States Geological Survey to recompute all of its leveling in order to conform to the results of the 1929 General Adjustment. Both organizations are still at it and the end is not yet.

The normal increase of field-work and the necessity for recomputing the elevations of all the old bench-marks combined to place us in an unenviable position at the time the field-work of leveling was expanded enormously as a result of the allotment of considerable sums of emergency funds to field-parties for further leveling. The leveling net, which, in 1912, included 30,000 miles of leveling, had increased to about 65,000 miles at the beginning of the period of greatest activity. By August, 1935, when field-work was abruptly terminated, the net included over a quarter of a million miles of leveling.

Had we been able, in 1929 and 1930, to foresee the enormous expansion of the work which later took place, it is possible that the general adjustment would have been delayed until the first-order leveling had been completed. A general adjustment of the completed first-order net would give, from a theoretical standpoint, very nearly a perfect control for all future leveling in the United States, at least in so far as we can visualize future needs at the present time. However, during the period of the greatly expanded field-program, practical considerations made it imperative that the results of the 1929 General Adjustment should be held fixed, and that new leveling should be adjusted to the previously adjusted portions of the net as rapidly as possible.

At the time of the enormous and quite sudden expansion of field-work, we were in the midst of a general readjustment of all our old leveling. The increase of personnel in the office did not at any time keep pace with the increased field-activity. There is always, even under the best conditions, a considerable lag between the completion of field-work and the publication of results. The result was that, when in August, 1935, field-work was abruptly terminated, we found ourselves possessed of an undreamed of accumulation of leveling records and field-computations. We had just begun to make appreciable headway in straightening out the accumulated work when the extra personnel in the office was also dropped. You can readily imagine that, had it not been for some of the field computing-parties, since kept alive to relieve unemployment in other cities, we would have been swamped.

Fitting this gigantic amount of additional leveling to the net, as adjusted in 1929, has necessitated a large number of supplementary adjustments. The studies preparatory to these adjustments have disclosed a number of unsuspected discrepancies of troublesome size. Where new lines tie to those run several decades before, we sometimes find discrepancies which require additional leveling to be done before a satisfactory adjustment of the new work to the old can be accomplished.

During the period of increased activity we had an opportunity to do some releveling in regions of known seismic activity. Real displacements of bench-marks resulting from this cause were uncovered and added to the existing confusion. The now more or less famous San Jose Net has added to our difficulties. Repeated relevelings of the net covering the San Jose Region prove that the maximum settlement, in the center of this region, is nearly six feet since 1906, with practically all of the settlement occurring since 1920.

An earthquake in the vicinity of Kosmo, Utah, caused us to rerun a portion of the old line originally run from Brigham, Utah, to San Francisco, California, and disclosed movement of bench-marks, as a result of the earthquake, up to a maximum of about 0.4 meter.

The earthquake of March 1933, which disturbed the Los Angeles - Long Beach Area, came almost on the heels of a complete releveling of the level-net in that section of California. The following winter we releveled the same lines, and added two more to strengthen the local net. Movements amounting to several inches were disclosed.

Earth movements of a few inches may not seem to be surprisingly large but, from the standpoint of the adjustment of the fundamental leveling net, they are quite serious. Movements of an inch can spoil otherwise fine circuit closures, and when the movements attain a magnitude of several feet, as in the case of the settlement around San Jose, the combination of new and old leveling becomes difficult, if not impossible. In fact, the settlement close to San Jose has been so rapid, at times, as to affect seriously our circuit closures when we returned to close a circuit on marks leveled over only a few weeks previously.

Another feature of the adjustment of our vertical-control net has developed with the increased activity. We have connected with a large number of new tide-stations. Mean sea-level as observed at each of these points should be held fixed in adjusting the new leveling to the previously adjusted work. The 1927 Special Adjustment and the 1929 Special Adjustment (distinct from the 1929 General Adjustment) have shown, beyond reasonable doubt, that the actual mean-sea-level surface, as determined by means of continuous tide-gage observations, departs from its theoretical form by amounts somewhat larger than had been suspected before these studies were made. Our subsequent experience has been that this irregularity of the actual mean-sea-level surface is not only real but that the irregularity is even greater than was indicated by the two special adjustments. Variations in the height of the mean water-level at stations fairly close together are often large enough to cause excessive rates of correction if we hold the observed mean-sea-level planes at zero in the adjustment of the leveling.

It should be remembered that tide stations cannot always be established at points where open-coast conditions prevail. Also, it is necessary to maintain an automatic gage in continuous operation for at least a year at a station before a sufficiently close approximation to mean sea-level at that station can be obtained. Practical considerations required us to adjust and publish the results of our leveling in Oregon before entirely adequate connections to mean sea-level had been obtained. The completion of a tidal survey of the Oregon Coast, shortly after we had published the results of our geodetic leveling in that State, disclosed discrepancies between the observed mean-sea-level surfaces and the adjusted results of the geodetic leveling, at points along the coasts of Oregon and northern California.

In adjusting the leveling in Oregon the observed mean sea-level at Garibaldi was held fixed at zero in the adjustment, since it was believed that this station fairly well represented open coast conditions. Later, when the tidal survey of the coast of Oregon resulted in the connection of 12 new tide-stations with the geodetic leveling, discrepancies ranging from 0.75 foot at Crescent City, California, to 0.65 foot in the opposite direction at Tongue Point, Oregon, were found to exist between the elevations referred to the local mean-sea-level datums and the elevations based on the geodetic datum, defined by the geodetic leveling as adjusted and published.

While the trend of the discrepancies follows the slope (upward to the north) indicated by the 1927 and 1929 Special Adjustments, the minor irregularities are more pronounced than we had previously believed possible.

Theoretically, to get the best possible results, a net should be readjusted every time new leveling or a new tidal connection is added to it. The engineers and surveyors throughout the country, from the topographers of the Geological Survey to the engineers practicing individually, are wailing and gnashing their teeth because we already have made so many different adjustments. I hate to think of the names we would be called if we were to readjust the net again in this generation.

The adjustment of the control-leveling net is another illustration of the warfare between theoretical and practical considerations. Obviously, with a large percentage of our leveling data still unadjusted, with the major portion of our results available only in form of manuscript, and with an ever-increasing demand for the results of our leveling, we cannot undertake another general adjustment of the net, no matter how much it might help us from a theoretical standpoint. To supply usable elevations to engineers and surveyors throughout the country is, at present at least, the main objective toward which we must work, even at the expense of some loss in the quality of the finished product. With this picture in mind you will probably more fully understand the reasons for the short but important recommendation which was presented to and approved by our Director, in November, 1937, as our policy with regard to further adjustment of the fundamental vertical-control net. I quote the relevant portion of the memorandum.

"It is therefore recommended that elevations based on the 1929 General Adjustment be held fixed with the possible exceptions outlined below, and that the datum be officially designated as 'sea-level datum of 1929.'

"(1) The case where new leveling discloses blunders in the old work and necessitates readjustment of a limited portion of the net, in order to absorb the blunder.

"(2) The readjustment of limited portions of the net, as a result of earthquakes, or other earth movements which are proven to have taken place.

"(3) The question of the introduction of new tidal stations and connections into the net should be handled by fitting the new work to the net in the usual manner, but not necessarily holding the tidal-datum planes in determining the geodetic elevations. We must either keep readjusting the net, especially along its edges, or else admit that an actual difference exists between the geodetic datum and the local tidal planes. As we cannot keep readjusting the net, the only alternative seems to be to admit differences between the geodetic datum and local tidal planes."

U. S. Coast and Geodetic Survey,
Washington, D. C.

ERRORS--ACCIDENTAL AND OTHERWISE

Hugh C. Mitchell

In a paper presented before the Philosophical Society of Washington in 1882, Mr. Doolittle of the United States Coast and Geodetic Survey divided errors of observation into two grand classes: Instructive errors and uninstructive errors. When I came across this classification some weeks ago, it was wholly new to me. It appealed to me so strongly that I have kept it in mind while preparing this paper, as a guide, and largely as a text for the paper as a whole. Certainly, any study of errors of observation, whether along purely theoretical lines or in the discussion of a series of actual observations, should have a twofold objective: The ascertaining of the quality of a result, and the determining of ways for improving a result.

This paper was written from the viewpoint of the engineer. With the exception of several illustrations, there is nothing new in it. I have followed the example of the poet:

"An' what he thought 'e might require,
'E went an' took--the same as me."

Wishing to use actual observations for illustration purposes, I have selected examples from the field of surveying. However, any basic conclusions reached in a study of the occurrence and behavior of errors in measuring a base-line or in observing an angle, may readily be adapted to other types of physical measures.

An error has been defined as the difference between an observed value of a quantity and the true value of that quantity. It is manifestly impossible to use this definition in a practical discussion of errors, since the true value of a quantity can not be known, so it has become the practice to broaden the meaning of the term and use it, with qualification, to represent the difference, in whole or in part, between an observed value of a quantity and the most probable value of that quantity. In its broader sense, error is used as a substitute for, or in connection with, the word residual. So, even in technical literature, we find the words error, residual, and residual error used rather freely. Perhaps we should limit our use of residual to concrete values obtained in actual computations.

For theoretical study as well as for practical computation, errors are generally classed as accidental errors and as systematic errors. There is also a third group of quantities which sometimes masquerade as errors. These are blunders, also termed mistakes. Blunders are sometimes so closely associated with errors in relative position to the most probable value that it is not practical to isolate them and they are retained. Thus a blunder may sometimes have a direct effect on the computation of a most probable value of a quantity. And sometimes authentic errors occurring in regions where blunders may be expected, are placed under suspicion, subjected to an artificial test, and cast out.

A blunder, if quite small, may cause considerable embarrassment; if large, it can usually be recognized and rejected. A blunder is best defined by examples: Dropping a tape-length in a traverse; reading an angle an even degree off; reading or recording the wrong number of feet in a measured distance--the list may be made indefinitely long. Blunders have no place in the theoretical discussion of errors; they are supposed to have been eliminated before the discussion commences.

Systematic errors are sometimes called constant errors, because in particular cases they remain practically unchanged, and sometimes persistent errors, because of their characteristic for recurring unless care is taken to eliminate or counteract them. The preferred term, systematic error, includes all errors of this class.

A systematic error is one which under similar conditions will tend to recur in kind, and under identical conditions will recur in size. There are many common examples which will illustrate this: The correction to the nominal length of a tape at the temperature of standardization; changes in length with variation in temperature; and personal equation in making contact between tape-graduations and ground-marks. But few examples can be cited that are wholly systematic errors since they will include the small--I might say minute--variations from constancy which are peculiar to the separate observations, that is, accidental errors.

Systematic errors may be grouped in three fairly distinct classes: (a) Errors which are inherent in the apparatus used, as the difference between the nominal length of a tape and its standard length; (b) errors which result from the effect on the apparatus of extraneous conditions, as changes in tape-length with changes in temperature; and (c) errors which are dependent upon the observer's relationship to the work, the so-called personal equations.

The methods of treating these various groups of systematic errors, and removing their effects from a result may readily be formulated and applied when once their presence is known and their true character recognized. Such methods include determination by standardization and calibration of the apparatus used, elimination by instrumental adjustment and by cancellation in observing programs, determination with auxiliary apparatus.

Seldom, however, will the observer encounter a case where a single type of error prevails. In laying down a tape on a base-line, the length error of the tape is known by standardization, but that standardization is subject to various errors involved in its determination; errors in the bench-standard itself which come from errors in its standardization; errors of contact in placing the tape on the bench; errors in reading thermometers which are themselves subject to error of calibration; and so on, indefinitely. A family-tree of the errors which accompany a tape to a base-line would be very involved, and when onto it are grafted the errors which arise in the use of the tape on the base-line, the picture becomes very intricate indeed.

In any observational work, the various systematic errors will usually be removed by methods of standardization, programming, observation, etc., but some are not removed, and remain to mingle with the second and more interesting of the two main classes of errors, namely, accidental errors.

An accidental error has been defined as being composed of an indefinite number of very small elemental errors, plus or minus, the chance of any such elemental error being plus being the same as its chance of being minus.

Accidental errors are sometimes referred to as residual errors. They are the errors remaining in the separate observations after all determinable systematic errors and blunders have, as far as possible, been removed. Knowledge of their sizes and signs will come from comparison with the most probable values which they determine.

We can not know the definite causes of these elemental errors, except that they come from the many small imperfections in human operations, in machines, in physical conditions, even in psychological reactions. We can, however, know how accidental errors behave, and can chart the course of that behavior with reasonable accuracy.

From a mathematical study of accidental errors based on the theory of probabilities it has been determined that where there are a number of direct measures of a quantity, all made with equal care, the most probable value of the quantity is the arithmetical mean of the various measures. Also that plus and minus errors will occur with equal frequency and that small errors will occur with greater frequency than large errors.

We can study observational data by comparing the residuals obtained in their mathematical reduction with the errors resulting from a consideration of probabilities in a theoretical problem. Such a comparison is readily made by means of curves and graphs. The probability-curve is well known--at least in form--to all of us. Equally simple of construction and of the same form is the graph depicting distribution according to size of the residuals derived from the reduction of observational data.

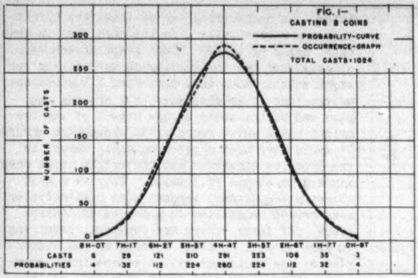

FIG. 1—
CASTING 8 COINS

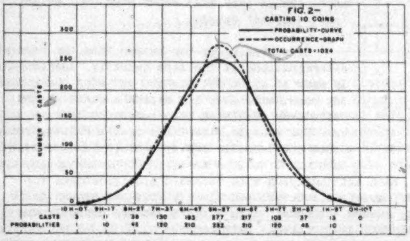

FIG. 2—
CASTING 10 COINS

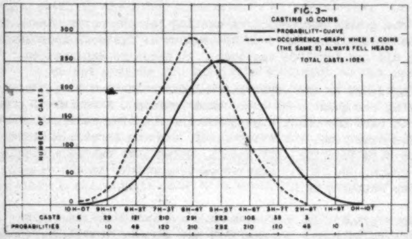

FIG. 3—
CASTING 10 COINS

Figure 1 shows such a comparison. The full line is the theoretical curve showing the probable occurrence of different combinations of eight errors, where each error is as apt to be plus as minus. This curve is symmetrical with respect to its highest point, which represents the arithmetical mean; plus and minus errors can be seen to be present with equal frequency; and small errors are more numerous than large ones. The dashed line is the graph obtained experimentally by casting eight coins in such a manner that each coin was as apt to come heads as tails. One thousand and twenty-four casts were made. It takes very little imagination to let each coin represent an elemental error, heads being plus, and tails minus. It is interesting to look at some of the actual figures involved. For instance, the probability of casting eight heads and no tails was four in 1024 casts; it occurred six times. The probability of casting four heads and four tails was 280; the occurrence was 291.

But from the coin-experiment was obtained an even more interesting exhibition than this direct comparison of a probability-curve with an event-curve. Ten coins were used in making these casts; two of these coins were polished up till they were bright enough to be readily identified, and while they were all cast together, separate records were kept of how the dull coins and the bright coins fell at each cast. A simple sum gave the actual figures for the occurrence of the ten coins at each cast. These gave an experimental graph (Fig. 2) of about the same agreement with the theoretical curve as was obtained with the eight coins.

But what I sought to show was the behavior of a group of errors, some of which were of the systematic type. The eight dull coins represented accidental errors, each of which was as apt to be plus as minus, that is, to fall heads as to fall tails. The two bright coins represented systematic errors, and were assumed to always come plus (heads).

In the illustration (Fig. 3), the full line represents the curve of theoretical probabilities for 1024 casts of ten coins, all being considered alike. The dashed line was obtained from casting ten

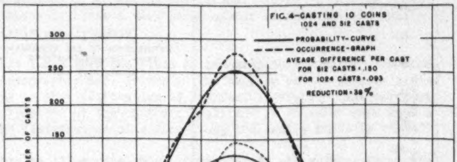

FIG. 4—CASTING 10 COINS
1024 AND 512 CASTS

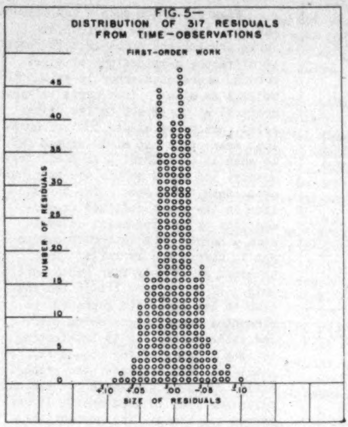
FIG. 5— DISTRIBUTION OF 317 RESIDUALS FROM TIME-OBSERVATIONS

FIRST-ORDER WORK

coins, and considering two of them (the bright ones) as always falling in the same way--in this case, heads up (plus). This graph agrees very closely with the probability-curve obtained for eight events shown in a preceding illustration, except that it is displaced well towards the plus end of the scale by the effect of the two bright coins which represented systematic errors. It would have been shifted an equal amount in the opposite direction had the bright coins been assumed to always fall tails. This is a good illustration of what happens when systematic errors remain undetected in a series of observations. If those errors are constant, recurring in size as well as kind, nothing in the discussion of the residuals will disclose their presence, though they will enter with full weight into resultant values.

In surveying it may happen that two separate determinations of the same quantity, independently made at different epochs, but with corresponding care, will show two separate event-graphs, definitely displaced with reference to each other. For example, the files of the United States Coast and Geodetic Survey contain records of horizontal directions to triangulation-stations observed in full sets on different nights, each set complying with standard specifications for such work. The individual observations of a set will be grouped closely together around their mean, but the various means may differ by several seconds. Such differences are probably due largely to systematic errors caused by variations in lateral refraction.

Before dismissing consideration of the graphs obtained from casting the ten coins, I would like to show how the graph approaches the probability-curve as the number of casts is increased. The illustration (Fig. 4) shows this for 512 and for 1024 castings. It needs no explanation.

A graph representing the residuals obtained in the observational determination of a quantity affords an excellent means of studying the quality of those observations. A series of precise observations on a quantity, made with care and using high-grade instruments, will produce a tall narrow graph, its peak high above the base-line, its sides steep, and its terminals close in. On the other hand, observations made with inferior instruments, under less exacting conditions, will tend to produce a long flat graph, low in its highest part, and less smooth at all points and much more extended than for the better work.

Two graphs are shown. The tall narrow graph (Fig. 5) represents the residuals from time-observations for first-order longitude-determinations. No residual was greater than 0.10 second. The other graph (Fig. 6) represents residuals obtained from time-observations for second-order longitude-work. Its residuals run up into the tenths of seconds. One residual was greater than a half-second--it is not shown. Equally striking illustrations could be obtained from many other types of physical observations.

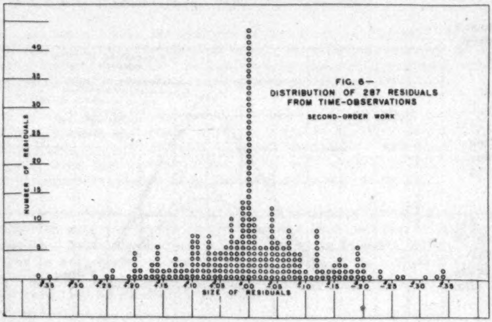
FIG. 6— DISTRIBUTION OF 287 RESIDUALS FROM TIME-OBSERVATIONS

SECOND-ORDER WORK

To give a paper on the subject of errors without some brief mention of criteria for the rejection of observations would be missing an opportunity for correcting what I believe is a rather common misunderstanding of a

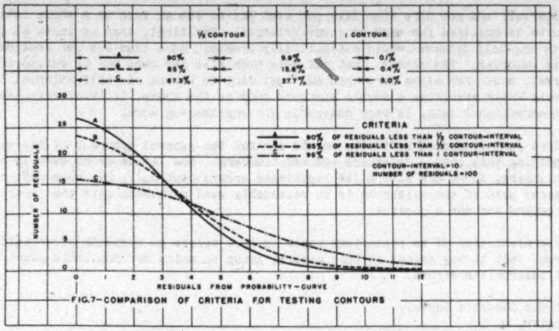

FIG.7—COMPARISON OF CRITERIA FOR TESTING CONTOURS

criterion. In the sense in which it is used to discuss observations in surveying, a criterion is simply a practical rule for insuring that no harmful blunders are retained in the observations. It does this by establishing limits beyond which no observations, good or bad, may be retained. But the rejection of an observation because its residual error falls outside a specified limit does not mean that we are sure such an observation contains a blunder, nor does retaining an observation because it is within the pale mean that it does not contain a blunder.

An observation which falls outside the limit may be a good observation, just as free from mistakes as any of its group. It is just this type of observation which produces what Mr. Doolittle termed an instructive error. Such errors (residuals) are important in showing that errors exist in the other observations of the series. It is from a study of such residuals that the computer may learn much about the observations--possibly more than he can from any other single residual of those observations that are grouped close around the mean.

I do not endorse Mr. Doolittle's comment on the use of a criterion, though it came from a man who was responsible for one of the most valuable and useful contributions in the field of practical computing, namely, the Doolittle method of solving normal equations, also called the "correlate-method." In addressing the Philosophical Society, Mr. Doolittle placed heavy responsibility on the judgment of the computer for the retention or rejection of observations. And he made the further statement: "If a computer resorts to the criterion when he feels that his own judgment is worthless, and only then, the criterion is harmless; since it is of little importance whether a decision is made by a worthless judgment or a worthless criterion."

As used in surveying, I look upon a criterion as an engineering device for promoting a balance between efficiency and accuracy and for establishing a consistent program of observing involving accuracy of results, and economic quantity and quality of observations.

A criterion which does not contradict theory in any essential way and is consistent with experience is necessary in any large program of surveying operations. There is nothing inconsistent with this statement, since theory involves a very large number of observations, while practice must be limited to a relatively small number.

My final illustration (Fig. 7) is taken from the field of topographic surveying. It seems a fair assumption that in making a topographic map, errors in the mapping of the contour-lines will follow the general laws applying to the distribution of accidental errors. This being so, if it is specified that a certain proportion of the contours shall not be in error more than a certain part of an interval, then the distribution of errors along the entire scale is fixed.

I have several probability-curves to illustrate this. These curves are without regard to sign, all residuals being plotted in one quadrant. Curve A shows the distribution of errors where a single specification requires that not less than 90 per cent of the contours check within one-half interval. This automatically limits to 0.1 per cent the number of contours which may exceed the one-contour interval.

But suppose that it is specified that at least 85 per cent of the contours check within one-half interval, and not more than five per cent may be out as much as a whole interval: A separate curve is required for each of these criteria. The first, that at least 85 per cent check within one-half interval, would automatically prohibit more than 0.4 per cent being out more than a whole interval. The second, that not more than five per cent may be out more than a whole interval, would not allow more than 32.7 per cent to be out one-half interval. Yet an engineering rule which specifies a double standard such as the above, if it be reasonably consistent with observational data, is very desirable for engineering work.

The first part of such a criterion should control the general accuracy of the work; the second is nothing more than a safeguard against blunders. And little or no harm is done if, with some blunders, there are also a few legitimate errors cast out. But care must be taken that the second part of the criterion is in reasonably good agreement with the first part--that it is a safeguard and not a control.

If the presentation of so elementary paper as this before an audience of scientists is an error, I trust that it may belong to that general group to which Mr. Doolittle years ago gave the name of instructive errors.

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Washington, D. C.

A STAR ZENITH-FINDER

Thomas A. Jaggar

The zenith-star locates a place in latitude and longitude, if accurate time is read at the instant of observation. The zenith-point on a star-map may be found by three-pointing other stars, by the method of the topographer. A timed observation, by eye, of the zenith-point among the stars may locate a practiced observer within 30 nautical miles. All navigational schools should train students to recognize declination-belts and meridian-belts among the stars. A student so trained, flying an airplane, could tell, by merely looking up at night, the half-degree belts of latitude between 19° and 22° north, from the south end of Hawaii Island to the middle of Kauai Island.

The experiment with an F. 1.9 camera directed to the zenith, one-minute exposure, super-sensitive film, photographed numerous stars, each one mm long on a 3 1/4- by 4 1/4-inch film. One minute of time is 15 miles of longitude. For finding the zenith-point among the stars, the camera was rotated in azimuth 180°, and exposed for a second time on the same stars, the epoch of the observation being the instant of rotation between the two exposures. Joining identical stars across the print for this instant with ruled lines, the intersection-point was the zenith. This was matched to a star-map for that instant of sidereal time Greenwich. The declination is the latitude. The difference of the right-ascension, from Greenwich, is the longitude.

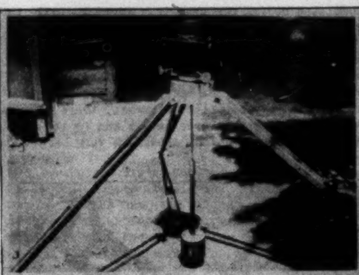
At sea or in airplane the essentials of the method are: (1) A basic course for the time of observation; (2) simultaneous azimuth for three or more stars; (3) corrected Greenwich-time reading for the epoch; and (4) a strictly photographic star-projection map, wherein azimuth-lines are of first importance.

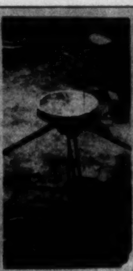
A shipboard plane-table (not illustrated) was a first experimental attempt to use a mirror, and rule star-azimuths with a hard pencil. The instrument is a rebuilt theodolite hung in gimbals. The telescope, converted to a sighting tube, is hung with a mercury-cup floating a star-mirror, and straight edges mark the star-azimuths on a ring-card. The ring-card, with stretched silk-threads on the azimuths, is set over the star-map. A voyage to Kauai was used for tests. The gimbals hold the star-image in the sights in a seaway, and observing a compass-card checks the steersman's holding of the base-azimuth. Preliminary trials gave errors of from six to 15 miles, and tests of technique are in progress. These involve reversals and repetitions for averaging out error, improvements in lighting, sighting and clamping for securing simultaneous record, and calibration of the mirror-leveling, for holding the zenith. It is believed that a simpler instrument will result, accurate within about ten miles on an airplane and possibly five miles on a steamship. Incidentally the star-map gives local time and total compass-correction directly.

The model now experimented with is a box low-power telescope with right-angle mirror so that ocular glass is vertical and objective horizontal. The essential feature is three cross-wires rotated by hand separately in the field of the ocular, and these are luminous. The telescope is

FIG. 1—ZENITH-FINDING TEST AT KEALAKEKUA WITH GNOMON AND GLASS-PLATE FLOAT IN MERCURY ON PLANE-TABLE, FEBRUARY 27, 1935, $21^{H}39^{M}$, HAWAIIAN STANDARD TIME; LATITUDE 19°25' NORTH, ERROR 6' SOUTH; LONGITUDE 155°28' WEST, ERROR 33' WEST (LARGE ERROR DUE TO TIME CONSUMED; LINES RULED ON GLASS AND PLATE SET OVER STAR-MAP)

FIG. 2—ZENITH-FINDING TEST AT KILAUEA WITH ZEISS CAMERA (F. 2.6, 4 m) ROUGHLY SET IN A ROTATION-TABLE WITH LEVELS OR GIMBALS, FEBRUARY 4, 1935, $20^{H}47^{M}$, HAWAIIAN STANDARD TIME (RIGHT-ASCENSION $5^{H}52^{M}41^{S}$, ERROR 15' WEST; DECLINATION 19°43'37", ERROR 17' NORTH)





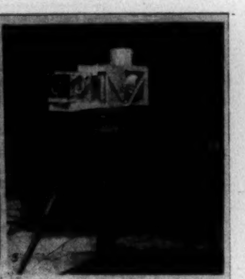


FIG. 3—ZENITH-FINDING CAMERA (F. 1.9 LENS)

FIG. 4—ZENITH-FINDER MODEL FOR SIMULTANEOUS STAR-OBSERVATIONS WITH FLOATING MIRROR IN MERCURY, 3-ARM PROTRACTOR TO CLAMP AND SET ON STAR-MAP, SIGHT-HOLE IN ARCH

FIG. 5—ZENITH-FINDER MODEL FOR SIMULTANEOUS 3-STAR OBSERVATIONS WITH 3-ARM PROTRACTOR, LUMINOUS MOVABLE CROSS-WIRES (ENTIRE OCULAR SYSTEM REMOVABLE TO SET ON PHOTOGRAPHIC STAR-MAP)

levelled by gimbals, the objective axis zenithal.

The cross-wires are set on three zenith-stars and time is read. The whole ocular system is removed and set over a photographic star-map marked with rectangular coordinates. Adjusted to the same stars, the cross-wires intersect at the zenith-point graphically obtained. Right-ascension and declination are thus read for the epoch.

National Park Service,
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ANNUAL REPORT OF PROGRESS OF THE GEODETIC WORK OF THE UNITED STATES COAST AND GEODETIC SURVEY

C. L. Garner

The work accomplished in the field of geodesy during the last year embraces all of the usual activities, though operations have been greatly diminished due to lack of funds. By far the greater part of the geodetic work in this country are the horizontal and vertical control-surveys which are so essential for mapping. Although strongly advocated for several years by many organizations--both Federal and non-Federal--as yet no mapping program has been adopted and it now appears doubtful if one will be adopted in the immediate future. Therefore, there is but little progress to report for the past year in the various stages of mapping looking toward the completion of the topographic map of the United States. Some mapping activities have been practically at a standstill while others have been making considerable progress, but none of the activities is coordinated from the standpoint of mapping alone.

Thus, while the control-surveys and the topographic work have been lagging, considerable attention has been paid by various agencies such as the Soil Conservation Service, the Agricultural Adjustment Administration and others in obtaining aerial photographs for various special problems of their own. It is understood that last year about 365,000 square miles of the country were covered with aerial photographs and that an even larger area is to be photographed this year. Thus a large section of the country is being covered by photographs which it is understood are made in accordance with specifications which make them usable for planimetric or other mapping purposes whenever the necessary control can be obtained, station-identification furnished, or the photographs reduced. Admittedly, a great part of the topography can be done by compilation in the office from the reduction of photographs, but this is not true of the control or of station-identification, etc. The major problem, therefore, is that of completing the control and distributing the data for general use when and where needed.

Table 1 shows the amount of geodetic work completed during the year from April 1, 1937, to March 31, 1938, as well as the grand total of all work completed.

Cooperative work--The accomplishments through cooperative undertakings during the past 12 months were not large in scope as respects the amount of funds involved, yet are large in scope when considered from the standpoint of the number of projects and the various organizations which have lent their support. Without doubt, in the mind of the writer at least, there is more thorough cooperation through many of the governmental agencies than ever before and in the final analysis it is the one principle on which all public-service operations should in very large measure depend.

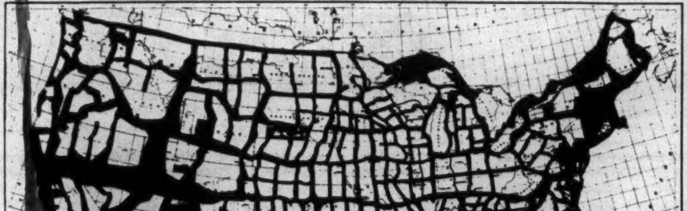

FIG. 1

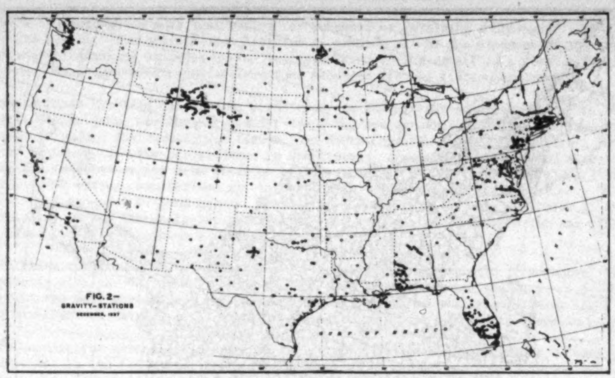

FIG. 2—
GRAVITY-STATIONS
DECEMBER, 1937

Table 1--Geodetic work, April 1, 1937, to March 31, 1938, and grand totals to March 31, 1938

Work	Completed		Grand total
	miles	stations	miles
Triangulation, first- and second-order	956		69,000
Leveling, first- and second-order	1,312		263,000
First-order base-lines	1		210
Second-order base-lines			54
Latitude-stations			840
Longitude-stations			691
Azimuth-stations			1,152
Gravity-stations		135	890

The United States Coast and Geodetic Survey has cooperated in geodetic operations during the past 12 months as follows: First- and second-order triangulation in Utah, Colorado, Wyoming, Arizona; Nevada has been carried on in cooperation with the Soil Conservation Service for the purpose of obtaining necessary horizontal control in regions of great relief where planimetric mapping is required. Funds for the work were furnished by the Soil Conservation Service and similar cooperative work is being undertaken in the same general region for the next several months. For this work the Soil Conservation Service required that stations be placed at certain definite intervals along the boundaries of the different areas where work was done and that no point within the area be more than 14 miles from a triangulation-station. Each of the stations was marked in a special and distinctive way for identification on photographs, and azimuth-marks were furnished at all of them for use in orientation.

Within the last month a first-order level party has been organized in New Orleans, Louisiana, to extend about 375 miles of first-order leveling between Bay St. Louis, New Orleans, and Baton Route, and from New Orleans to the lower Mississippi River in cooperation with the United States Engineers. Some of this work is rerunning of those lines which are many years old following the levee where the marks very likely have been buried to considerable depths and where the recovery will be difficult. During the past few years there has been a great demand for both horizontal and vertical control-data in the valley of the Mississippi River and its tributaries, mostly for flood-control surveys. This leveling in the vicinity of New Orleans will be another step in the completion of the net in that region as a means of furnishing sea-level datum on the 1929 adjustment of elevations. Present plans are for the continuation of this cooperative project until the level-net in the alluvial valley of the Mississippi River is completed. The Mississippi River Commission is furnishing the necessary funds.

A first-order triangulation-party is now engaged in the establishment of about 35 or 40 stations of horizontal control in close proximity to the boundary of the Metropolitan District of

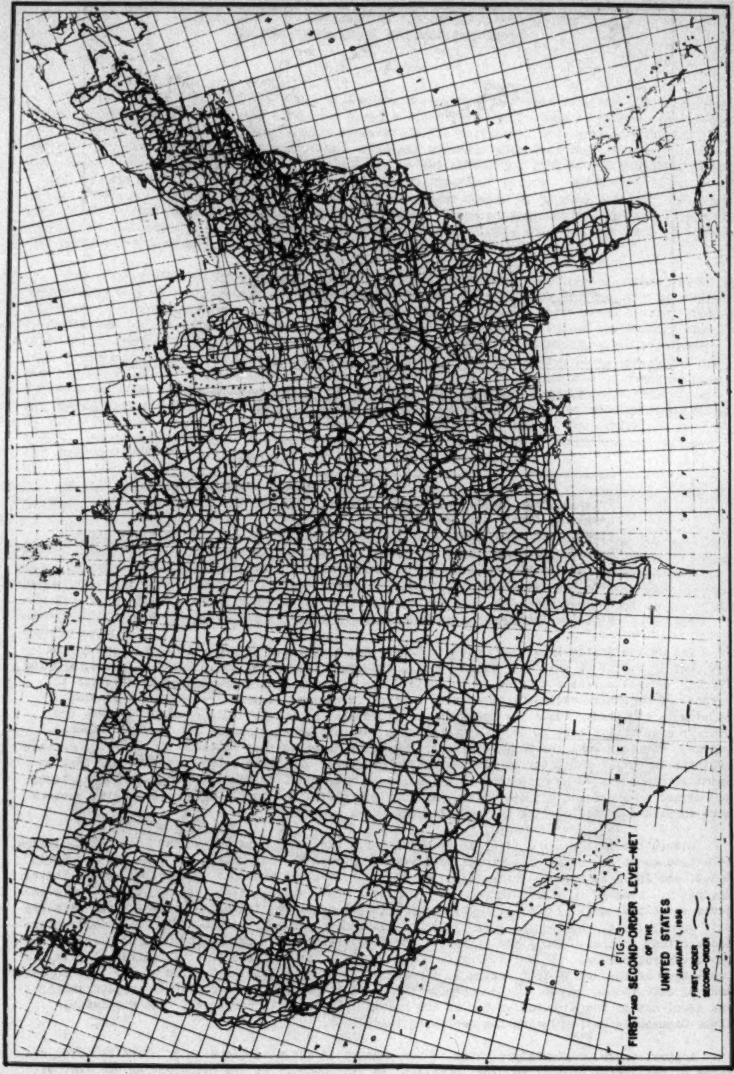

FIG. 3—FIRST- AND SECOND-ORDER LEVEL-NET OF THE UNITED STATES JANUARY 1, 1936

FIRST-ORDER
SECOND-ORDER

Baltimore in cooperation with that office for purposes of obtaining a tax-map of the City and the entire region. The Metropolitan District is meeting all of the expenses of the work and the United States Coast and Geodetic Survey is cooperating by the detail of a field-supervisor and the loan of such instruments and equipment as required for the field-operations.

Cooperation has been extended to Works Progress Administration organizations in Massachusetts, Connecticut, Westchester County, New York, New Jersey, Louisiana, Florida, Georgia, Arkansas, Oklahoma, California, Washington, and North Carolina. Technical advisers have been detailed to cooperate with the projects in Connecticut, Arkansas, Oklahoma, and Georgia. In Florida the cooperation has been extended to the detail of two engineers and a signalman for supervision of office-computations and the extension of first-order triangulation. Cooperation has also been extended to King County, Washington, by the detail of the full time of one officer and part time of two others to supervise first- and second-order triangulation and other operations connected with obtaining a tax-map for King County.

In Massachusetts an arc of first-order triangulation was completed from Boston, westward to a connection with the Oblique Arc along the New York - Massachusetts Boundary. A triangulation-connection was made across Cape Cod Bay, and the adjustment of the triangulation along Cape Cod and in southeastern Massachusetts was also completed by the Massachusetts Geodetic Survey.

In Connecticut triangulation was completed to delineate the Rhode Island - Connecticut boundary-line at the request of the Boundary Commission. An arc of triangulation was also run from Hartford, Connecticut, eastward to the Coastal Arc.

Triangulation in Florida was mostly in the northern part of the State and included an arc along the northwestern boundary with ties to the Gulf Coast triangulation. When this is finished, the 25-mile spacing of triangulation in the southeastern states of North Carolina, South Carolina, Georgia, and Florida will be completed.

With the exception of the triangulation mentioned, most of the work carried on by these projects has been second-order traverse and leveling intended to supplement the fundamental arcs of control-data.

Publications on control-data have been issued by the Works Progress Administration control-survey projects in Massachusetts and Florida, and are in process of preparation in Georgia.

In San Jose, California, cooperation was extended by the detail of two officers assigned to rerun lines of first-order leveling in a region where settlement has been going on for a number of years. This work has been repeated six times and it is understood that Works Progress Administration officials at San Jose plan to submit another project for repeating the work later on during this year. Previous reports indicate that maximum settlement is approximately six feet, and that settlement has greatly decreased in recent years.

Cooperation has been extended to the cities of Cleveland, Ohio, and Minneapolis, Minnesota, in detailing the services of an officer for part time in organizing a Works Progress Administration party for extending horizontal and vertical control in those regions. Both of these cities are now engaged in an extensive program to obtain tax-maps covering their entire districts.

Cooperation with other States mentioned has consisted chiefly of the loan of instruments and equipment and in such technical advice with regards to methods and procedure in the field and office as could be handled by correspondence.

The United States Coast and Geodetic Survey maintains, in cooperation with the Works Progress Administration, a computing office at 45 Broadway, New York, New York, which has been employing on the average about 70 men. Recently the personnel was increased. These men are supervised by personnel of the United States Coast and Geodetic Survey numbering from three to five, depending on the conditions, who have entire charge of the operations and keep close contact with the office in Washington as the work proceeds. By this arrangement it has been possible to process a considerable portion of the large amount of records remaining on hand at the close of our major field-operations in 1935. Incidentally it is interesting to note that even in a city the size of New York, it has not been possible to obtain as many people of proper qualification as was at first expected.

The United States Naval Observatory throughout the year has continued the distribution of time-signals, and has recently added four daily broadcasts which are very essential in making accurate observations of longitude, latitude, azimuth, and gravity.

We have also cooperated with the Comision Mixta de Limites of Guatemala-El Salvador in the observation of one astronomical station near the Pacific end of the boundary of those countries and later, at their request through the State Department, cooperated with the two countries in making observations at two additional stations in each country.

The usual cooperation has been received from the National Bureau of Standards in the standardization of base tapes and other equipment.

Non-cooperative work--During the summer of 1937 a leveling party of three units was engaged in running levels in the northwestern states in continuation of a program for completing a number of unfinished loops and circuits which could not be finished in 1935 and for the purpose of clearing up discrepancies which have developed in some of the lines of levels. A large area has been cleared up and the work will be continued to the limit of available funds.

The triangulation-party now engaged in the cooperative project in the vicinity of Baltimore, on completion of that project will transfer to New York, New York, and extend first-order triangulation along the Hudson River to Albany, New York. This will tie in and thus coordinate several horizontal control-surveys and greatly strengthen the net in that region.

Gravity and Astronomy--The triangulation done during the year has been so closely joined to the main net that no additional Laplace control has been required and therefore no astronomical work has been done in this country since my last report.

One gravity-party has been in the field most of the year and has determined 135 new stations and made redeterminations at about ten old stations. The party was in Arizona a year ago but was moved to the east coast soon afterward. Most of the work since then has been in four principal areas as follows: Rhode Island, Connecticut, Massachusetts, and New York (at locations selected by Professors Charles W. Brown and Chester R. Longwell); Eastern Pennsylvania and New Jersey (at locations selected by Dr. Meridith E. Johnson, Professor Benjamin L. Miller, and Dr. George W. Stose); Coastal area of Virginia and North Carolina (at points selected by Professors Wilbur A. Nelson, Benjamin L. Miller, and Maurice Ewing); Northern Florida and southern Alabama and Georgia (at points selected by Dr. Roy E. Dickerson and the State geologists of the three States involved).

During the late spring of 1937 considerable experimental work was done with the Meinesz gravity-apparatus and the Bell Telephone Laboratories crystal-chronometer to test the practicability of these instruments for land determinations. The same apparatus was also used to determine the vertical gradient of gravity in the Empire State Building in New York, New York. Dr. Ewing cooperated in making these tests.

The latitude-observatories at Ukiah, California, and Gaithersburg, Maryland, have been continued in operation.

Office computations and publications--Satisfactory progress has been made in the office processing of control-data, when the present lack of funds and personnel for carrying on any definite program is considered. It should also be understood that much of the field-work now completed cannot be reduced to final form for distribution until the unfinished triangulation- and leveling-loops and circuits are closed.

The following publications were issued during the past year:

Triangulation in Utah, Special Publication No. 209
Leveling in North Carolina, Special Publication No. 210
Triangulation in Wyoming, Special Publication No. 212

A large amount of material, both for triangulation and leveling, is awaiting publication and will receive attention as rapidly as funds are made available.

Conclusion--The value of control-survey data to many Federal, State, county and municipal organizations in the proper coordination of their surveying and mapping projects, for boundary-surveys and many other purposes, is well known. Many cities are now sponsoring control-surveys through the auspices of the local Works Progress Administration as relief-projects. These data are also used by a large number of private organizations and individuals in the conduct of their work dealing with boundary- and property-surveys and for many uses in laying out water-supply systems, sewerage, and numerous other problems of engineering peculiar to counties, cities, and towns. The large and ever-increasing number of requests being received at the office of the

United States Coast and Geodetic Survey from innumerable sources over the country attest to these facts in no uncertain terms.

Aside from this, the control-data, and particularly the astronomical and gravimetric data, are of great value to scientists and to people who are making researches into physical geography, natural resources, and many scientific problems. A program for obtaining in a reasonable time the control and the mapping which is an essential for so many operations vital to our economic life is a more pressing problem now than ever before.

U. S. Coast and Geodetic Survey,
Washington, D. C.

PROGRESS OF GEODETIC WORK IN MEXICO DURING THE YEAR 1937

Manuel Medina

In accordance with the general features which mark the six-year plan of the Government of the Republic for the Secretary of "Agricultura y Fomento," the Department of Geography executed the work corresponding to the said plan as follows:

Triangulation--The geodetic observations were continued on the triangulation established along the Rio Grande de Santiago-Tololotlan through the States of Jalisco and Nayarit and also on the chain of Los Altos, Jalisco. Twelve stations were established.

The Wild theodolite, on which an angular measure within 0.2 second can be estimated, was used on all these stations with the following results: The number of closed geodetic figures was six; the average angular closure of the triangles was 2".39; the average probable error of a direction observed 12 times was ±0".31; the average probable error of a direction observed 18 times was ±0".10; the average linear closure in units of the seventh decimal place of logarithms was 95.4; the average mean error of an observation was ±1".61, and the average mean error of the triangle closures of the figures (Ferrero's formula) was 3".92.

Astronomy--For immediate cartographic purposes reconnaissance astronomic stations were established in the State of Zacatecas using a sextant, on which an angular measure within ten seconds can be estimated, and portable radio equipment for the reception of the signals. The program of observation was as follows: Observation of equal altitudes of the Sun; observations of the circummeridian altitudes of the Sun; and reception of standard time-signals. Table 1 shows the results obtained.

Table 1--Determinations of latitude and longitude

Station	Latitude, north	Longitude, west
	° ′ ″	h m s
Hacienda de Santa Rosa	22 55 00	6 52 29
Hacienda de San Jose Llanetes	22 54 58	6 53 07
Chalchihuites (town)	23 29 42	6 55 33
Valparaiso (town)	22 46 30	6 54 16

The probable errors obtained were on an average ±3" for the latitude and ±0.3 second of time for the longitude.

Bases--A geodetic base to the west of the City of Fresnillo, Zacatecas, having an approximate length of six km and a mean grade of 0.25 per cent, was established along with the expansion-figure for connection with the primary net.

Gravimetric measurements--Through the initiative and under the auspices of the Pan-American Institute of Geography and History a circuit of gravity-observations was begun in the month of November in the northern part of the Republic including the following places: Tacubaya, D.F.; Rio Verde, San Luis Potosi; Ciudad Valles, San Luis Potosi; Tampico, Tamaulipas; Ciudad Victoria, Tamaulipas; Monterrey, Nuevo Leon; and Tacubaya, D.F. The observations at the five primary stations were completed during the present year. A pendulum-equipment of nickel-steel, constructed by the Gaertner Scientific Corporation of Chicago, was used.

Precise leveling--On account of the steep grades crossing the mountainous zone between Taxco and Iguala, Guerrero, over the highway from Mexico to Acapulco, the progress was very slow during this period; only about 40 km were leveled of which some sections were repeated because the tolerance was exceeded. By presidential agreement the "Mixed Commission for the Study of the Subsoil of Mexico City," formed by representatives of the Central Department, of the National University, and the Secretary of "Agricultura y Fomento," was created. This Commission will study the movements of the soil and subsoil of the Valley of Mexico and especially of the capital of the Republic in order to apply the findings to the problems of building foundations and to the drainage of the city. The said Commission has begun to execute some of the precise leveling extending from the fundamental bench-mark at Tacubaya, D.F., for a distance of 45 km along the Gran Canal del Desague. The very interesting result obtained up to the present time is a marked decrease in the elevation of some places, which seems to indicate a progressive sinking of the soil in Mexico City. The mean closing error was 1.09 mm.

Traverses--For the detailed survey of the State of Zacatecas and its contiguous use in the construction of the map on a scale of 1:500,000, traverses were run with a one-minute theodolite and a 50-meter steel tape along the following roads: Villa del Refugio to Juchipila to Santa Rosa; Fresnillo to Sombrerete to Chalchinuites; and Fresnillo to Sain Alto to Rio Grande. These surveys which extend approximately 316 km were connected to triangulation-stations.

Direccion de Geografia, Meteorologia e Hidrologia,
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ASTRONOMICAL DETERMINATIONS IN CENTRAL AMERICA

R. W. Woodworth

Geodetic activities, in connection with the demarcation of the Republic of Guatemala's boundaries with the adjoining Republics of Honduras and El Salvador, were admirably presented to the American Geophysical Union a year ago in a paper by Sidney H. Birdseye, Chief of the two Boundary Commissions charged with these projects.

Execution of the first-order boundary triangulation-arc, now extending from the Atlantic to the Pacific along the 400 km of generally mountainous country comprising Guatemala's southerly frontier, called first for the establishment of a provisional datum based on one or more accurately determined geographic positions and azimuths. These conditions were met by the establishment of two astronomical stations in 1935 by J. P. Lushene, United States Coast and Geodetic Survey, under whose direction the Guatemala-Honduras first-order boundary-arc was accomplished. One station was located at the Atlantic end of this triangulation, the other near the common juncture of Guatemala, Honduras, and El Salvador.

These two astronomical positions, although of high accuracy in themselves, are affected by such "deflection of the vertical" as is caused by the gravitational pull of surrounding mountain-masses. Lacking the necessary topographic maps for an accurate delineation of the extremely rugged country it was not possible to compute deflection-corrections for either astronomical position. The closure of 12".9 in latitude and 11.4 seconds in longitude, obtained through the interconnecting first-order triangulation (in itself practically unaffected by deflection), supplied a measure of relative deflection which was applied in part to the southerly astronomical station. This corrected position was adopted as the provisional datum for adjustment of the triangulation-arc. The northerly astronomical determination served thereafter only as a Laplace azimuth-station used to correct the twist of 3".97 accumulated in the triangulation.

This adopted position of the point in Honduras, near its juncture with Guatemala and El Salvador, known as the Ocotepeque Datum, together with the adjusted Atlantic-Pacific Arc of first-order triangulation based upon it, furnishes a series of excellent position-length-azimuth bases for future expansion of precise horizontal control through all the Central American countries in the form of triangulation-arcs necessary for the control of internal or of boundary-surveys.

To furnish the Laplace azimuth-station required to adjust the Guatemala-El Salvador section of the Interocean Arc was the reason for the request by the Comision Mixta de Limites, through the United States Department of State, that the United States Coast and Geodetic Survey determine latitude, longitude, and azimuth at one triangulation-station of the Pacific base-net for this Arc.

The writer was assigned this detail and sailed from New York City on April 17, 1937. Actual observing was commenced May 5 at the astronomical station located on the Pacific coast just east of the Guatemala-El Salvador Frontier. Following the completion of this determination four additional stations were established upon the requests of the governments of Guatemala and El Salvador.

This class of work required only a small station-party, namely, observer, recorder, general handy-man, cook, and helper. With a small advance party to handle monumenting, construction, and camp-establishment it was possible for the station-party to commence observations their first night in camp. The abilities and close cooperation of my three principal assistants--Recording Engineer Mejia (R.P.I., 1918) for El Salvador, Recording Engineer Sarti (M.I.T., 1918) for Guatemala, and Constructor Ordonez--were largely responsible for the successful completion of astronomical determinations during 1937-38, the majority of which were necessarily obtained under decidedly adverse conditions of weather.

Until late October and the cessation of the rainy season, the party journal notes only 12 per cent of the nights (during which all-night "watches" were maintained) as even possible for astronomical observing. The two stations occupied during the rainy season averaged 72 days each. The remaining stations, although occupied during the dry season and averaging but 15 days each, were considerably affected by local conditions of weather, only one in the lowlands of northwest Guatemala permitting a practically uninterrupted observing program. This station was completed in nine nights. In many respects the mile-high station at Guatemala City was rendered the most difficult, through the fast-scudding mountain-clouds that prohibited observing 80 per cent of the nights during December and January, plus the very heavy dews at night which caused weakening of the low-voltage transit-circuits and necessitated frequent removal of condensation on the lens.

Nights were cool even at the stations in the lowland, and days at these locations were rendered quite comfortable under the high-peaked palm-leaf thatches of the quickly-erected "champas".

Minor earth-temblors, of frequent occurrence at the two northerly stations in El Salvador, fortunately avoided the periods of observation. The sensitive stride-levels of the Bamberg gave evidence of the slightest "shake".

Transportation presented few problems inasmuch as all but the first station were directly accessible by rail, truck, or ox-cart. The last-named, plus mules and dugout-transport along the beach and five tidal crossings en route to the first station, resulted in the total immersion of the party's short-wave receivers. Hence the rather important element of time was obtained by star-observations entirely until a replacement radio was received from San Salvador.

First-order triangulation will carry position and length from the precision of a first-order base-line over considerable distances with practically no loss in accuracy save for the tendency of small systematic angular errors to impart a twist to the arc. To correct this is the prime function of the Laplace azimuth, an astronomical azimuth adjusted for the astro-geodetic longitude-difference and derived from astronomical stations located six to eight figures apart along the triangulation-arc.

For the foregoing purpose, in connection with the international boundary-surveys, the following accuracy criteria were used: A longitude probable-error not to exceed 10 feet; an azimuth probable-error not to exceed 0″.3; a latitude probable-error not to exceed 10 feet.

To obtain these measures of precision under field-conditions calls for an astronomical transit-instrument readily portable, with sufficient weight for stability, yet not so massively constructed that flexure enters unduly into the observing of stars at varying altitudes. For this purpose the United States Coast and Geodetic Survey has found a Bamberg broken-telescope transit, of approximately 200 pounds weight, an excellent instrument for both latitude and longitude. It is a fixed-base type, equipped with 1″.3 stride-levels, with magnifications from 44 to 96. Its micrometer-eyepieces are readily transferable for the observation of latitude or longitude, the former readings being taken and recorded directly to 0″.1, the latter being transmitted electrically for chronograph-registration.

The Bamberg transit is mounted either atop a heavy concrete pier as for the recent Central American work, or on a knock-down, braced, aluminum tripod as is the usual practice for Laplace stations of the States. It is then carefully placed in the meridian within one second of time and held there for the duration of the observations by means of base-plates secured by plaster-of-paris, one of which permits a slow-motion adjustment in arc. On a concrete-pier mount it is very seldom found necessary to correct the original meridian-placement, even for as long a period as a two-month station-occupancy.

For latitude the Horrebow-Talcott method of equi-altitude stars is used, in which is measured the small difference of meridional zenith-distance of a north and a south star culminating at approximately the same time. This method is particularly accurate in that refraction-error is practically eliminated. Star pairs are obtained from United States Coast and Geodetic Survey star-charts and the Boss catalog, using magnitudes between three and seven with a 65-magnification eyepiece. Two nights of 12 pairs each will usually yield the accuracy required, but it is desirable to observe a slightly greater number of pairs for each of three nights, thus obviating possible constant night-errors due to atmospheric conditions.

Although actual star-pointings must needs be secured with extreme precision, sidereal time to one-half second is sufficiently accurate for purposes of latitude-computation. Daily radio-chronometer comparisons using Naval Observatory time-signals give a continuous chronometer-correction.

Having obtained the required number of latitude-observations, a skilled astronomical observer is assured of satisfying the accuracy-criterion. Hence it is possible to speed field-operations by having all latitude-computations made directly at the Washington Office of the United States Coast and Geodetic Survey. For the five Central American astronomical stations of 1937-38 the mean probable-error for all latitude-determinations was 0".04, or four feet in distance.

For longitude, dependent upon accurate simultaneous comparison of local sidereal time with that of the Naval Observatory at Arlington through the medium of short-wave time-signals, the Bamberg transit is supplemented by special battery-operated recording apparatus consisting of a short-wave code-receiver, a two-stage amplifier, a constant-rate sidereal chronometer, and a recording chronograph. Two sets of longitude obtained on two different nights will usually suffice, but on this special work five sets over three nights were required under the instructions for the project.

A single longitude-set consists of observations on from six to ten time-stars obtained just prior to the recording of a Naval Observatory time-signal, after which immediately follows a similar group of time star-observations. True local sidereal time of the Observatory signal recorded is derived with thousandth-second accuracy from these observed time-stars, which also establish the chronometer-rate during the observing period. Therefore, a longitude can be obtained as the difference of the simultaneous sidereal times at the field astronomical station and at the center of the Naval Observatory clock-room.

Only American Ephemeris 10-day stars, with magnitudes from two to six and of small zenith-distance, are used for time-observations. With the approximate latitude (within one minute of arc) and local sidereal time as finder-arguments a star to be observed comes into the telescope-field about 1-1/2 minutes prior to its meridian passage. Its bisection is held by a movable hair, the gear-train for which also actuates an agate and platinum-contact drum for the electrical transmission of definite intervals of the observed star-time. Ten to 20 contacts are made prior to the star's culmination with the instrument direct, followed by a reverse-order repetition of the identical contacts after culmination and with the instrument reversed. Thus the observed time of culmination becomes the mean of any pair of direct and reverse contact-breaks as recorded on the chronograph-sheet. Scaling ten pairs of breaks per star yields hundredth-second accuracy for a single star. The time mean of the six to ten stars in each half-set permit thousandth-second accuracy for the determination of local sidereal time.

The chronograph-sheet records two-second breaks of the sidereal chronometer against which the star-breaks are scaled for time. Using double speed to permit closer scaling the Naval Observatory signal is recorded in a similar manner, and from 10 to 40 breaks are used to derive a thousandth-second mean. Amplifier- and relay-settings are maintained as constant as possible throughout each longitude-set to obviate variable lags in the circuits. Longitude is computed in the field and allowance is made for possible set-differences due to the then unknown values for sending-lag at the Observatory.

For the five Central American astronomical stations of 1937-38 the mean of observed star-time residuals was 0.025 second with a maximum of 0.09 second for a single star. A mean probable error of 0.005 second of time was obtained, or approximately 7 feet in distance. This value, as also the latitude-value, deals with observational accuracy only since it was not possible to correct for deflection of the vertical.

For azimuth-observations a Parkhurst 9-inch direction-theodolite, equipped with a quick-finding star-pointer was used. This instrument was stably mounted on a 3-meter stand, carefully protected from wind, and observations taken over clear lines to an azimuth-mark of about the same elevation at least two miles distant. No trouble was experienced save for the one azimuth-line necessarily observed just inshore from the Pacific Beach, on which surf-haze caused considerable flaring and walking of the signal-light pointed on. Polaris only was observed with 16 to 24 positions on each of three nights. Half-second sidereal time sufficed. Azimuth was computed in the field, using the best available values for the latitude and longitude of station. It is possible to obtain a rapid derivation of both within two seconds of arc.

The mean probable-error obtained from the five astronomical station-azimuths of 1937-38 equalled 0".14. Field-computations were checked and the final computations completed by mathematicians at the Washington Office of the United States Coast and Geodetic Survey.

At the Pacific Coast astronomical station of the Interocean Triangulation Arc the astro-geodetic differences were very small except in latitude. This discrepancy in latitude indicates that the adopted provisional datum of 1935 may be too far north, but it was felt that no change in the original datum should be considered until several more astronomical determinations were obtained, as also accurate large-scale topographic maps for the computation of theoretical deflections at all astronomical stations. Such additional stations would necessarily be connected by first-order triangulation to the present Arc. Actually, however, the slightly improved datum to be expected even with these other stations and theoretical deflection-values still might not warrant upsetting the 1935-datum, which in itself will serve all practical purposes of expansion through Central America until the accomplishment of a first-order connection between the present Central American Arc and the North American Datum through the 98th meridian Mexican Arc. With this connection a reality, all Central American triangulation could be placed on a fully corrected horizontal datum.

Upon completion of the astronomical station at the Pacific end of the Guatemala-El Salvador Frontier, work was taken up on the four additional stations requested by the individual countries. These were desired to furnish the Laplace azimuths essential to the triangulation-control of the complete aerial-cadastral surveys contemplated. Incidentally the same astronomical stations were strategically located for possible future usage (1) when the triangulation-connection to the North American Datum is undertaken and (2) should a precise delineation of the El Salvador-Honduras Boundary be desired. Two of the stations also established exact astronomical positions for the Meteorological and Seismological Observatory at Guatemala City and for the Seismological Observatory at San Salvador.

With a single exception, all were located to permit the ready measurement of an adjoining first-order base-line. Astronomical stations were established, and permanently monumented, at San Salvador, in the central part of El Salvador, La Union, in southern El Salvador near the end of the Honduras Frontier, Guatemala City, in the south-central part of Guatemala, and El Sitio, on the Guatemala-Mexico Frontier near the Pacific Coast. With these, plus the three astronomical stations on the Interocean Arc, El Salvador has sufficient Laplace control for the two or three more triangulation-arcs necessary to establish basic control for any internal mapping. One such arc could also serve for precise demarcation of the El Salvador-Honduras Boundary. The astronomical station at La Union, on Fonseca Bay, might possibly be of some use in connection with any future boundary-survey of the Honduras-Nicaragua Frontier now in dispute.

The five astronomical stations in and adjacent to Guatemala will furnish sufficient Laplace control for that portion of the North American Datum connecting arc crossing this country. Together with such astronomical positions of secondary accuracy as have been determined by British Honduras along its westerly and southerly frontiers and those now being established by the Fairchild Aerial Surveys to control their mapping operations in northern Guatemala, there will possibly be sufficient astronomical position-data to supply a measure of horizontal control for internal surveys.

It is to be hoped that the considerable advance made and the local interest aroused in precise geodetic control through the efforts of Honduras, Guatemala, and El Salvador may continue in their own and the adjoining Republics of Central America, in order that these countries may achieve the complete and accurate network of first-order control so essential to the proper mapping and development of their natural resources.

U. S. Coast and Geodetic Survey,
Washington, D. C.

INVESTIGATION OF THE VERTICAL GRADIENT OF GRAVITY

Sigmund Hammer

(1) Introduction--A precise numerical value for the rate at which the acceleration of gravity changes with height at the surface of the Earth is of great importance in geodesy and applied geophysics. This quantity is called the vertical gradient of gravity or in the present paper simply the vertical gradient.

The previous determinations of the value of the vertical gradient may be summarized as follows:

(a) A theoretical "normal" value, -0.3086 mg (milligal) per meter, was calculated by Helmert about 1910 [see 1 under "References" at end of paper]. Helmert's value appears to be in universal use today.

(b) Experimental values have been determined by several observers who noted the change in weight of a mass with height by means of high-sensitivity balances. These experimental results [1] are given in Table 1.

Table 1--Previous experimental values

Observer	Year	Elevation-range in meters	Observed gradient in mg/meter	Departure from normal[a] in per cent
von Jolly	1881	21.005	-0.295	-4 1/2
M. Thiesen	1890	11.479	-0.272	.....
			-0.303[b]	-2
Scheel and Diesselhorst	1895	29.731		
		14.055	-0.289	-6 1/2
		7.599		
Richarz and Krigar-Menzel	1898	2.2628	-0.285	-7 1/2

[a] The sign of the entries in this column is based on the absolute values of the vertical gradient and is mathematically incorrect. The reason for the adopted sign convention will be apparent in section 3.

[b] Corrected for numerical error by Scheel and Diesselhorst. These observed values are consistently smaller, numerically, than the theoretical normal value.

(2) Present experimental determinations--The present series of observations was undertaken to determine whether the apparent systematic discrepancy, indicated by the last column in Table 1, is real. Observations were made in five high buildings located in Pittsburgh, Pennsylvania, Washington, D. C., and New York, New York, with a gravimeter designed and constructed by R. D. Wyckoff and the late Archer Hoyt at the Laboratory of the Gulf Research and Development Company. The gravimeter was calibrated against a gravity-difference which had been determined by Gulf pendulum-apparatus, and the absolute calibration is known to a probable error of about 0.2 per cent. The constancy of the calibration was checked against the pendulum gravity-difference before and after each vertical gradient-determination and showed a relative variation of only 0.1 per cent over the four-month period during which the observations were made.

The observations in the present experiments were made as follows: The gravimeter was placed on the ground floor, oriented, leveled, and read. Also the vertical component of the magnetic field was measured. [The gravimeter is appreciably affected by strong magnetic fields. Out of doors the Earth's magnetic field is sufficiently constant so that the differential effects are negligible. However, within buildings the steel framework distorts the magnetic field so strongly that "magnetic corrections" are necessary. The influence of variations in the strength of the horizontal component of the magnetic field is eliminated by orienting the gravimeter with a compass. However, the variations in the strength of the vertical component of the magnetic field must be measured to determine the corrections for their effect. Unless there are large magnetic variations within the dimensions of the gravimeter--about two feet--the results obtained in the present experiments should be free from spurious magnetic effects.] Similar observations were then made on several progressively higher floors to the top of the building. On the way down the floors were reobserved in reverse order. To increase the precision and to check the gravimeter-performance another round trip was made. All readings on any one floor were made at the same spot. A plot of the gravimeter-readings in the Cathedral of Learning, University of

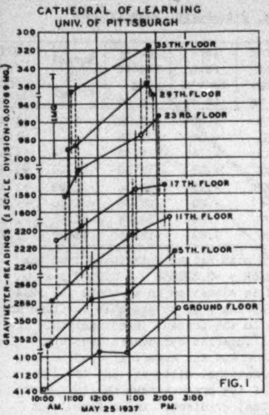


FIG. 1

Pittsburgh, is shown by Figure 1. Successive readings taken on any one floor are joined by straight solid lines. The general slope of these lines is the drift of the instrument which in this case was about 0.2 mg per hour. From such a plot, the average gravity-changes between the various floors observed are easily calculated from the differences indicated by the dotted vertical lines.

Observations were made in the same manner in the Gulf Building in Pittsburgh, the Washington Monument in Washington, and in the Empire State and Chrysler buildings in New York, but to save space the plots of the original data are not reproduced here.

The internal consistency of the gravimeter-readings is illustrated in Figure 2 which shows the readings for each building reduced to the ground-floor by subtracting the observed average differences from the individual readings on the upper floors. The points thus calculated are shown by solid dots. Actual readings on the ground-floor are shown by open circles. These points give the "composite drift-curve" of the gravimeter in each of the buildings observed. From the deviations of the individual points from the average straight line or estimated smooth drift-curve the probable error of a single reading (r_1) can be calculated by the usual least-squares formulae. From r_1 and the number of readings involved the probable error of an average observed gravity-difference (r_g) may be obtained. These probable errors are shown on the respective curves in Figure 2. [Probable errors calculated in this manner will necessarily be low because of the tacit assumption that the average of the few determinations for each floor is free from error. The magnitude of the underestimation has not been investigated but is probably not large.] It is noteworthy that the probable errors of the observed gravity-differences are about 1/20 mg.

The observed gravity-differences need several corrections: (1) For variations in the vertical component of the magnetic field; (2) for the mass-deficiency in the excavated basement of the building in which the observations were made; (3) for the mass of the building above ground; (4) for the terrain effect--this was appreciable only for the two determinations at Pittsburgh.

An an illustration, a detailed tabulation of one set of observed data and calculated corrections is given in Table 2.

The final results of the present measurements are shown in Figure 3. For clearness in the Figure the origin of ordinates has been shifted ten mg for each curve. The slopes of the straight lines through the points on these plots give the observed vertical gradients of gravity. The data for the Chrysler Building are not shown because all the data needed to complete the analysis have not yet been secured. Two determinations, with different gravimeters, were made in the Washington Monument.

For comparison, the right-hand part of Figure 3 shows an assembly of the individual observed slopes and the theoretical normal slope. Two observed slopes fall above and two fall below the normal slope and therefore, on the average, the present experimental determinations check the theoretical normal value of the vertical gradient of gravity. The results are shown more clearly in Table 3. The last column in Table 3 gives the departures of the observed vertical gradients from the theoretical normal values, expressed in per cent of the normal value. The range of the vertical-gradient anomalies is about three per cent. Note that the

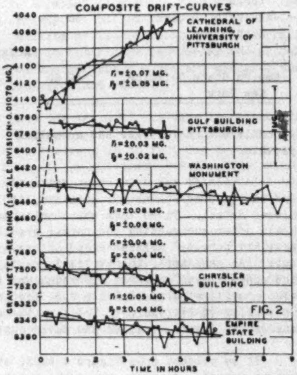


FIG. 2

Table 2--Gravity-variations in the Gulf Building, Pittsburgh, Pennsylvania

Floor	Relative elevation in meters	Observed Δg in mg	Magnetic corr'n in mg	Corr'n for basement in mg	Corr'n for bldg. in mg	Terrain corr'n in mg	Corrected Δg in mg	Normal Δg in mg[a]	Difference in mg
Sub-basement	- 10.59	+ 2.88	-0.22	+1.55	+.19	+.30	+ 4.70	+ 3.27	+1.43
Ground	0	0	0	+ .58	+.31	+.26	+ 1.15	0	+1.15
6th	+ 27.03	- 6.58	-0.58	+ .18	+.02	+.14	- 6.82	- 8.34	+1.52
12th	+ 49.28	-12.90	-0.72	+ .08	-.03	+.05	-13.52	-15.20	+1.68
18th	+ 71.22	-19.18	-0.86	+ .04	-.03	-.12	-20.15	-21.98	+1.83
24th	+ 93.17	-25.86	-0.83	+ .03	-.04	-.30	-27.00	-28.75	+1.75
31st	+118.77	-33.43	-0.97	+ .02	-.07	-.39	-34.84	-36.65	+1.81
38th	+147.73	-42.06	-0.72	+ .01	-.25	-.69	-43.71	-45.58	+1.87

[a] This column gives the theoretical normal gravity-change with height calculated from the formula for the normal vertical gradient in mg per meter, $\partial g/\partial H = -0.308550 - 0.000227 \cos 2\varphi + 0.000145H$. In this formula φ is geocentric latitude and H is elevation in km above the 1924 International Ellipsoid. The difference introduced by considering φ to be geodetic latitude and H to refer to sea-level is very small and has been neglected in the present investigation. The author's derivation of this formula follows Helmert closely ("Theorien der Höhere Geodäsie," Vol. 2, Chap. 2) but has been carried to a higher approximation and the numerical coefficients are based on the latest accepted data, namely, the 1924 International Ellipsoid and the 1930 International Gravity-Formula. Since the oral presentation of the present results the author has learned of two other modern formulae for the normal vertical gradient: (a) A formula on page 23 in "Fundamental tables for reducing gravity observed values," by G. Cassinis, P. Dore, and S. Ballarin, Milan, 1937, which is equivalent to $\partial g/\partial H = -0.308552 - 0.000220 \cos 2\varphi$, and (b) the formula now in use by the United States Coast and Geodetic Survey which is equivalent to $\partial g/\partial H = -0.30855 - 0.00022 \cos 2\varphi + 0.000144H$. The differences between these formulae are negligible in connection with the present measurements.

Table 3--Present vertical-gradient results

Location	Latitude	Mean elev. meters	Elev.-range meters	Observed vertical gradient in mg/meter	Normal vertical grad. in mg/meter	Vertical-gradient anomaly in per cent
Cathedral of Learning, University of Pittsburgh	40 27	345	137.8	-0.3060±.0004	-0.30854	-0.8±0.1
Gulf Building, Pittsburgh	40 27	300	158.3	-0.3048±.0006	-0.30854	-1.2±0.2
Washington Monument, Washington	38 53	90	152.2	-0.3128±.0011 -0.3115±.0017	-0.30858 -0.30858	+1.4±0.4 +0.9±0.5
		Weighted averages		-0.3124	-0.30858	+1.2
Empire State Building, New York	40 45	160	290.6	-0.3128±.0003	-0.30856	+1.4±0.1

probable errors indicate the anomalies to be real. This point will be considered later in the paper.

In view of the consistent failure of the previous determinations to check the normal value and the good agreement now obtained, it is desirable to examine the present data in somewhat closer detail. To do this the results have been replotted on a very much enlarged ordinate-scale after subtracting the normal gravity-change. The plot then shows the observed vertical gravity "anomaly" and the slope of the curve measures the observed anomaly in the vertical gradient. [As used here, the vertical gravity "anomaly" is simply the algebraic difference between the appropriate "observed Δg" value and the normal gravity-change. The details of the calculation are illustrated in Table 2.] Figure 4 shows such data for the Cathedral of Learning at the University of Pittsburgh. The results before and after removing the calculated effects of local masses are shown by dashed and solid curves, respectively.

It is evident from Figure 4 that, except for one questionable point, the data plot on very smooth curves. However, the curve which includes the effects of local masses is definitely

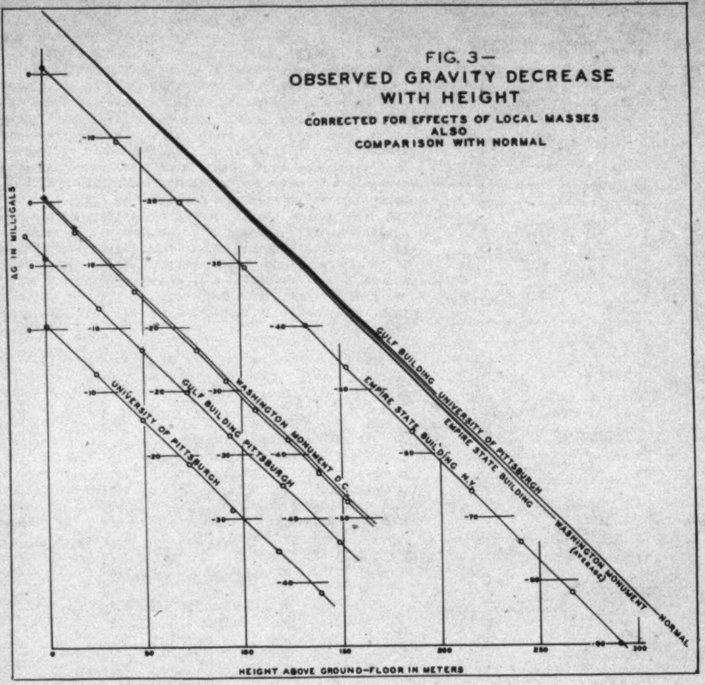


FIG. 3—
OBSERVED GRAVITY DECREASE WITH HEIGHT
CORRECTED FOR EFFECTS OF LOCAL MASSES
ALSO
COMPARISON WITH NORMAL

VERTICAL GRAVITY-ANOMALY,
CATHEDRAL OF LEARNING,
UNIV. OF PITTSBURGH

O WITHOUT MASS-CORRECTIONS
x WITH MASS-AND TERRAIN-CORRECTIONS

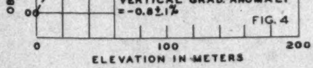


FIG. 4

convex. Applying the mass-corrections makes the points lie on a nearly straight line which suggests that the curvature is largely, if not entirely, due to the local gravitational effects of the building and the terrain. The straight line in the Figure represents the probable average slope of the corrected curve and measures the observed vertical-gradient anomaly.

Similar data for the Gulf Building in Pittsburgh are shown in Figure 5. The very great change in slope caused by the corrections is in part due to large effects of terrain of the Allegheny River valley which is about 500 feet deep and about one mile wide at this point. The Gulf Building is located near the bottom and to one side of the valley.

The results in the Washington Monument are given in Figure 6. We believe the erraticalness is due to strong local disturbances in the magnetic field caused by steel floor plates. To check the first results a second set of observations was made with a different gravimeter. These data are also erratic but the vertical-gradient anomalies in the two cases are in fair agreement. The "calculated" curve in the center of Figure 6 will be discussed later.

The data in the Empire State Building are shown in Figure 7. Note the extreme curvature of the uncorrected curve. In fact, the first part of the curve shows a strong negative vertical-gradient anomaly while the rest of the curve shows a positive anomaly. The corrected curve also shows some change in the value of the vertical gradient with height, that is, it is also slightly convex, but on account of the rather large corrections involved, the reality of this indication is uncertain. In this case reduced weights were assigned to the first few points in calculating the average straight line because the mass-corrections for these points are so large that the uncertainty in the assumed density of the excavated material becomes of appreciable importance. [The uncertainty arises mainly from lack of information as to whether or not the excavation extends into the basement rock--mica-schist of measured density 2.8. The present corrections were based on the assumption that the basement is surrounded by soil only--assumed density 2.0.]

The data for the Chrysler Building are not yet ready to report. However, the results appear to be in agreement with those obtained in the nearby Empire State Building.

The obvious conclusion from the curves in Figures 4 to 7 is that the local masses appreciably affect the observed gravity-differences in a building. In fact, if we calculate the vertical gradients from the present data over elevation-ranges comparable to those used in previous determinations (see Table 1) and if we do not apply corrections for local masses, the results in Table 4 are obtained. Note the systematic negative anomalies in the vertical gradient values obtained in this way.

Table 4--Observed vertical gradients without mass-corrections

Location	Elev.-range meters	Vert. grad. mg/meter	Anomaly per cent
University of Pittsburgh	25	-0.291	- 5
Gulf Building, Pittsburgh	27	-0.265	-14
Washington Monument	15	-0.270	-12
Empire State Building	37 1/2	-0.294	- 5
Chrysler Building	33	-0.285	- 8

Thus, the present data strongly suggest that the systematic discrepancy in the previous experimental values is due to uncorrected local mass-effects. [Thiessen's result, which included corrections for local masses, is an exception to this statement. His observed vertical-gradient anomaly of -2 per cent may be real.

(3) Explanation of vertical-gradient anomalies--The vertical-gradient values obtained in Pittsburgh, Washington, and New York appear to show significant differences which are of considerable interest. The following material substantiates their reality.

VERTICAL GRAVITY-ANOMALY
GULF BUILDING-PITTSBURGH

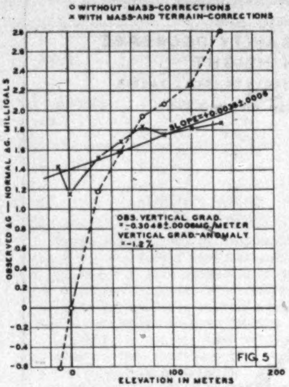


FIG. 5

VERTICAL GRAVITY-ANOMALY
WASHINGTON MONUMENT

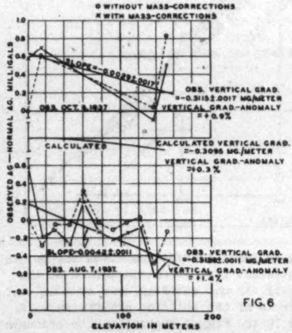


FIG. 6

VERTICAL GRAVITY-ANOMALY
EMPIRE STATE BUILDING
NEW YORK

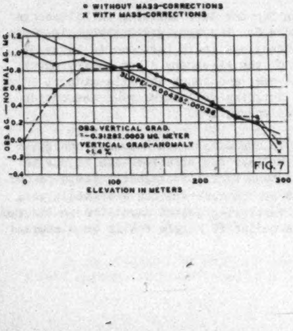


FIG. 7

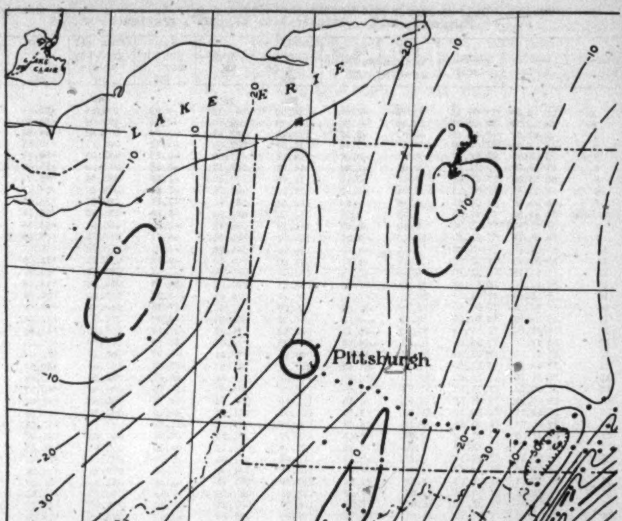
Table 5--Gulf Research and Development Company gravity-survey around Washington Monument, D. C., October 7 to 10, 1937, gravimeter No. 18

Station	Latitude	Longitude	Elevation meters	Δg re subbase mg	Observed gravity value mg	γ 1) mg	Elevation corr'n mg	Free air gravity value mg	Free air anomaly mg	Bouguer corr'n $\sigma = 2.4$ mg	Bouguer anomaly $\sigma = 2.4$ mg
	° '	° '			980	980		980			
A	38 53.35	77 02.16	11.75	-2.51	114.09	082.18	-3.63	078.55	+35.54	-1.18	+34.36
B	38 53.38	77 02.08	10.18	-1.82	114.78	082.23	-3.14	079.09	+35.69	-1.02	+34.67
1	38 53.37	77 02.13	12.61	-3.32 (2)	113.28	082.21	-3.89	078.32	+34.96	-0.69 (3)	+34.27
2	38 53.46	77 02.00	3.97	+0.08	116.68	082.34	-1.23	081.11	+35.57	-0.40	+35.17
3	38 53.52	77 02.18	2.54	-0.06	116.54	082.43	-0.78	081.65	+34.89	-0.26	+34.63
4	38 53.30	77 01.96	9.83	-1.87	114.73	082.11	-3.03	079.08	+35.65	-0.99	+34.66
5	38 53.28	77 02.22	4.66	-0.95	115.65	082.08	-1.44	080.64	+35.01	-0.47	+34.54
6	38 53.33	77 01.51	7.61	+0.73	117.33	082.15	-2.35	079.80	+37.53	-0.76	+36.77
7	38 53.50	77 02.65	4.58	-1.68	114.92	082.40	-1.41	080.99	+33.93	-0.46	+33.47
8 (subbase)	38 53.80	77 02.06	8.70	0.00	116.60 (4)	082.84	-2.68	080.16	+36.44	-0.87	+35.57
9	38 52.90	77 02.00	3.90	-2.42	114.18	081.52	-1.20	080.32	+33.86	-0.39	+33.47
10	38 54.59	77 01.80	28.43	+2.32	118.92	084.00	-8.77	075.23	+43.69	-2.86	+40.83
11	38 52.60	77 00.80	3.23	-3.55	113.05	081.08	-1.00	080.08	+32.97	-0.32	+32.65
12	38 51.95	77 03.02	1.59	-5.60	111.00	080.10	-0.49	079.61	+31.39	-0.16	+31.23
13	38 54.15	77 03.50	5.93	-3.05	113.55	083.36	-1.83	081.53	+32.02	-0.60	+31.42
14	38 51.90	76 57.11	83.88	-25.49	091.11	080.05	-25.89	054.16	+36.95	-8.42	+28.53
15	38 49.60	77 05.25	58.55	-22.16	094.44	076.67	-18.07	058.60	+35.84	-5.88	+29.96
16	38 53.85	77 07.40	122.25	-28.52	088.08	082.92	-37.73	045.19	+42.89	-12.28	+30.61
17	38 55.83	76 57.55	29.42	- 0.69	115.91	085.83	-9.08	076.75	+39.16	-2.95	+36.21
18	38 48.20	76 59.45	57.00	-28.11	088.49	074.62	-17.59	057.03	+31.46	-5.73	+25.73
19	38 48.24	76 54.45	80.34	-34.52	082.08	074.68	-24.79	049.89	+32.19	-8.07	+24.12
20	38 45.15	77 03.00	19.35	-26.36	090.24	070.14	-5.97	064.17	+26.07	-1.94	+24.13
21	38 49.80	77 11.80	109.14	-34.49	082.11	076.97	-33.68	043.29	+38.82	-10.96	+27.86
22	38 55.10	77 13.80	147.15	-26.58	090.02	084.75	-45.41	039.34	+50.68	-14.78	+35.90
23	38 59.33	77 06.91	45.52	+1.29	117.89	090.97	-14.05	076.92	+40.97	-4.57	+36.40
24	39 00.96	77 02.56	112.51	-5.10	111.50	093.35	-34.72	058.63	+52.87	-11.30	+41.57
25	38 59.37	76 56.10	19.92	+8.94	125.54	091.03	-6.15	084.88	+40.66	-2.00	+38.66
26	38 55.63	76 52.96	38.13	-16.21	100.39	085.53	-11.77	073.76	+26.63	-3.83	+22.80
27	38 53.06	77 02.68	3.05	-2.83	113.77	081.79	-0.94	080.85	+32.92	-0.30	+32.62
28	38 53.37	77 02.38	2.58	-0.74	115.86	082.21	-0.80	081.41	+34.45	-0.26	+34.19
29	38 53.12	77 02.13	2.28	-1.17	115.43	081.84	-0.70	081.14	+34.29	-0.23	+34.06
30	38 53.08	77 01.69	12.80	-2.72	113.88	081.79	-3.95	077.84	+36.04	-1.29	+34.75
31	38 53.73	77 01.85	4.39	+1.39	117.99	082.74	-1.35	081.39	+36.60	-0.44	+36.16
32 (5)	38 53.22	77 00.54	16.81	-4.18	112.42	081.99	-5.19	076.80	+35.62	-1.69	+33.93
33	38 53.38	77 02.11	11.68	-2.22	114.38	082.23	-3.60	078.63	+35.75	-1.17	+34.58
34	38 53.39	77 02.15	11.61	-2.34	114.26	082.24	-3.58	078.66	+35.60	-1.17	+34.43
35	38 53.35	77 02.11	11.45	-2.30	114.30	082.18	-3.53	078.65	+35.65	-1.15	+34.50
36	38 53.35	77 02.07	7.69	-1.31	115.29	082.18	-2.37	079.81	+35.48	-0.77	+34.71
37	38 53.37	77 02.20	6.82	-1.53	115.07	082.21	-2.10	080.11	+34.96	-0.68	+34.28
38	38 53.42	77 02.13	4.44	-0.59	116.01	082.28	-1.37	080.91	+35.10	-0.45	+34.65
U.S.C.&G.S. No. 337	38 53.60	77 02.00	-0.40 (6)	+1.40 (7)	118.00 (8)	082.55	+0.12	082.67	+35.33	+0.04	+35.37
U.S.C.&G.S. No. 84A	38 56.50	77 03.93	90.80	- -	100. (8)	086.81	-28.02	058.79	+41.	-8.5 (9)	+32.5

1) 1930 International Formula. Tables by Lambert and Darling. 2) Including magnetic correction = +0.17 mg. 3) Including correction +0.58 mg for mass of Monument. 4) Adopted gravity-value at sub-base No. 8 = 980,118 (United States Coast and Geodetic Survey, Commerce Building, base) - 1.40 (Δg to station No. 8 including magnetic correction -0.42 in Commerce Building) = 980,116.60 mg. 5) Requested by Mr. Swick; near old pendulum-base at 205 New Jersey Ave., N.W. 6) Observation taken on floor beside pendulum-pier for United States Coast and Geodetic Survey station 337. 7) Including magnetic correction +0.42 mg. 8) Adopted value by United States Coast and Geodetic Survey. 9) Including roughly estimated correction of +0.6 mg for terrain around sub-basement. Station is 31 feet below ground-level.

Figure 8 shows a regional isostatic gravity-anomaly map of a portion of the northeastern United States. [The map was prepared by L. L. Nettleton and the author from United States Coast and Geodetic Survey pendulum-results and from the unpublished results of a line of gravimeter-stations from Pittsburgh, Pennsylvania, to Washington, D. C.] At Pittsburgh, where the observed vertical-gradient anomalies are negative, the gravity-anomaly is also negative, while at Washington and New York the anomalies of vertical gradient and gravity are positive. Now, positive gravity-anomalies must be caused by local positive subsurface mass-anomalies which, in turn, would increase the local numerical value of the vertical gradient of gravity at points above the excess mass. In other words, a positive anomaly in the vertical gradient (according to the convention of sign used in this paper) corresponds to a positive mass- or gravity-anomaly. Thus, the observed variation in the value of the vertical gradient correlates in the expected way with observed gravity-values.

This correlation suggested an attempt to calculate the expected vertical-gradient anomaly from an areal gravity-survey. A very detailed survey is needed near the point under consideration because small but shallow mass-anomalies produce large gradient-effects. At greater distances these local effects become less important and the spacing of the gravity-stations may be



increased. The Washington Monument was selected as the site for this test because New York is clearly out of the question and the high topography at Pittsburgh would make the calculation of the station-reductions very laborious. Complete data on the gravimeter-survey about the Washington Monument are given in Table 5.

Figure 9 shows a map of the results in the immediate vicinity of the Monument. This map covers an area of about two-thirds of a square mile and contains 16 gravimeter-stations. The contour-interval on this map is one-tenth mg. A very local gravity-minimum is shown at the Monument, superposed upon a general rise in gravity towards the northeast. The remainder of the

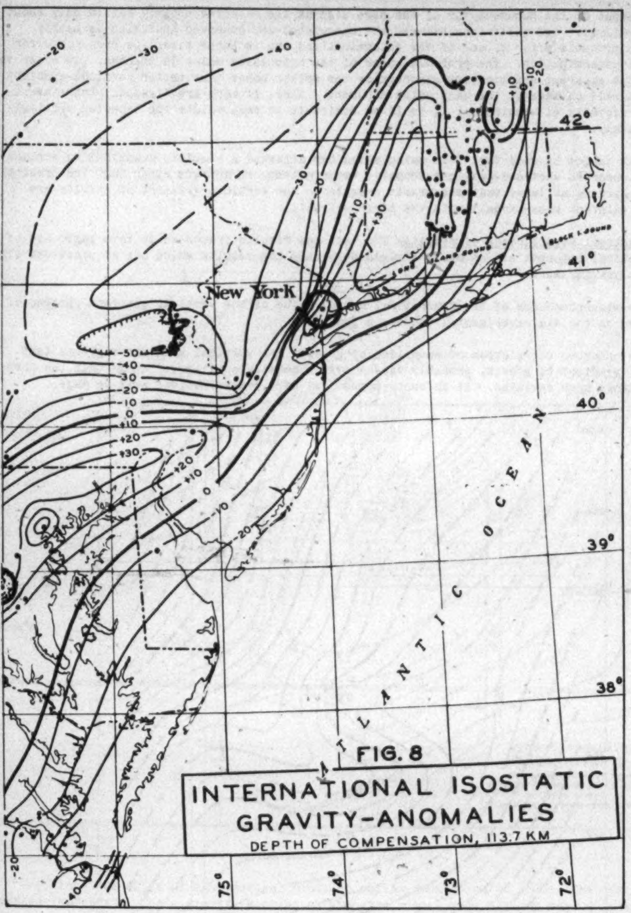

-20
-30
-40
10
20
0
42°
30
20
+10
+30
BLOCK I. SOUND
LONG ISLAND SOUND
41°
New York
-50
-40
-30
-20
-10
0
+10
+20
+30
+20
+10
0
10
20
40°
39°
38°
ATLANTIC OCEAN
-20
-40
75°
74°
73°
72°

FIG. 8

INTERNATIONAL ISOSTATIC GRAVITY-ANOMALIES

DEPTH OF COMPENSATION, 113.7 KM

results are mapped in Figure 10 which covers an area about 20 miles square and contains 22 additional gravimeter-stations. The contour-interval on this map is one mg. Washington, D. C., is located upon a strong, local, positive gravity-anomaly.

On the basis of these maps (Figs. 8, 9, and 10), supplemented by Woollard's Isostatic Map [2], Thomas A. Elkins of the staff of the Gulf Laboratory has calculated the expected vertical-gradient anomaly at the Washington Monument. The corresponding vertical-gravity anomaly is shown in the central part of Figure 6. The curvature of the calculated curve is due to the small gravity-minimum at the Washington Monument. The average calculated vertical-gradient anomaly (over the height of the Monument) is of the same sign as the observed anomaly but is only about one-third as large. The difference between the calculated and observed anomalies is nearly equal to the probable error of one of the determinations and is three times the probable error of the other determination. The probable error of the calculated value is unknown. In order to elucidate this apparent quantitative discrepancy the writer hopes that better vertical-gradient observations will be made in the Washington Monument. Also, if more gravity-data become available in the vicinity of Washington, it would be desirable to recalculate the expected vertical-gradient anomaly.

While it cannot be said that this calculation has afforded a complete quantitative explanation of the observed vertical-gradient anomaly, nevertheless, it appears clear that the present experiments have established that measurable changes in the vertical gradient of gravity are produced by existent mass-anomalies in the Earth's crust.

(4) Practical significance of results--The data and results presented in this paper are of great theoretical interest but we shall discuss here only the results which are of practical significance in gravity-work.

(a) The widespread use of the theoretical normal value of the vertical gradient instead of a value based on the old experimental values is justified.

(b) The observed correlation of anomalies of gravity and vertical gradient suggests that the vertical gradient of gravity probably varies over a considerably larger range than the present measurements have revealed. It is to be hoped that more determinations will be made.

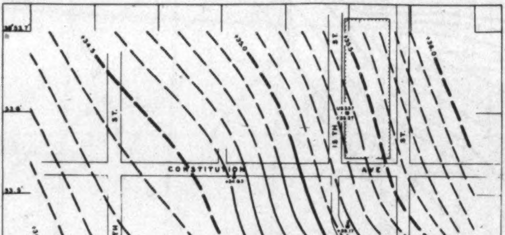


FIG. 9—
DETAILED GRAVITY-SURVEY – WASHINGTON MONUMENT
BOUGUER ANOMALY

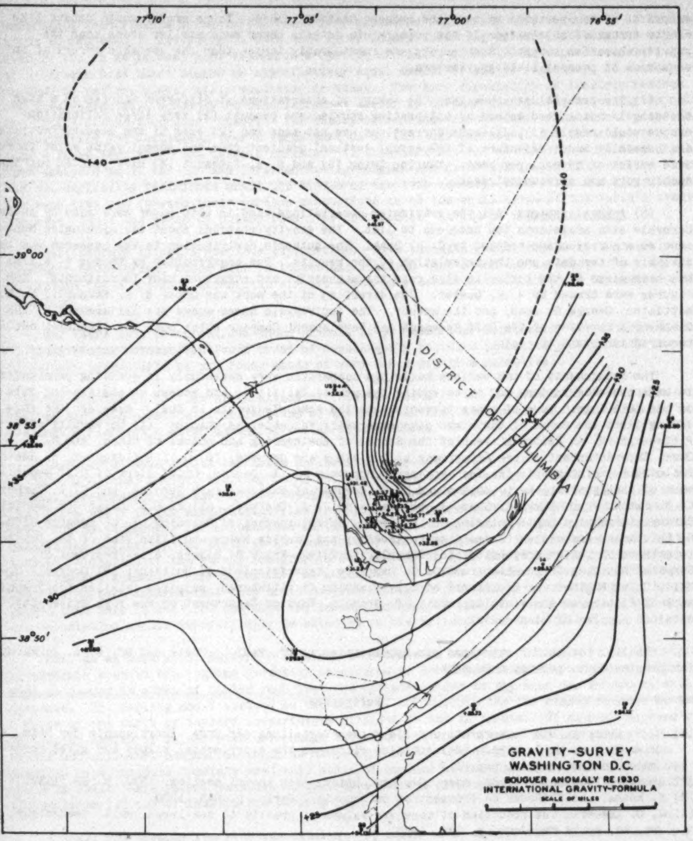

77°10'
77°05'
77°00'
76°55'
39°00'
38°55'
38°50'
140
140
135
130
125
DISTRICT OF COLUMBIA
GRAVITY-SURVEY
WASHINGTON D.C.
BOUGUER ANOMALY RE 1930
INTERNATIONAL GRAVITY-FORMULA
SCALE OF MILES

FIG. 10

(c) Since the value of the vertical gradient varies from point to point, the conventional elevation-correction for a gravity-station on a mountain or a high plateau may be in error by many mg.

(d) If the local value of the vertical gradient is not known accurately, there is no point in allowing for the variation of the normal vertical gradient with height, which E. Rothe [3] and W. D. Lambert [4] have suggested is desirable, because the horizontal variation is much larger than the normal vertical variation over terrestrial elevations.

(e) In practical gravity-prospecting possible departures of the actual vertical gradient from normal may be of economic significance. For example, in country of local topographic relief of 300 feet, an error of one per cent in the elevation-correction would introduce relative errors of up to one-third mg into the reduced station-values. These errors would behave like random errors of observation if the topographic details cover much smaller areas than the gravity-anomalies sought. Such errors are considerably larger than the probable errors of observation of present field-gravimeters.

(f) The present data show that the taking of observations at different stories in a high building is not a good method of calibrating gravimeters because (1) very large calibration-errors would result if local mass-corrections are not made and (2) even if the mass-corrections are carefully made, departure of the actual vertical gradient from the normal value might introduce errors of several per cent. Maurice Ewing [5] and E. A. Eckhardt [6] have pointed out that such errors are very undesirable.

(5) Acknowledgments--All the gravimeter-observations used in this paper were made by Anthony Carnvale with assistants too numerous to list. The gravity-stations about the Washington Monument were surveyed and reduced by C. H. Beam. The author's contribution to the research was the analysis of the data and the calculation of the results. The contribution by Thomas A. Elkins has been given in the text. He also read the manuscript and suggested many improvements. The Figures were traced by W. L. Dowler. The direction of the work was under E. A. Eckhardt, L. L. Nettleton, George B. Lamb, and the author. The individuals named above are all members of the Geophysics Division of the Gulf Research and Development Company which bore the financial outlay required to obtain the data.

The managements of the various buildings cooperated very generously in granting permission to make the observations and by extending the use of facilities and access to engineering data on the buildings. Lack of space prevents detailed acknowledgments in full. However, the following lists the names of those who gave especially valuable assistance: (1) University of Pittsburgh--E. A. Holbrook, Dean of the School of Engineering and School of Mines, and R. L. Coss, Superintendent of the Department of Buildings and Grounds; (2) Gulf Building--E. H. Heath, Building Superintendent; (3) The Washington Monument--F. E. Dodson, Chief Clerk of U.S. Department of the Interior, H. L. Wooten and B. C. Gardner of National Park Service, and C. L. Garner, C. H. Swick, W. D. Lambert, Chas. A. Whitten, and J. A. Duerksen, all of U.S. Coast and Geodetic Survey; (4) Empire State Building--F. L. Brown, Superintendent of Building, J. L. Edwards of H. G. Balcom and Associates (Consulting Engineers), and Maurice Ewing and Allyn Vine of the Physics Department of Lehigh University; (5) Chrysler Building--Frank B. Rogers, Vice-President of Chrysler Building Corporation, and C. C. Lawrence, Superintendent of Building; (6) General--C. V. Hill, Chief Engineer of Department of City Planning of Pittsburgh, supplied detailed topographic maps of Pittsburgh and vicinity, and L. P. Granath, Physics Department of New York University, obtained samples of mica-schist.

Finally, the author expresses his appreciation to Dr. Paul D. Foote and Dr. E. A. Eckhardt for permission to publish this paper.

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FORMULAS AND TABLES FOR THE DEFLECTION OF THE VERTICAL

Walter D. Lambert

The use of the spheroid of revolution as representative of the figure of the Earth rests on the assumption of a regular distribution of matter. The fact that there is an irregular distribution is patent to everyone--there are hills, valleys, plains, mountains, and oceans. But underneath this visible irregularity there is a concealed regularity that the theory of isostasy tries to represent. According to this theory, or rather according to the Pratt-Hayford formulation of it, if we go down into the Earth a certain distance, 60 to 120 km, upon every unit-area of a certain size there stands an approximately equal amount of matter regardless of whether the surface be valley, plain, hill, mountain, or ocean. The Airy formulation of isostasy reaches the same approximate equality of superincumbent matter by another conception, which may be called the "roots-of-the-mountains" theory. The theory of isostasy in one form or another represents approximately the conditions over a large portion of the land-surface of the Earth and probably over a large portion of the oceanic surface. If this were not so, the theory would never have been accepted as it is. It was foreshadowed by Bouguer and Cavendish in the 18th century but was not explicitly formulated until the middle of the 19th century, when Pratt and Airy at about the same time put forward their various conceptions as to the equilibrium of the Earth's crust. The name itself was given by Dutton [see 1 of "References" at end of paper] in 1889.

But isostasy is not a complete explanation. Even though a region may be as a whole in isostatic equilibrium, there will be individual stations that stand out as exceptions. We try to explain these by local geological structure and this seems in the main to be an adequate explanation.

But there seem to be regions where we must adopt some other hypothesis. Over large areas of ocean Vening Meinesz has found areas of consistently positive anomalies and just off the edge of certain island-arcs he has found bands of very large negative anomalies.

The problem is to find explanations of these in terms of the structure of the Earth's crust. Part of our data consists of deflections of the vertical and the problem at once arises as to how much deflection a given amount of matter will produce.

Hayford [2] gave tables for these deflections computed on the assumption of perfect isostasy at various depths, perfect isostasy being defined as absolute equality of mass between the subcrustal compensation and the visible topography compensated. Soon afterward Helmert [3] showed that there was a slight mathematical flaw in Hayford's argument. Hayford computed the deflections due to topography for a spherical Earth, but when it came to the isostatic compensation he treated the surface of the Earth as an indefinitely extended plane. Hayford gave a set of factors to be applied to the purely topographic deflections to give the resultant deflections. These factors gradually approached zero at a distance of a few thousand kilometers from the station and Hayford assumed that they would be practically zero at all greater distances. Helmert showed that this is not true and that the factors in reality take on negative values and that the calculation should theoretically be extended to the antipodes of the station.

But, as we have seen, Hayford's isostasy is not always adequate. Perhaps it is not strictly adequate even in the regions generally considered to be in approximate isostatic equilibrium. What is needed is a set of tables such that the deflection due to any mass can be conveniently computed. Mr. Darling and I assumed as the basis of our calculation the masses bounded at the surface of the Earth by Hayford compartments extended to the antipodes. Of course, Hayford's particular choice of values does not change the nature of the formulas and any particular set of radial and azimuthal boundaries could be used. Such tables are of what might be called the Cassinis type, because Cassinis used this style of tabulation in connection with attractions as early as 1911. More recent examples of this type are the gravimetric tables of Cassinis, Dore, and Ballarin [4] and of Lambert and Darling [5].

I shall not trouble you with the details. It seems to be a case where nature has no mercy on our mathematical incapacity and the formulas at best are long and complicated. They fall into three classes: (1) Formulas suitable for compartments near the station; (2) formulas suitable for compartments distant from the station; and (3) general formulas for any compartment--these general formulas, as it happens, involve elliptic integrals.

The numerical computation of these elliptic integrals is of a particularly troublesome kind and Mr. Darling has given it special attention. The difficulty is that one starts out with a large number of figures and ends up with only a very few significant ones. Mr. Darling and I have given the derivation of all three formulas and Mr. Darling has computed a particularly troublesome example of the formula involving elliptic integrals.

By these three kinds of formulas we have also computed a table of the Hayford factors on the assumption of complete isostasy, again defined as the conception of equality of mass between subcrustal compensation and visible topography compensated. These show, as was to be expected, negative values at a certain distance from the station and they begin to differ from Hayford's factors at a surprisingly small distance from the station, where one might perhaps suppose that the curvature neglected by Hayford would have but little effect. In these laborious computations we had the help of others, especially Messrs. Winslow, Duerksen, and Whitten.

The results of our labors are embodied in two papers that have been accepted for publication by the Bulletin Géodésique. Copies of the two papers have also been sent to Professor Heiskanen, Director of the Isostatic Reduction Center at Helsinki, in the hope that he or his associates may find short cuts that we have overlooked and that will make it possible to compute tables with less labor than by the formulas that we have given. At any rate, our formulas, long and troublesome though they may be, are sufficient for any compartment of the Hayford type. What is needed are general tables adapted to any hypothesis, perfect isostatic compensation, under-compensation, over-compensation, or to special conditions. With sufficient labor our formulas will give these.

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GEOLOGIC INTERPRETATION OF GRAVITY-ANOMALIES IN THE NORTHEASTERN UNITED STATES

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(Abstract)

The United States Coast and Geodetic Survey has established 124 gravity-stations, by the pendulum-method, in southern New England and eastern New York. Most of the locations were chosen with regard to the local geology. Specific gravities of rock-samples from the vicinity of each station have been determined.

Isanomaly-maps, based on the isostatic and the Bouguer anomalies, reveal a pronounced gravimetric "high" along a north-south axis in western Connecticut and Massachusetts, and two pronounced "lows," one in eastern New York, the other in Rhode Island and eastern Massachusetts. The isostatic anomalies range in value from +34 to -50; the Bouguer anomalies, from +40 to -70. There is a striking similarity between the two maps, extending to features of detail.

The wedge of Triassic sedimentary deposits in Connecticut and Massachusetts is reflected in the anomaly-values, but less faithfully than would be expected. Some other rock-units are reflected also; but their effects are superposed on the regional "highs" and "lows," which do not correspond closely to specific gravities of the surficial rocks. Results of the study lead to speculation as to the influence of deep-seated regional structure.

[It is planned to publish the full paper in the Bulletin of the Geological Society of America.]

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THE EFFECT OF GEOLOGIC CORRECTIONS ON GRAVITY-ANOMALIES

George P. Woollard

The marked relation between geologic structure and gravity-anomalies has been pointed out by many investigators, and Chamberlin [see 1 of "References" at end of paper] made a correction for the geology of the Bighorn Region of Montana and Wyoming to see if the anomalies observed there could be attributed primarily to the known geologic factors.

The results of this correction which was applied so as to embrace the geology within a radius of 16 km of the gravity-station and to a depth of eight km below it showed a marked reduction of the anomalies both positive and negative toward zero.

More recently the writer [2] in studying this subject made a geologic correction for the anomalies in the same region but applied it so as to embrace the effects of local geology out to Zone "N" of the Hayford-Bowie correction-zones and to sea-level depth below the station. The results of this correction showed the same reduction of anomalies toward zero as observed by Chamberlin, but the magnitude of the correction was very much smaller.

This difference in results was due to two factors. First, the writer's correction in stopping at sea-level, the surface of the geoid, did not cover sufficiently the vertical effects of the geology underlying the station. Second, the Chamberlin correction, although adequately considering the vertical effects of the geology (depth of eight km), failed to adequately consider the horizontal effects in going out only to 16 km. The result was that this correction was primarily a topographic one since compensation-effects do not become a marked factor in the anomalies within the area considered, and the resulting correction was far too large.

The present correction, designed to overcome the effects of this earlier work, considers the geology out to Zone "N" and to a depth of -20,000 feet. This is the depth at which it is believed basement-rock conditions are achieved at all points in the area under consideration--the Bighorn Region of Montana and Wyoming. Actually the basement is not reached at one point in the area until -23,000 feet, but from the standpoint of density at depth it is believed that basement-conditions are obtained at a much shallower depth. The -20,000-foot level adopted gives a reasonable factor of safety.

Density-distribution

Although no density-depth relations have ever been established for this Region to the writer's knowledge, it was found from the results of determinations of density and porosity on 50 of the formational units in the area that apparently conditions here approximate those observed in north Texas. In Figure 1 are shown density-depth and porosity-depth curves modified after those of Athy [3] for the shales in northern Texas. In this phase of the work the writer is indebted for laboratory facilities to the Simmons Oil Company of Bradford, Pennsylvania, and to Professor Chamberlin for the use of density-determinations that he had on this area.

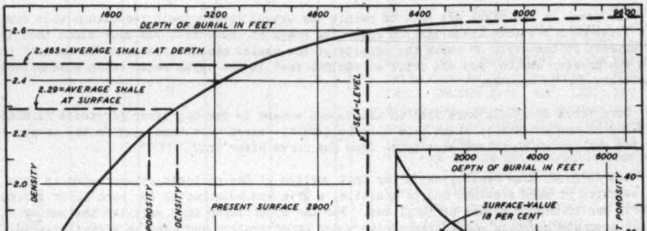


FIG. 1—DENSITY, POROSITY-DEPTH OF BURIAL CURVES FOR SHALES

The depth of burial for the shales, both on the basis of porosity and density at the surface, indicates that around 2500 feet of material has been eroded away from the central portions of the basin-areas. This, Dr. Thom [4] tells me, is in agreement with the physiographic data on the erosional history of the region.

A further check on the applicability of the curve is shown by the mean value of the core density-determinations for the area above sea-level compared against the equivalent value indicated by the curve. The values are 2.46 against 2.48.

In the light of the above it appears possible in this case to use this density-depth curve for the Bighorn Region without introducing any marked error in results.

Since the curve indicates that present sea-level conditions represent a burial of 5600 feet and that the shales at this depth would have a density of 2.58, it is evident that compaction will become practically complete with very little increase in depth. Curves by Athy [5] on per cent compaction against depth indicate that sea-level conditions in the Bighorn Region represent about 93 per cent of compaction, and this is in agreement with the work of Foley [6] on the static compaction of the Hudson River silts which shows less than two per cent remaining pore-space for the above conditions. The writer has therefore assumed that, at a depth of an additional burial of 5000 feet, density-conditions in the shales would approximate a constant. Athy's curve projected into this region indicates a value of about 2.67, the same as that observed for the basement-rock. That the density of the shales might be even greater at greater depth is quite probable, but at present no work has been done by the writer towards determining the ultimate possible density. For each shale it would be different depending upon the nature of the mineral constituents and grain-size.

Athy [6] has shown as a result of tests in the laboratory and field-observations that the sandstones can be regarded as having reached their maximum density under burial conditions of a few hundred feet, most of the compaction having taken place as a result of water-sorting at the time of deposition with few changes resulting from static loading.

For the other lithologic units present--limestones, dolomites, crystallines--little or no changes can be expected with depth of burial since the surface specimen of all of these exhibited a porosity of less than five per cent.

The other factors which would affect density with depth are pressure and temperature, and Birch [7] has advised the writer that, in the light of his investigations in this field in the laboratory, neglecting these factors would not introduce an error of more than one per cent in the density-values. Within the limits of the present investigation--zero to -20,000 feet--the two effects practically cancel out.

Therefore, in carrying out the present investigation the density-distribution has been treated in three distinct layers: (1) Ground-surface to sea-level; (2) sea-level to -5000 feet; (3) -5000 feet to -20,000 feet.

The break at sea-level was made to permit the use of the geologic reductions made down to this level on a previous investigation [2]. The break at -5000 feet was made since this level corresponds to the depth at which the density of the shales can be treated as being 2.67 for this and greater depths, and the break at -20,000 feet is the depth below which basement-rock conditions can be assumed.

Core-value densities were used in all layers except in the top layer for those formations actually at the surface, in which case outcrop density-values were used, and in the second layer for the shales, which values were taken from the curve after Athy.

The weighted mean density-value for that portion of the geologic column lying in each layer was computed at each station, and, in addition, a grid was constructed for each layer giving the density per 10-minute square of longitude. For the upper layer this entailed the making of a fairly accurate geologic map of the region under consideration and also an elevation-grid, since the density varied with these two factors. For the construction of the density-grids for the other two layers a structure contour-map and depth-to-basement grid had to be first constructed. In making both the geologic map and the basement-structure map the writer is indebted to the Yellowstone-Beartooth Research Corporation for the use of various unpublished material without which this part of the work would not have been possible.

The method of constructing the lower density-grids was to determine the density-values for each layer at about 50 points scattered throughout the area where the geologic column had been ascertained either from well data or by projections from measured sections. These values were then plotted against the depth to basement relative to sea-level at these various points, yielding a curve from which similar density-values could be picked using the depth-to-basement grid which had been constructed.

Method of correction

The method of making the geologic correction was to compute what the isostatic reductions would have been if the above density-distribution had been used and to substitute these values in place of those determined by the United States Coast and Geodetic Survey out to Zone N, the limit of the correction.

In detail the correction is a little more complicated than the above would indicate. Compensation, instead of being distributed from sea-level down, has to be distributed through that part of the column lying below -20,000 feet. Further, it was advantageous to treat the compensation as a single calculation for the entire column rather than as a series of corrections for each of the zones of changing density. This entails the determination of what density-value would yield isostatic equilibrium. This is determined by equating the actual column against the standard column extending down from sea-level to the zone of compensation. The same practice was adopted as followed by the United States Coast and Geodetic Survey in taking compensation-depth from the surface of the ground, and in this case 113.7 km was used.

Figure 2 shows the columns under consideration and the method of measuring compensation.

The following equation expresses the conditions of isostatic equilibrium, and from it the necessary density to be distributed in the lower portion of the column below -20,000 feet was determined.

$$H_0 \times d_1 + 5000 \times d_2 + 15{,}000 \times d_3 + (113.7\text{ km} - 20{,}000 - H_0)\, d_x = (113.7\text{ km} - H_0)\, 2.67$$

where H_0 = elevation above sea-level of the column, d_1 = density of layer above sea-level for the column, d_2 = density of layer 0 to -5000 feet for the column, d_3 = density of layer -5000 to -20,000 feet, d_x = density of layer below -20,000 feet for compensation, and 2.67 = average density-value assumed for the crust. It will be seen by inspection that the assumed value 2.67 might well have been 3.0 or any value with very little change in the results. A grid was constructed for these density-values of compensation similar to the other density-grids and in the same manner.

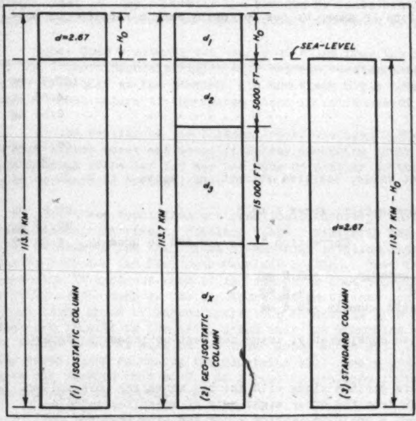
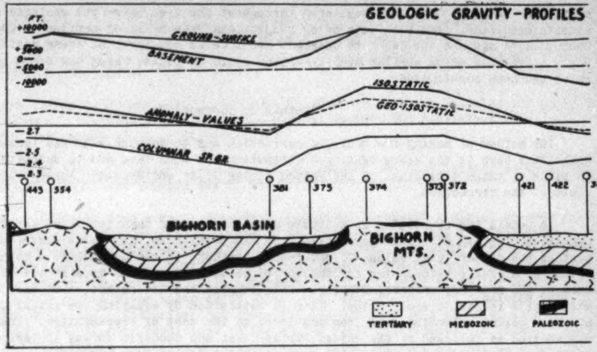


FIG. 2—DEPTH OF COMPENSATION

For determining the actual gravitational attractions the tables of Cassinis were used. These give attractions for the various zones above and below the point of observation with a density of 1.0. To determine the attraction for each of the layers under consideration the attraction was taken over a series of elevations varying from 2000 feet to 14,000 feet, the relief in the region being studied, for sea-level, -5000 feet, -20,000 feet, and -113.7 km. The difference between the attractions at sea-level and -5000 feet, -5000 and -20,000 feet, and -20,000 feet and 113.7 km is the net attraction for each layer. These net values were plotted against elevation above sea-level, yielding a series of curves for zones A to N for each of the layers.

The mechanics of making the corrections were to place the transparent zonal template on the density-grid for the layer under consideration, determine the compartment density, consult the isostatic reduction-sheets for that



station and get the elevation of that compartment, and look up on the attraction-curves the value for a density of 1.0 with that elevation in the zone and layer being considered. The product of this attraction and the density determined yielded the attraction desired.

For the layer above sea-level the actual density was used since it represented an excess of mass above the surface of the geoid. For the layers below sea-level the difference between the actual density-values and the assumed crustal value of 2.67 was used since they represented departures from the standard column which extends up to sea-level.

In correcting for the layer above sea-level it was found advantageous to make the correction by substituting the actual density-values instead of 2.67 in the topographic portion of the reductions. To this was added the correction-value for difference in elevation between station and zones. It was assumed that this value, which embraces both effects of topography and compensation, was primarily a topographic value. Check-calculations made on this zone working from the tables of Cassinis showed that, after due corrections had been made for these station-compartment elevation-differences, only a difference of around one mg was had between the two methods.

The magnitude of the various corrections is shown by the following sample results for Station No. 367 at Ulm, Wyoming.

Positive attractions:	
Layer above sea-level	137.7 mg
Sea-level to -5000 feet	4.85 mg
-5000 to -20,000 feet	2.59 mg
Negative attractions:	
Compensation	46.18 mg
Elevation-correction	1.9 mg
Net value, positive attractions, zones A to N	82.18 mg
Isostatic reduction for topography and compensation, zones A to N	86.8 mg
Geo-isostatic reduction	82.18 mg
Correction to be applied to anomaly	4.62 mg

Isostatic anomaly	-23.0 mg
Correction	4.6 mg
Geo-isostatic anomaly	-18.4 mg

The magnitude of the corrections varied considerably; these can best be shown by comparative profiles.

In Figure 3 the isostatic anomalies are plotted along with the new anomalies here called geo-isostatic anomalies to distinguish them from the other eight sets of anomalies which have been derived for this region. Also is shown a geologic section along the line followed and, in addition, data for specific gravity and depths of sediments at the various stations.

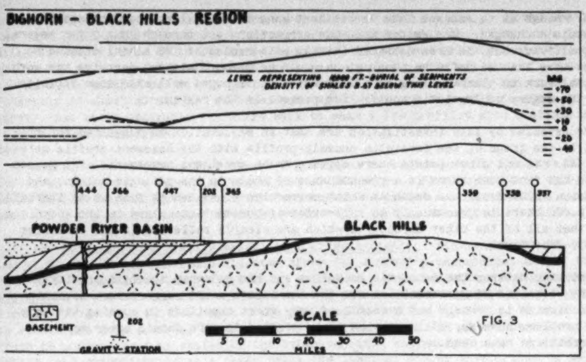


FIG. 3

Discussion of results

It will be noted on Figure 3 that the anomalies have been reduced towards zero both positive and negative, but the magnitude of the correction varies considerably. In the Black Hills Region the correction is marked in the areas of sedimentation to the east, but over the exposed crystalline basement-rocks and the relatively thin layer of sediments to the west the correction is negligible. In the case of the Bighorn Mountains and the Beartooth Mountains, where the crystalline basement is also exposed, the correction is marked, which introduces an anomalous situation. Why in one case should there be practically no correction and in another of apparently the same geological setting should there be a relatively large correction? The answer appears to lie in the geology of the adjacent areas.

It will be seen from the geologic section that the Black Hills is a relatively gentle domal uplift with a gentle slope to the west and a somewhat steeper slope to the east. The wedge of sediments on the west is relatively thin and the sediments are of Paleozoic and Mesozoic Age. In the case of the Bighorns and the Beartooth Mountains the change of geology is both sharp and great, that is, the mountains are bounded by fault-scarps, and the change in lithology is from dense crystalline rocks to light Tertiary sediments of great thickness.

Since Zone M extends out nearly 40 miles from the station it is obvious that the outer zones in the Bighorn Mountain Region will embrace considerable geology of a marked difference in character from that at the station. In the Black Hills this condition is not realized due to the transitional nature of the change, both in thickness of sediments and in density.

As the density of the basement-rocks averages 2.67--the same as that value assumed by the United States Coast and Geodetic Survey in making their reductions--no correction would occur if the geology corrected for was the same or similar to that underlying the stations in these regions of exposed crystallines.

That these conditions are practically achieved in the Black Hills Region suggests an explanation of the results obtained there. Similarly the abrupt change in the horizontal distribution of density and the volume of material involved appears to explain the results obtained in the Bighorn and the Beartooth Mountain regions. That the corrections are such as to lower the anomalies is apparent when it is remembered that topographic effects give way to compensation-effects beyond Zone J. As the compensation-effects are minus in sign and the outer zones embrace these areas of marked deviations in density from 2.67, the anomaly is lowered. Conversely stations located in the basin-areas near the mountains will have marked changes in the topographic values since it will be the inner zones which will lie in the area of changed density while the outer zones embracing the mountains will remain practically unchanged as the density-values have not changed from 2.67. As a result, stations in these locations should have their anomalies raised so as to approach zero.

Stations lying well out in the Basin so as to embrace only areas of low density would have both corrections for topography and compensation changed. The resultant anomaly, if the geology was considered far enough as to embrace Zone O, the last zone in which large corrections occur, would probably remain unchanged. The writer has made corrections out through Zone O for several stations in key-positions, but the area embraced (circle with radius of 103 miles) crossed so many geologic provinces that no definite conclusions could be reached without carrying the work further. Additional work is planned along this line which it is hoped will shed some definite information upon the degree of isostatic equilibrium present in the region.

Other factors revealed by this investigation are that an explanation is suggested for the lack of agreement in the trend of the isostatic anomaly-profile with the basement-profile between stations 367 and 444. At all other points there appears to be excellent agreement. The reason apparently lies in the fact that there is a predominance of shale in the geologic column, and that for depths below -5000 feet, the depth at which compaction of shales is such as to increase their density to 2.67, there is practically no reflection of basement-structure in the anomalies. It will be noted that all of the other structures which are clearly reflected in the anomalies lie above the -5000-foot level.

The marked relation between the isostatic anomalies and the columnar specific gravity is also shown, and the closeness of agreement between the two curves would suggest that deviation from isostatic equilibrium is perhaps not a factor of very great magnitude in causing the anomalies here. This problem, however, still remains to be studied in more detail after more complete geologic corrections have been made.

Conclusion

In the light of the present investigation it appears that anomalies can definitely be attributed to four main causes: (1) Density of material deviating from 2.67; (2) volume of material of a differential density; (3) distance from disturbing density-features; (4) lack of isostatic equilibrium.

Other factors, such as the Bowie effect, are regarded as minor contributing causes.

It also appears that in areas having much shale in the geologic column, the gravity-anomalies can not be regarded as reflecting basement-structure if it is thought a depth of burial in excess of 11,000 feet is obtained.

Although the present corrections have demonstrated that most geologic factors have been satisfactorily considered in treating the geology out to Zone N, the indications are that these corrections will have to be carried out to embrace Zone O if the anomalies are to be reduced so as to permit a study of isostatic and other factors entering them.

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A PROPOSED INVESTIGATION OF VENING MEINESZ ANOMALIES

Maurice Ewing and H. H. Pentz

To deduce the size, shape, and density of the anomalous masses which produce the Vening Meinesz belts of negative gravity-anomalies is an important problem. This calculation has been carried out by Vening Meinesz [see 1 of "References" at end of paper] for a profile from Java to Christmas Island and by Jung [2] for a profile across the Nares Deep and also for one across East Timor. The present paper considers the possibility of increasing the accuracy of this calculation in future work. Three possibilities are discussed.

(1) Stations more closely spaced on profile--Three of the Vening Meinesz belts of negative

anomalies have been discovered and surveyed more or less completely. These are associated with the East Indian, the West Indian, and the Japanese archipelagoes. They range from 1000 to 3000 miles in length, from 60 to 100 miles in width. The maximum gravity-variation across a belt is 100 to 300 mg and gravity-changes as great as four mg per mile are commonly observed in profiles across these anomaly-belts. In past work the stations were commonly spaced on the profile at intervals of about 30 miles. A closer spacing of stations is hardly feasible in a reconnaissance-survey, but it is desirable in future work to make a few profiles with stations spaced at intervals of about five miles.

A gravity-observation with the Vening Meinesz apparatus usually requires 30 to 40 minutes, during which time the submarine travels about two miles. Placing stations at 5-mile intervals would require very frequent diving or long-continued travel in the submerged condition, but the need for a few closely spaced profiles would amply justify the effort involved.

(2) More accurate gravity-measurement at each station--Vening Meinesz [1, v. 1, pp. 101-109, and v. 2, pp. 78-79] has found the mean error for a gravity-measurement at sea to average about four mg. The second-order correction to values of g measured with the Vening Meinesz apparatus, which Browne [3] has pointed out, may increase this mean error somewhat. For profiles with closely spaced observations, like those proposed in section (1) above, it might be worth while to reduce the mean error of observation by reducing the motion of the pendulum-apparatus. This could be accomplished by enclosing the apparatus in a water-tight case, anchoring it to ocean-bottom, and supporting it at a depth below the limit of strong wave-action by means of an oil-filled buoy [4]. It is not suggested that this method should supplant the standard Vening Meinesz method, but that it be used on a few test-profiles to give results with a mean error of one to two mg.

(3) Measurement of g at various depths--The proposed method for measuring gravity at sea, at any depth between sea-level and the ocean-bottom, invites consideration of possible advantages in determination of the anomalous masses which might accrue from it. It might be considered that shifting the observation-station through a range in elevation of several thousand fathoms would produce a slight change in the gravity-anomaly if the anomalous mass were deeply seated, and a great change in gravity-anomaly if the mass were relatively near the surface. Eve [5] has used the change in magnetic readings on shifting a magnetometer from the ground to the top of a 10-foot portable platform to estimate the depth of magnetic dikes, and in his discussion of Eve's paper Heiland emphasizes the great possibilities in observing magnetic intensities at different heights and states that he has designed a method for measuring these intensitiesin an airplane flying the same traverse twice at different heights. Actually the advantages gained in determination of depth by vertical shifts in the station are largely illusory in both the magnetic and the gravitational case. The magnetic field at points above the surface or the gravitational field at points below the surface may be calculated if a suitable network of measurements at the surface are available. In ordinary circumstances it would be easier to take a large number of surface-measurements than a small number of measurements at points removed from the surface.

Measurement of gravity at depth could give but little information, in addition to that obtainable from measurements at sea-level, regarding subsurface density-distribution. It would appear to be unwise to build an instrument for measuring gravity at various depths, in the hope that the variation with depth would give direct evidence about the depth to the anomalous masses associated with the Vening Meinesz belts of negative anomalies. The most promising modification of present technique is that mentioned in section (1).

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A STUDY OF CERTAIN REDUCTIONS AND ADJUSTMENTS IN PENDULUM-OBSERVATIONS

Albert J. Hoskinson

The method used by the United States Coast and Geodetic Survey to determine the value of gravity at a field-location consists of measuring accurately the period of a free-swinging pendulum at the station. The period of this pendulum at the base-station is already known, and since gravity is also known at the base-station the value of gravity may easily be computed for the field-station. The present field-practice is to swing the pendulum for a period of six hours at each field-location. Two instruments are used simultaneously at each location, thus producing a check on the work as soon as the observations are completed.

The time-interval of the swing is determined from the radio time-signals sent out daily from Arlington. Three signals are recorded for each swing, one at the beginning of the swing, one at the end, and a third one during the swing. The middle signal allows the computation of two short-interval swings which serve as a check on the total swing and an indication of the performance of the instrument during the swing. The standardization-swings at the base-station to determine the period of the pendulum at that point are also six hours in length and the pressure, starting amplitude, etc., used at the base-station serve as standards for the field-work. An effort is made to duplicate the standardization-conditions as closely as possible in the field-work, and thus any errors in the corrections or constants used for the instruments will be held at a minimum.

The pendulums used in this work are made of invar and are mounted to swing in a vacuum-chamber with the agate-plane in the head of the pendulum resting on an agate knife-edge. In the latest design the plane and knife-edge have been reversed so that the knife-edge is in the head of the pendulum. The experiments so far conducted indicate that one method is just as good as the other. The vacuum-chamber is evacuated to two or three mm of mercury or as low as it can be taken with ease with field vacuum-pumps. The accuracy desired in this gravity-work is one milligal, which means that the period of the pendulum must be determined with an accuracy of two or three in the seventh decimal place of the period.

The period of a free-swinging pendulum at any place varies with the amplitude of the pendulum, with the pressure in the vacuum-chamber, with the temperature of the pendulum, and with the stability of the support. It is impossible to duplicate exactly the conditions involving these four factors at all stations; therefore, a correction must be applied to the observed period of the pendulum at each location to reduce it to what it would have been under standard conditions. We therefore apply corrections for arc, pressure, temperature, and flexure to each observed period to reduce that period to standard conditions. The arc-corrections reduce the pendulum-period to zero-arc or an infinitely small arc. The pressure-corrections reduce all observations to a pressure of 60 mm (that standard was adopted many years ago when it was difficult to evacuate the vacuum-chamber much below that point). The temperature-corrections reduce the observations to 15° C, which is about a mean value for field-conditions. The flexure-corrections reduce to zero-flexure, or rigid stability.

Recent field-work has indicated that the checks between the short-period swings at the field-stations might be improved by a redetermination of the constants used in some of the formulas. Therefore, it was decided to conduct a series of experiments in the laboratory to check on the accuracy of the various corrections and constants. The arc-corrections were tackled first because it was quite easy to hold the other three constants steady. The temperature of the instrument was held at 37° C within 0°.1. The pressure was held at two mm of mercury with a variation of less than half a mm. The flexure was constant for all swings. The pendulum was then set in motion with a large amplitude and readings were taken every hour to determine its period until the amplitude had decreased to one mm or less. This method of recording the period allows every hourly period to be computed and also any consecutive number of hours during the swing. The time used for this work was obtained from the crystal-chronometer built by Bell Telephone Laboratories and loaned to the American Geophysical Union and the United States Coast and Geodetic Survey for scientific research. The rate of the chronometer was so uniform that it was not necessary to apply any rate-correction during the entire series of the work. The period of the pendulum was computed to the eighth decimal place for these tests or one decimal place further than used in the field-work. Arc-corrections in accordance with our formula as used in field-work were applied to the observed values and the periods have then been plotted with the period of the pendulum as the vertical ordinate and the arc-corrections as the horizontal ordinate. If the arc-corrections are exactly correct, the final values should all reduce to the same value, but it is evident from the curve that there is a slight regular variation in the values, for the higher arcs give a period slightly longer than the lower arcs. All of the observed values fit

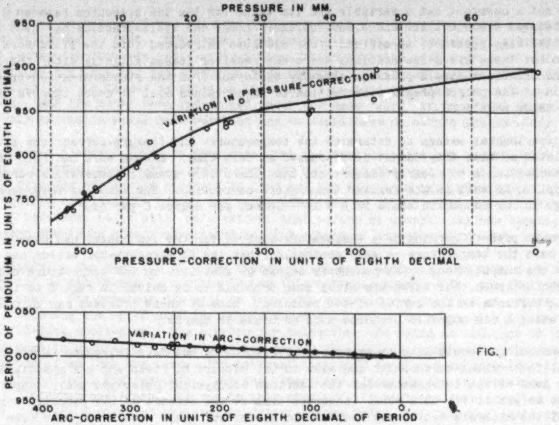

FIG. 1

very closely to the mean curve, the maximum variation being about four in the eighth decimal place of the period of the pendulum. The maximum variation shown between the largest and smallest arcs used is about two in the seventh decimal place or less than one milligal. The range is much larger than ever used in field-work so that the extreme conditions shown here are never even approached in field-work. With the effort made in recent years to duplicate standardization-conditions in the field the arc-corrections at field-stations are close enough together on the curve so that any correction from this curve would amount to less than 0.2 mg in gravity. We may therefore safely conclude that the formulas used in computing the arc-corrections have been verified by experiment and are sufficiently accurate for field-work as now conducted, but that an additional correction should be applied if results to tenths of milligal are desired. The formula used in computing the arc-corrections is known as Borda's formula and is

$$(PM/32)\ [(\sin A_0 + A_n)(\sin A_0 + A_n)/(\log \sin A_0 - \log \sin A_n)]$$

where P is the period of the pendulum, M is the modulus of the common logarithmic system or 0.43429, and A_0 and A_n are the initial and final semi-arcs expressed as angles.

Having verified the arc-corrections, the pressure-corrections were then undertaken. In this work the temperature and flexure were again held constant and a number of swings were made at various pressures inside the vacuum-chamber. All of the swings were started with the same amplitude of the pendulum, but the final amplitude differed somewhat because of the greater damping effect when more air was in the vacuum-chamber. Since the arcs are not identical on all swings, the arc-corrections have been applied to the results and the additional correction represented by the arc-correction curve discussed above has also been applied to each observation. Pressure-corrections in accordance with our formula have also been applied so that the final results should all agree if the pressure-correction formula is true. The values have been plotted with period of the pendulum as one ordinate and pressure-corrections or pressure in mm of mercury as the other ordinate. It is quite evident from the curve that the formula is seriously in error for the lower pressures now used in field-work. The formula used for determining pressure-corrections is

$$\text{Correction} = K\ [60 - Pr/(1 + 0.0036\ T°)]$$

where K is a constant determined experimentally. The value used for the invar pendulums is 0.000,000,089. This value was determined several years ago when much higher pressures were used in the vacuum-chamber and it is evident from the curve that it is approximately true for pressures around 60 mm, but decidedly in error for lower pressures. Pr is the pressure inside the vacuum-chamber, T° is the temperature in the chamber, and 0.00367 is a theoretical constant used in reducing the observed pressure to a temperature of 0° C. From the curve it is quite evident that K is not a constant but a variable and its value for the low pressures between 0 and 20 mm would be between 0.000,000,160 and 0.000,000,133. Since the field-practice has been to make all swings at the same pressure, no serious error has been introduced into the field-work by this correction but there are a few stations where the receiver leaked slightly after the swing was started and therefore have a pressure slightly different from the standardization-pressure. The application of the proper pressure-correction to these values will no doubt improve the field-checks at these stations.

The experimental swings to determine the temperature- and flexure-corrections have not yet been completed so that they cannot be discussed at this time. Enough work has been done with variable temperatures to clearly demonstrate that there is a dynamic temperature-correction that must be applied as well as the regular temperature-correction. The value of this correction from swings so far conducted would be 2×10^{-7} second per degree C per hour.

We are at present working on a temperature-control for the instrument and hope in the near future to have the temperature so well controlled that the temperature-correction may be eliminated from the computations. The necessary degree of stability in the temperature-control has already been obtained, but there are still some problems to be solved in regard to the effect of the heating currents on the period of the pendulum. Some of these problems can no doubt be solved by using a non-magnetic pendulum such as brass or quartz.

In concluding I would like to express appreciation to the Bell Telephone Laboratories for the loan of the crystal-chronometer and also to Dr. Richard M. Field and his associates in securing the loan of the chronometer for the American Geophysical Union and their cooperation in allowing us to use it for this work. Accurate time is the essence of all pendulum-observations, and without the accurate continuous time of the chronometer this work would have been nearly impossible or so long drawn out as to have been difficult of achievement.

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A METHOD FOR INTRODUCING A NEW STANDARD OF LENGTH

Philip Kissam

The advantages of uniform standards throughout the world are so great that it is unnecessary to prove their desirability. It is most fortunate for science and commerce that the metric system is becoming more and more a universal standard. However, the English system of inches, feet, and yards still prevails as the most widely used standard of length.

Since they are used so extensively, it appears essential that the sizes of these units should be internationally standardized, and there is a movement under way to standardize them at a value which is approximately one part in 508,000 shorter than the lengths now in use by the National Bureau of Standards. While this change is very small indeed, it creates nearly all the problems that any change of standards creates. And before it is accepted, careful study should be given the entire question to the end that all the advantages shall be gained, without incurring the many disadvantages that always threaten such a change.

How the length now used by the National Bureau of Standards came to be adopted and why it differs from the length used in England is a long story and not pertinent to the question in hand. The important fact is that the Bureau, and therefore the public, have used this length in the United States for over 45 years. Some individuals believe that this length is entirely without legal basis and some believe that it was legalized by the Act of Congress of 1866. But standards are only accurate definitions of words and, like words, they become accepted by custom as well as by law. They are none the less standards when they are generally accepted even if no statute supports them. Such standards might be classed as common law as compared to statute law. It appears that, whether or not Congress has legalized them, we have United States standards for the length of the inch, foot, and yard, just as we have universal standards for the ratio between these units which no law can change. Any attempt to change these present units would cause such confusion that all possible benefits would be lost.

If a new length is to be introduced, that new length must be given a new name. Any reference to "United States Standard" for the new unit should be avoided in spite of the great temptation to do so. Instead, "International Standard," or some other name should be applied to the new lengths. If "United States Standard" is used, the elevations of all bench-marks in the

United States of, or derived from, the United States Coast and Geodetic Survey net will have new values when expressed in feet. The State plane-coordinates of all horizontal positions will have to be recomputed from the first-order triangulation-stations even down to the individual property-surveys and recorded deed-descriptions--obviously an impossibility. But this is the sort of fallacy that is caused by applying the name of an accepted standard to the value of a new standard. Had the meter been introduced under the name "yard" in an effort to enhance its adoption, only confusion would have resulted and we would have no metric system today.

Any accepted standard finds so many uses that the ramifications are legion. The great advantages of a standard cause men to seize upon any unit that they believe will be generally accepted. To discover how an old standard is used and the effects of the introduction of a new standard requires considerable study, and representatives of widely varying activities should be consulted. Therefore, it is still more evident that before we accept this new length, the proposed standard should be widely advertised and the method of adoption thoroughly debated.

In considering the question of a new standard, its most probable uses should be borne in mind. When the metric system is applicable, an international standard for the English system will not be used. Its greatest use will be found for manufactured articles of import and export. The inch is becoming the chief unit for manufactured articles. We speak of a 120-inch wheel-base, not a 10-foot wheel-base. The mirrors for telescopes are rated as 100-inch or 200-inch. The inch as used in manufacturing has been completely decimalized; hundredths, thousandths, and ten-thousandths of an inch are used. The international inch is therefore more important than any other international English unit. Why not, therefore, legalize the international inch under the name of "International Inch" for use in this country? It would be soon accepted as the basic measure for industry instead of the present United States inch. The international yard, foot, etc., would be legalized by this act by inference, but they would seldom be used.

The most important United States standard is the foot, and its chief use is for engineering, construction, and surveying. It, too, has been decimalized. Route surveyors use the "station," which covers 100 feet, and tenths, hundredths and thousandths of a foot are employed almost universally. In order to clarify the law by showing exactly what is meant by the international inch the same act should contain a clause specifying the present length as the United States foot. This would prevent questions coming up in the future and once and for all satisfy those who feel that our present standards are not legal.

The question immediately arises whether it is not unwise to establish two standards in the United States which are so nearly the same but do differ slightly. A careful study should be made of this matter. Perhaps the international inch should be rejected on this ground, or an international inch requested which corresponds with our own. However, nothing should be done which might block the acceptance of some international English standard.

Practically, with the ratio between the two units clearly defined, there could be no uncertainty as to the values of each, and each would fit naturally into its own field. In practice, the inch and the foot are already becoming divorced as they do not bear a decimal relation to each other.

It has been suggested that both standards be made available but that their use be restricted to certain applications. Such legislation is dangerous due to the many ramifications and unknown needs mentioned above. When such limitations are imposed, contracts may be voided by using illegal units. A railroad company was actually relieved of paying for certain locomotives it had purchased as the wheels were not constructed in accordance with the State legal ratio of 3 1/7 for π.

In conclusion, therefore, it seems evident that an international English standard is definitely desirable and that if it cannot be the same as is used in the United States, it should be given a new name. The present foot should be legalized in the same paragraph so that a ratio is established and our present foot given definite legal standing. Also, no restrictions, no matter how tempting, should govern the free use of either system.

The following is offered as a suggestion for legislation:

> That the UNITED STATES FOOT shall be a legal unit of length defined as the distance comprising twelve hundred of three thousand nine hundred thirty-seven (1200/3937) parts of the distance defined as a meter. That the INTERNATIONAL INCH shall be a legal unit of length defined as the distance comprising two hundred fifty-four ten-thousandths (254/10000) of the distance defined as a meter. Provided, however, that no length shall be expressed in terms of a combination of UNITED STATES FEET and INTERNATIONAL INCHES.

As the American Geophysical Union has as one of its functions the duty of offering advice to the Congress on technical matters, I move that the Chairman of the Section of Geodesy be empowered to appoint a committee to study the matter and to report what recommendations should be made to Congress by the Union through the National Research Council.

Princeton University,
Princeton, New Jersey

AMERICAN GEOPHYSICAL UNION

Nineteenth Annual Meeting

SECTION OF SEISMOLOGY

Washington, D. C.
April 27, 1938

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GEOPHYSICAL INVESTIGATIONS OF THE GEOLOGIC STRUCTURE OF THE COASTAL PLAIN

George P. Woollard, Maurice Ewing, and Meredith Johnson

Introduction

In presenting this paper on structural conditions in the Coastal Plain the authors have a threefold purpose:

(1) To illustrate the effectiveness of the scientific method of investigation in working from known to unknown quantities.

(2) To show the marked benefits that may accrue through the active cooperation of governmental agencies and scientific groups.

(3) To show by specific example the value of using various types of data in conducting geologic structural investigations.

One of the first geophysical investigations of the structure of the Coastal Plain was conducted by Ewing, Crary, and Rutherford [see 1 under "References" at end of paper] in preparation for their offshore seismic investigations. A refraction seismic profile was run from Petersburg, Virginia, where the basement-rocks to be studied outcropped at the Fall Line, to Cape Henry on the Coast. The apparent excellence of this work is shown by the check on depth against the well-log at Fortress Monroe and requires no further comment here.

The United States Coast and Geodetic Survey established a gravity-profile along the same line as that followed in the above seismic work. The section was extended, however, so as to have four stations on the exposed basement-rocks in the Piedmont Area, and this work has been discussed by Swick [2].

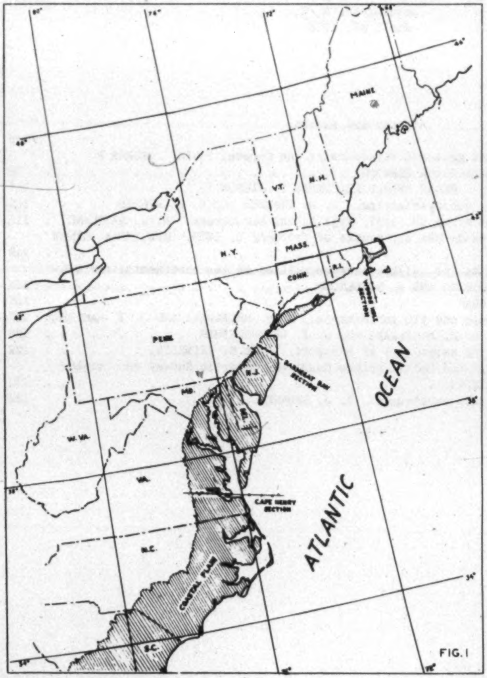


FIG. 1

More recently Ewing, Woollard, and Vine [3] have established in cooperation with the New Jersey State Department of Geology a refraction seismic profile across the Coastal Plain of New Jersey from a point near Princeton to Silverton on Barnegat Bay. Inasmuch as this work has not appeared as yet in print, the writers will discuss primarily this investigation and the problems involved.

In Figure 1 will be seen the relative positions of the geophysical profiles mentioned above, and in addition a third profile established offshore from Woods Hole, Massachusetts, by Ewing [1], but for which as yet no complete geologic correlation has been attempted.

Barnegat Bay, New Jersey, Section

In carrying out the New Jersey work the line was established so as to be not only essentially perpendicular to the structure, but to pass also through two localities in which there were wells to basement. These were located at Hightstown where basement was reported in drilled wells at -370 feet, and at Jacksons Mills where basement was drilled into at -1226 feet. Deep wells extending down to -2200 feet at the coast failed to reach basement. It was thought that this information used in conjunction with the outcrop of the basement-rocks at Princeton Junction would serve as a check on the seismic results throughout practically the entire length of the line. This was highly desirable since a published criticism [4] of the Virginia work had questioned the validity of the seismic results obtained there.

The first seismic station was established at Jackson Mills so that the characteristics of the region could be ascertained and a check depth-determination made. It is significant that both on this occupancy and on a later one, established with a different geophone and shot-spread, a consistent deviation of ten per cent on depth to basement was had from the reported well-depth. However, a very close check was obtained upon an intermediate horizon which was identified at the State Geologist's office as the rock reported by the driller at a depth of 103 feet. Similar close checks on intermediate horizons at other stations where the wells were not deep enough to yield information concerning the basement led the authors to construct a geologic profile for the section under consideration.

This geologic profile was constructed entirely from well- and outcrop-data, and in only one case was a well-record transferred more than three miles to the line. In all cases where the well did not fall exactly on the line vector-components of dip for north-south, east-west azimuths were established for each horizon and the well was moved to the line in strict concordance with these values of dip so that no distortion would be introduced into the constructed geologic profile. The vector-values of dip were determined from the observed dips in various azimuths as shown by true position structural sections that were constructed from the observed well-records.

Since only two of the well-records used are available in published form, and other of the records used are not available to the public at all, the success of this part of the work can only be attributed to the New Jersey State Department of Geology. Without the active assistance and cooperation of this office, this part of the work could never have been completed and the resulting geologic correlations of the seismic data attempted.

The seismic results as they were attained were plotted against this geologic profile, and the consistency of the correlations between the two sets of data as shown in Figure 2 permits only one explanation, namely, the seismic refraction-method of investigating geologic structural conditions yields in the Coastal Plain results that are apparently in excellent agreement with known geologic conditions.

In Figure 2 two intermediate seismic horizons are indicated--V and M. V in the upper part of the column has been identified as being a hard limy layer in the Vincentown Formation of Tertiary age and its calculated position is closely checked in wells extending from its outcrop near the Charleston Springs seismic station on out to the coast. M, an intermediate horizon in the lower part of the column which is apparently masked by the V-layer in the seaward end of the section, can not be definitely identified. Its location would suggest that it might be the unconformable contact between the Raritan and Magothy formations of Cretaceous age, but since these two formations are not differentiated in the well-records except in one case, this correlation can only be advanced on a suggestion. However, at the one well where the two formations were differentiated--Elys Corners--the M-horizon gives very good agreement with the reported contact.

The basement indicated as seismic layer B gives very good agreement in part with the projected basement determined from well-records. A divergence is noted at the Jacksons Mills Station where the seismic profile indicates a rise above the projected geologic contact, and another at the seaward end of the section where the seismic profile indicates a much steeper slope for the basement. No geologic check is possible on the latter feature, but it appears significant that it is the same condition that was noted for the seaward end of the seismic profile in Virginia [1].

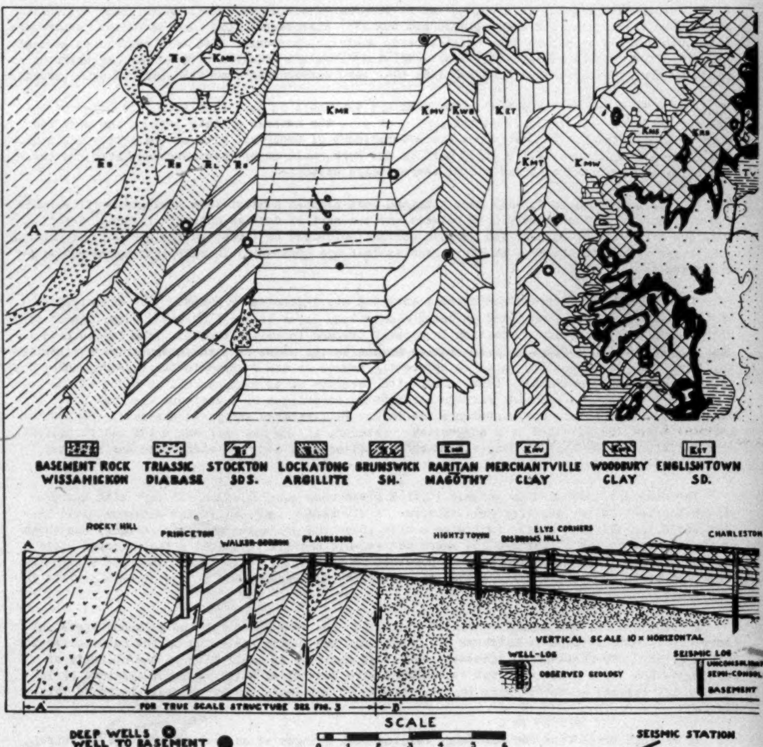
A comparison of the amount of relief indicated with that found for the basement-rocks in the vicinity of Baltimore by Matthews [5] shows that the relief indicated by the seismic results in New Jersey--280 feet--is in agreement with that shown by the well-data in the vicinity of Baltimore--285 feet. This must not be confused with the relief which would be obtained by including the exposed basement-rocks in the vicinity of Baltimore and which would increase this figure.

It is also noted from the Baltimore results that changes of as much as 200 feet in relief occur in a half mile. This appears to offer an explanation of the lack of check on depth obtained at Jacksons Mills in New Jersey. There the seismic station-profile is a mile and a half long, and the well is located at one end of this profile. It therefore appears quite possible that the average depth indicated by the seismic results over this spread would differ from a point determination at one end by as much as the 120 feet observed.

In the light of these various data it would appear that the geologic structural conditions indicated by the seismic results may well be essentially correct. That discrepancies do exist the authors well realize since the results of reverse profiles established at the Charleston Springs and Lakewood stations indicate a dip to the south for the basement of twice the magnitude of that shown along the line of the section as based on well-data. However, that in this case no great error was introduced on depth-determinations by neglect of this dip to the south is shown by the geologic correlation attained and, for the type of reconnaissance-work being conducted, the present accuracy of results is regarded as sufficient.

Plainsboro

In Figure 2 it is seen that the geology in the area adjacent to the first station on the profile--Plainsboro--is extremely complicated. At the time of occupancy the writers had no idea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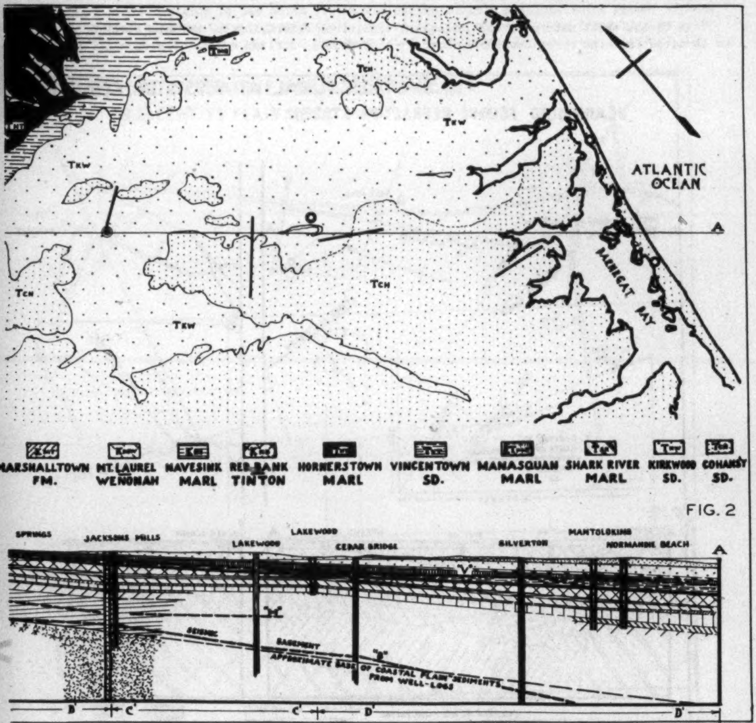
A
BASEMENT ROCK WISSAHICKON
TRIASSIC DIABASE
STOCKTON SDS.
LOCKATONG ARGILLITE
BRUNSWICK SH.
RARITAN MAGOTHY
MERCHANTVILLE CLAY
WOODBURY CLAY
ENGLISHTOWN SD.
ROCKY HILL
PRINCETON
WALKER-GORDON
PLAINSBORO
HIGHTSTOWN
DISBROWS HILL
ELYS CORNERS
CHARLESTON
A
VERTICAL SCALE 10 × HORIZONTAL
WELL-LOG
OBSERVED GEOLOGY
SEISMIC LOG
UNCONSOLIDATED
SEMI-CONSOLID
BASEMENT
A
FOR TRUE SCALE STRUCTURE SEE FIG. 3
B
SCALE
0 1 2 3 4 5 6
MILES
DEEP WELLS
WELL TO BASEMENT
SEISMIC STATION

that these geological conditions existed. The station was established to give an intermediate depth-determination to basement between that shown by the well at Hightstown and the outcrop at Princeton Junction, but for cultural reasons it had to be moved somewhat off the section-line.

That different geological conditions obtained here was first revealed by a marked change in the characteristics of the high-velocity wave. A further complication was introduced when it was found on plotting the time-distance data that there was a marked offset in the basement-velocity line. The velocity-value itself was about that of the average observed for the basement--18,000 feet per second--and thus no clue was offered as to the change in lithology indicated by the change in wave-characteristics. Similarly geological knowledge of the area did not support any marked structural changes such as were indicated by the time-distance graph. Consultation of the files of the New Jersey State Geological Survey showed that the United States Army Engineering Corps, in establishing a line of drill-holes across the State, had reported baked shale in a hole within 100 yards of the No. 1 geophone, and that another hole within a quarter of a mile of the seismic station had recorded diabase underneath the sedimentary cover. Further, the computed depth to basement based on the first part of the time-distance data showed good agreement with the 160 feet to basement reported in the bore-hole data. Since the regional geology of the exposed Triassic areas in New Jersey consists of many diabase intrusions into shale, it appeared that these were the conditions underlying the station although no geological evidence


Tch
Tkw
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ATLANTIC OCEAN
A
BARNEGAT BAY
MARSHALLTOWN FM.
MT. LAUREL WENONAH
NAVESINK MARL
RED BANK TINTON
HORNERSTOWN MARL
VINCENTOWN SD.
MANASQUAN MARL
SHARK RIVER MARL
KIRKWOOD SD.
COHANSY SD.
SPRINGS
JACKSONS MILLS
LAKEWOOD
LAKEWOOD
CEDAR BRIDGE
SILVERTON
MANTOLOKING
NORMANDIE BEACH
A
"V"
"M"
SEISMIC
BASEMENT
"B"
APPROXIMATE BASE OF COASTAL PLAIN SEDIMENTS FROM WELL-LOGS
B'
C'
C'
D'
D'

FIG. 2

NEW JERSEY GEOLOGIC-SEISMIC SECTION

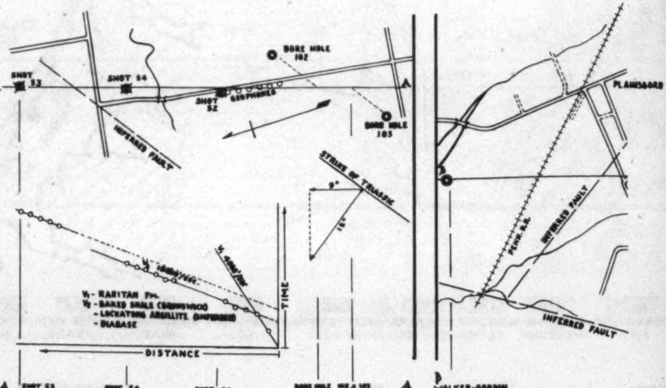
had ever been published that would indicate that Triassic material might underly the Coastal Plain sediments within five miles of the seismic station at Plainsboro. The bore-hole data thus satisfactorily explained the lithologic changes indicated by the seismic data.

The time-distance data, however, could be explained as representing either a fault or a sharp dip seaward. That it was a fault, rather than a marked seaward dip of the Triassic material, seemed logical since all exposures of the Triassic showed a dip of around 15° in the opposite direction and, further, the Triassic was marked by numerous faults. Also a well at the Walker Gordon Dairy a few miles distant showed that under a cover of 30 feet of sediments at least 600 feet of lower Triassic sandstone--the Stockton--was present necessitating the presence of a fault between that point and the station since the shales lie in the upper part of the Triassic geologic column. That the inferred fault between the two outer shot-points at the Plainsboro seismic station could not be the only fault present, however, is shown by two lines of evidence. First, the displacement indicated by the time off-set on the time-distance graph would not permit the observed geologic condition to be accommodated. Second, the similarity of velocities observed on both sides of the break would not indicate any great change in lithology such as would be expected in changing from a baked shale with diabase to an unaltered sandstone. Accordingly another fault is inferred to exist somewhere between the two points, and it has tentatively been placed along the line of the Triassic-Basement contact extending from Trenton through Princeton Junction (see Fig. 3-B).

As to the exact nature of the geological conditions underlying the seismic station, it is to be noted that the first two shots indicate a refraction-horizon essentially parallel to the

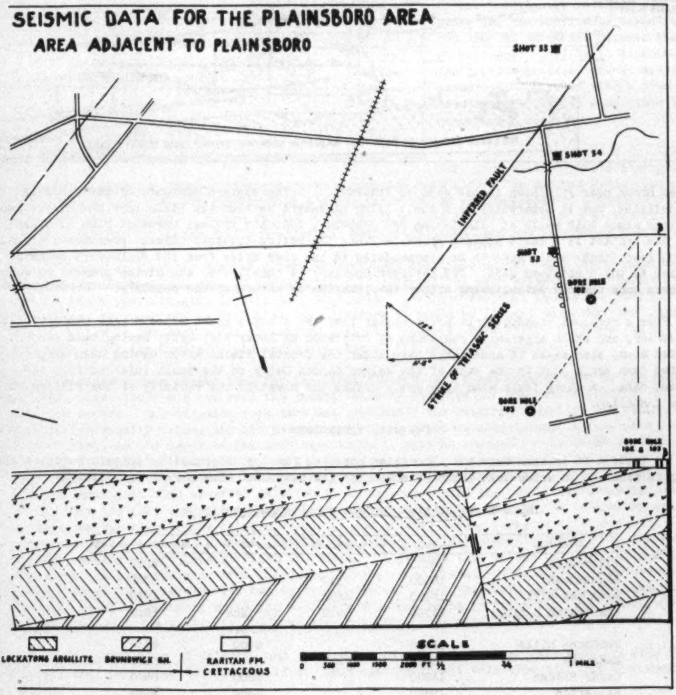
FIG 3—STRUCTURAL ANALYSIS ON GEOLOGIC—

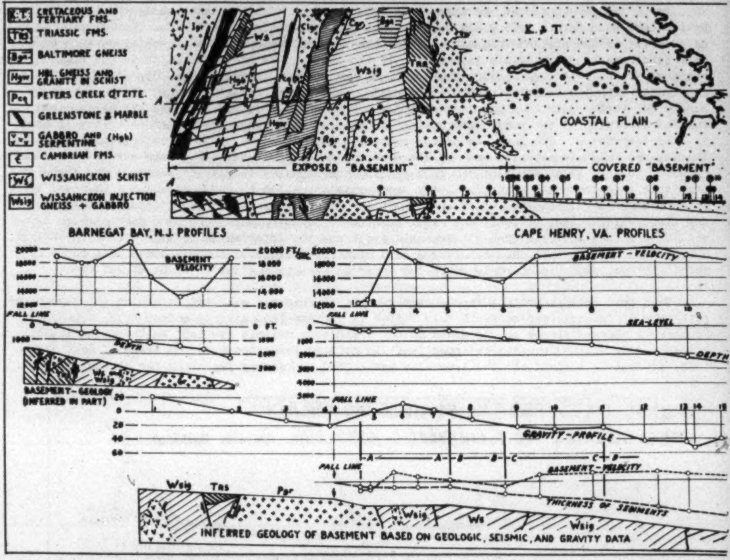
PLAINSBORO SEISMIC REFRACTION-STATION


SHOT 53
SHOT 54
SHOT 52
GEOPHONES
BORE HOLE 102
BORE HOLE 103
A
A
INFERRED FAULT
STRIKE OF TRIASSIC
9°
15°
V1 - RARITAN Fm.
V2 - BAKED SHALE (BRUNSWICK)
- LOCKATONG ARGILLITE (INFERRED)
- DIABASE
TIME
DISTANCE
PLAINSBORO
PENN. R.R.
INFERRED FAULT
INFERRED FAULT
SHOT 53
SHOT 54
SHOT 52
BORE HOLE 102 & 103
WALKER-GORDON
VERTICAL SCALE = HORIZONTAL
WELL-LOG
SEISMIC LOG
DIABASE
STOCKTON SDS.
TRIASSIC

surface of the ground and at the same depth indicated for both the baked shale and the diabase by the bore-hole data. This would suggest that here the sediments are either not dipping at 15° as observed in the exposed Triassic, or else the horizon depicted is a truncated surface of a very hard rock of considerable thickness. If the latter case prevails, which in the light of the regional Triassic geology appears probable, and the diabase intrusion does dip at about 15° in conformity with the sediments which it intrudes, then the thickness of baked shale must be around 500 feet. This is shown in Figure 3-A. This would indicate that the intrusion itself must be something of the same magnitude as the Rocky Hill diabase sheet seen at Princeton which is 1200 feet thick. The location of the intrusion is inferred to follow the regional strike of the exposed Triassic material and, if the bore-holes are projected along this strike to the seismic line, it is at the contact, thus placing the boundary fairly accurately. The throw of the fault inferred between the two outer shot-positions which is shown as about a thousand feet, is an estimated value, since under the conditions it would be extremely difficult to calculate any exact value. That it is of the right magnitude, however, has since been demonstrated by well-data in the critical area between this fault and the inferred fault adjacent to the Walker Gordon Well. These well-data reported a hard black rock believed to be diabase at a depth of about 250 feet below the surface where the inferred structure shows it to be at 235 feet.

That this is apparently a fault-bounded block of Triassic material extending out beyond the main mass of Triassic sediments in this region is further indicated by a bore-hole at Locust Corners, a short distance to the south of the Plainsboro seismic station, which showed typical basement-rock. Although this inferred fault is more or less normal to the trend of most other Triassic faults, a fault of like trend has been noted by Fanshaw [9] along the course of the


SEISMIC DATA FOR THE PLAINSBORO AREA
AREA ADJACENT TO PLAINSBORO
SHOT 53
SHOT 54
INFERRED FAULT
SHOT 52
BORE HOLE 102
STRIKE OF TRIASSIC SERIES
BORE HOLE 103
BORE HOLE
LOCKATONG ARGILLITE
BRUNSWICK SH.
RARITAN FM.
CRETACEOUS
SCALE
0 500 1000 1500 2000 FT. 1/2 3/4 1 MILE


K&T CRETACEOUS AND TERTIARY FMS.
Trs TRIASSIC FMS.
Bgn BALTIMORE GNEISS
Hgw HBL. GNEISS AND GRANITE IN SCHIST
Pcq PETERS CREEK QTZITE.
GREENSTONE & MARBLE
GABBRO AND (Hgb) SERPENTINE
Є CAMBRIAN FMS.
Ws WISSAHICKON SCHIST
Wsig WISSAHICKON INJECTION GNEISS + GABBRO
K & T
COASTAL PLAIN
EXPOSED "BASEMENT"
COVERED "BASEMENT"
A
BARNEGAT BAY, N.J. PROFILES
CAPE HENRY, VA. PROFILES
BASEMENT VELOCITY
BASEMENT - VELOCITY
20000 FT/SEC.
FALL LINE
SEA-LEVEL
DEPTH
BASEMENT-GEOLOGY (INFERRED IN PART)
GRAVITY-PROFILE
BASEMENT-VELOCITY
THICKNESS OF SEDIMENTS
Wsig
Trs
Pgr
Ws
INFERRED GEOLOGY OF BASEMENT BASED ON GEOLOGIC, SEISMIC, AND GRAVITY DATA

Stoney Brook near Princeton in the exposed Triassic. The eastern boundary of the block is not definite, but it apparently is a fault lying somewhere between the Plainsboro and Hightstown stations since both wells at Hightstown and Jamesburg indicate typical basement-rock at depth. That the contact is a fault appears probable since the entire Triassic column--presumably around 10,000 feet thick--would have to be accommodated in the five miles from the Plainsboro seismic station to the Hightstown Well. The northern boundary is indefinite, and at the present no work has been done towards establishing either the location or nature of the boundary.

From a regional standpoint it would appear that the diabase sheet exposed near Lambertville, New Jersey, and which apparently reappears at Princeton as Rocky Hill after having been step-faulted down, also makes it appearance here under the Coastal Plain, after having been step-faulted down again just to the east of the Walker Gordon Dairy on the fault inferred from the present data. A minor fault also apparently breaks the sheet in the vicinity of the Plainsboro seismic station.

Velocity-variations

In Table 1 it is seen that the velocities observed for the intermediate seismic horizons are far more constant in value than those observed for the basement. The spread in basement-

Table 1--Velocities for observed seismic horizons

Station	Layer B	Layer M	Layer V	Surface-layer
Plainsboro	18800			4000
Hightstown	18500	7140		5300
Disbrows Hill	18100	6400		5480
Charleston Springs	21060	6970	Surface-outcrop	5610
Jacksons Mills	16000		6000	5150
Lakewood	13000	7900		5575
Cedar Bridge	14200		6200	5400
Silverton	18800		6350	5350

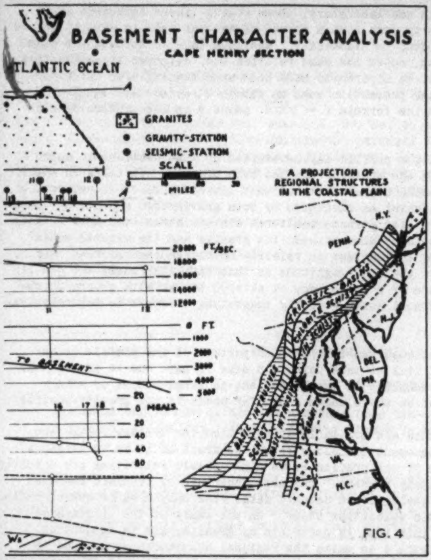

BASEMENT CHARACTER ANALYSIS
CAPE HENRY SECTION
ATLANTIC OCEAN
A
GRANITES
GRAVITY-STATION
SEISMIC-STATION
SCALE
0
10
20
30
MILES
A
11
12
15
16
17
18
A PROJECTION OF REGIONAL STRUCTURES IN THE COASTAL PLAIN
N.Y.
PENN.
N.J.
DEL.
MD.
VA.
N.C.
TRIASSIC BASINS
CHLORITE SCHIST
W. SCHIST
BASIC INTRUSIVES
SCHIST
BASIC INTRUSIVES
GRANITE
20000 FT./SEC.
18000
16000
14000
12000
11
12
0 FT.
1000
2000
3000
4000
5000
TO BASEMENT
20
0 MGALS.
20
40
60
80
16
17
18
Ws

FIG. 4

velocities ranges from 13,000 to 21,000 feet per second. The fact that the distribution of these velocity-variations is systematic led the authors to reinvestigate the velocities observed in Virginia, and it was found that the same spread was observed there and also that a systematic distribution appeared to be present. This indicated that there existed a possible regional structure extending up the coast under the cover of the Coastal Plain sediments which the basement-velocities reflected. There was no apparent relation between the depth and the velocity-variants, and the phenomenon appeared to be dependent upon the physical nature of the basement-rock.

To throw additional light on this problem the gravitational data that had been determined for the Virginia seismic profile were examined and it was found that apparently a solution to the problem lay in these data. Swick [2] had previously commented on the lack of agreement between the seismic depth-profile in Virginia and the gravity-profile, and it was this disagreement coupled with a study of the local geological conditions that suggested a solution.

As mentioned earlier in this paper, four of the gravity-stations were established upon the exposed basement-rocks across the Fall Line. The surface geological conditions showed that a marked gravity-high was obtained over an area composed of Wissahickon Schist which had been altered by gabbro intrusions to an injection gneiss, and that a marked gravity-low was obtained for those stations which had been established upon the Petersburg granite.

Although no specific gravity-determinations were available on the rocks under consideration, the gravity-observations were in concordance with observations made elsewhere on similar rocks and for which specific-gravity data were available. Geologic evidence showed that the younger granite had intruded the older schist and therefore it would be logical to expect the altered schist to lie to the east of the intruding granite. However, here direct evidence was not available since the Coastal Plain sediments covered the postulated contact.

The seismic data showed that for the first station, which was located just to the east of the Fall Line, there was observed the lowest velocity recorded for the entire profile--12,780 feet per second. The basement-rock here was presumably the Petersburg granite indicating that perhaps low gravity-values and low seismic velocities might be correlated. At the next three seismic stations the depth to basement was constant, with no changes in thickness of sediments or in their character sufficiently marked to affect gravity-determinations, and it is significant that over this area there is a definite relation between the seismic velocities and the gravity-anomalies. The observed relation is in agreement with the postulated geologic conditions, that is, that the altered Wissahickon Schist lies to the east of the Petersburg granite, as shown by the high in the seismic velocity- and gravity-curves. Beyond this point the gravity reflects the two controls present, namely, the density of the basement-rock and the thickness and low density of the Coastal Plain sediments. It is seen that as the thickness of sediments increases and over the same area the velocity rises, the gravity holds constant, reflecting a balance established between the two opposite effects. Over that area in which the basement-velocity remains constant, while the sediments are increasing in thickness, the gravity drops off showing that the latter is the predominant control over this portion of the section.

This correlation of velocity and density has been noted by ther investigators [6], but to the writers' knowledge this is the first field-correlation of this type that has ever been presented.

Ide [7], in his experimental work in the laboratory, shows that a linear agreement apparently exists between elasticity and density such that a change of two per cent in density may be expected to give a change around 40 per cent in elasticity. Woollard [8], in testing the elastic properties of Georgia building stones, noted the same relation and, although in detail this relation does not hold for all materials, it apparently does hold when dealing with materials having marked changes in density and other properties such as fabric structure are equal. The velocity-correlation is evident from the formula $V = \sqrt{E/d}$, where E is the modulus of elasticity and d is the density.

In terms of geology it appears that the profile may be divided up into sections in which the basement-lithology may be inferred on the basis of the two sets of data. In the area where the depth of sediments is constant, it would appear that basement-lithology is first Petersburg granite followed by altered Wissahickon Schist as indicated by both gravimetric and seismic data. The next section would indicate that here lies perhaps unaltered schist, since both sets of data show a lower density material of intermediate value between the granite and the altered schist. The following section is characterized by an increase in velocity which remains constant over most of the balance of the profile and of the same magnitude as that indicated where the gravity was also high. This suggests the presence of a large body of altered schist with gabbro intrusions--the outer stations with their somewhat lower velocity suggesting a return to the relatively unaltered schist conditions.

The marked drop in the gravity-value observed in the outer portion of the profile is believed to be due to the lateral shift of line at this point and also in part due to inherent defects of the present method of gravity-reduction in areas adjacent to large bodies of water. This also holds for the last gravity-station which shows a similar break in the gravity-profile.

Although no supplementary gravity-data are available at this time for the New Jersey profile, the problem of the nature of the basement-lithology is not as great as it is in Virginia. Well-data indicate that for that portion of the profile where high seismic velocities are observed the altered Wissahickon Schist is definitely present. The area underlain by Triassic material has already been differentiated on both geologic and seismic data. The only area of questionable lithology is that in which the low seismic velocities occur. On the basis of the Virginia observations it is highly suggestive that this area is underlain by granite, and in support of this postulation a sketch is shown in Figure 4 in which the regional structures as exposed in the Piedmont are extended up the coast, and in the Coastal Plain area those structures disappearing under it in the south and which apparently reappear in the New England province are projected. It is to be noted that if Carboniferous granites do exist as a belt between the Petersburg granite of Virginia and the Branford granite of Connecticut, then they should pass through that part of New Jersey in which the area of low seismic velocities is observed for the basement.

Conclusions

The examples that have been presented in this discussion of the application of geophysical methods in investigating the structure of the Coastal Plain have, it is believed, fully demonstrated the reliability of results obtained from such methods of investigation.

Further, the advantage and importance of having supplementary information of a diverse nature in carrying out such investigations has been shown. In the interpretations and correlations made between the seismic and geologic data in the Plainsboro area, the results presented could not possibly have been arrived at using either the geologic or seismic data alone, whereas used in conjunction with each other what is believed to be an adequate interpretation representative of the true conditions has been presented. In the interpretation of the seismic velocity-variations observed both in New Jersey and Virginia, it was only through the supplementary gravimetric and geologic data that were available that an interpretation became at all possible. In this case any two sets of data could not possibly have yielded the results obtained through a consideration of all three. The results, although they do not afford definite proof, are regarded as being highly suggestive, and it is believed that an additional seismic profile contemplated for the near future will shed further light on this problem.

Although the benefits to be derived through the close and active cooperation of scientific groups and governmental agencies do not, in the light of the present investigation, require further comment, the authors wish to emphasize in conclusion the fact that the positive results presented, and the much-needed light that was shed on previous work, could only have been presented as negative results and as another group of perplexing, unexplainable data had the cooperation enjoyed been lacking.

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MICROSEISMS RECORDED AT BERKELEY

Perry Byerly and James T. Wilson

The cause and nature of microseisms have been the subject of much study since the beginning of instrumental seismology. As yet, however, there is no uniformity of opinion on either the nature of the movement or the cause.

The periods of microseisms vary from a fraction of a second to a minute or more, and the amplitudes are frequently many microns. There is one class of microseisms with a period-range of four to ten seconds that is recorded all over the world on seismographs with appreciable magnification for motions of these periods. Only microseisms of this class will be considered in this discussion.

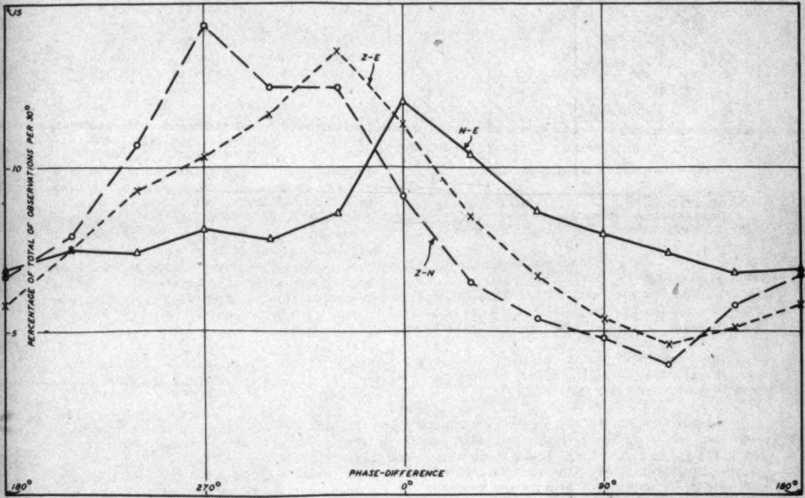

FIG. 1—DISTRIBUTION OF PHASE-DIFFERENCES FOR A TYPICAL CASE, BERKELEY, 17ʰ TO 3ʰ, PACIFIC STANDARD TIME, JANUARY 27-28, 1937 (525 OBSERVATIONS)

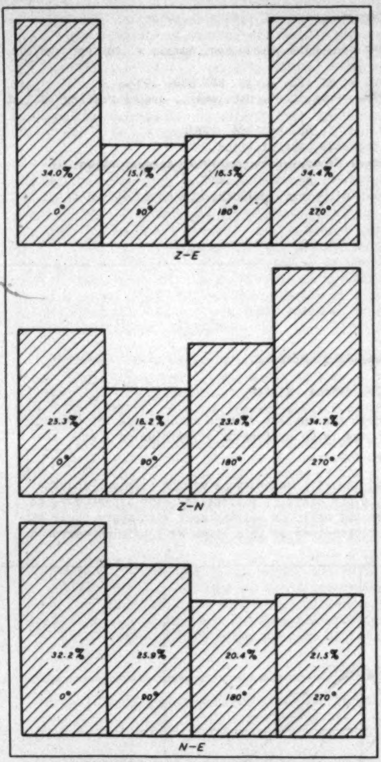

FIG. 2—DISTRIBUTION OF PHASE-DIFFERENCES, BERKELEY, OBSERVATIONS FROM 10 DIFFERENT MICROSEISMIC STORMS, 1935-1937 (2700 OBSERVATIONS)

While these microseisms exhibit a certain uniformity of motion and records of them are being written continuously by sensitive seismographs, the exact nature of the motion is not readily apparent. It therefore seems necessary to study the nature of the motion statistically using a large number of observations. The use of statistical methods in seismology has recently come into vogue. As it is frequently applied, however, there is no large amount of data. It appears from recent papers that it is now considered legitimate practice to apply such methods even with sparse data. However, it can scarcely be denied that the more data available, the more legitimate the conclusions. In this study of microseisms we decided that it would be highly desirable to have a great many readings. In this we were facilitated by the assignment of a Works Progress Administration assistant, who is devoting a year to the measurement of microseisms on our records. This work is still in progress. When it is completed we should have sufficient data that there will be no question as to whether or not there are enough to justify the use of statistical methods.

Microseisms have been variously attributed to free vibrations of the surface-layers in the immediate vicinity of the recording station, to surface-waves arriving from a source at some distance, to body-waves, and to the effect of atmospheric pulsations in the immediate vicinity of the station. That they are surface-waves and in particular, because they have a vertical component, are Rayleigh waves, seems to be the most generally accepted view.

Now if microseisms are Rayleigh waves arriving from a single source, the three components, north-south, east-west, and vertical, should show definite phase-relationships. For Rayleigh waves the path of a particle is an ellipse in a plane containing the source, observing point, and the center of the Earth, and the motion is retrograde to the direction of propagation. The vertical component should then lead the horizontal by 270° or 90°. For a homogeneous medium the amplitude of the vertical component should be about one and one-half times the horizontal; however, surface-layering may alter this considerably. At Berkeley the average ratio of the vertical amplitude to the horizontal is about one. It is, however, a function of the period, being about 1.7 for periods of four seconds and decreasing to about 0.7 for periods of seven and one-half seconds.

The phase-relationships of the microseisms recorded on the Wilip-Galitzins at Berkeley have been investigated using the method devised by Leet [see 1 of "References" at end of paper] and later used by Lee [2] and Archer [3].

The microseisms are assumed to be a simple harmonic motion, the phase of which is read on each component at successive minute-breaks. There is then no doubt that the measurements are being made at the same time on all components. The phase is estimated to the nearest 30°. The phase-differences between the various components are then formed and tabulated for 30° intervals of phase-difference. If there is any tendency for one phase-difference to predominate, such a statistical method should reveal it.

As the three components of the Wilip-Galitzins at Berkeley have similar free periods and damping, phase-comparisons may be made directly without any correction for instrumental constants. Readings were taken only at those minute-marks where the amplitudes were large enough for the phase to be estimated with some accuracy. This reduced the number of phase-comparisons per hour to about 40.

In all ten different intervals of large microseisms have been studied. The lengths of the intervals during which comparisons were made varied from two to 36 hours and the total number of comparisons made is about 2,700. The percentage of the total number of observations per 30° were plotted for each case.

In Figure 1 the distribution of phase-differences for a typical case is shown. During this interval, 17^h January 27 to 3^h January 28, 1937 (Pacific standard time), the microseisms had a maximum amplitude of about three and one-half microns and a period of about eight seconds. The nearest large barometric low was about 400 km off the coast west of Oregon. An examination of the phase-difference-frequency diagram shows a slight tendency for the horizontal components to be in phase and the vertical to lead the north-south by 270° and the east-west by 330°. If the microseisms are assumed to be Rayleigh waves, this would indicate a source to the southwest. However, as may be seen from the diagram, the predominance of these phase-differences is very slight. While the various intervals studied showed slightly different characteristics, the main features are as in the example illustrated. The horizontal components are usually in phase and 240° to 330° behind the vertical.

Figure 2 is a histogram showing the distribution of phase-differences by quadrants for all ten intervals studied. It can be seen that, while the predominance of any phase-difference is not pronounced, there is an indication of Rayleigh waves from the southwest.

At the present time reports on the strength of the surf at three different times through the day are being received from three points on the California coast near Berkeley. These are to be compared with the microseisms at Berkeley in an attempt to find a source.

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University of California,
Berkeley, California

SEISMIC PHENOMENA AS REVEALED BY QUARRY-BLASTING

J. R. Thoenen and S. L. Windes

For the last three years the Bureau of Mines has been measuring ground-vibrations from quarry- and mine-blasts. The construction of the seismometers used in these measurements has already been described [see 1 of "References" at end of paper] by the Bureau of Mines, and will not be discussed here.

In general, observations by the Bureau include the recording of a seismic wave at three different points simultaneously. At each of these points, two mutually perpendicular horizontal and one vertical component of motion are measured. The three measuring points or stations are seldom over 500 feet apart and in many cases only a few feet apart. This short spacing of stations and the close control possible with dynamite-shots are features seldom met in teleseismic work.

The Figures prepared for this paper deal with the actual seismic motion caused by a dynamite-blast. It is felt that the data presented herewith are of a fundamental nature because they show just how the ground moves under certain seismic conditions.

The Figures have been divided into two groups, one of which deals with the motion of a particle at the recording point, and the other with the wave-shape as a function of the locality of the source.

Figure 1 shows one of the vertical seismometers used in these tests. A typical quarry set-up is shown in Figure 2 in which a horizontal seismometer is indicated by x and a vertical seismometer by y.

(1) Motion of a particle at the recording point--Figure 3 represents the trace of a particle as seen from two views. This motion resulted from a 13,400-pound shot, 3,600 feet distant. The time taken by the particle to travel the trace shown was one second. The motion during the following second is shown on Figure 4. From these it is seen that the motion is quite complex.

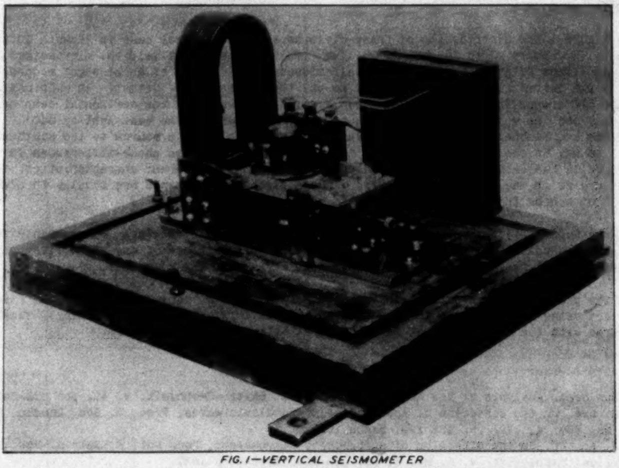


FIG. 1—VERTICAL SEISMOMETER

FIG. 2—TYPICAL QUARRY SEISMOMETER-STATION

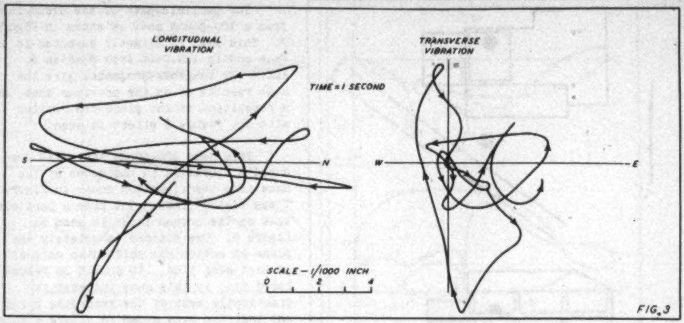


FIG. 3

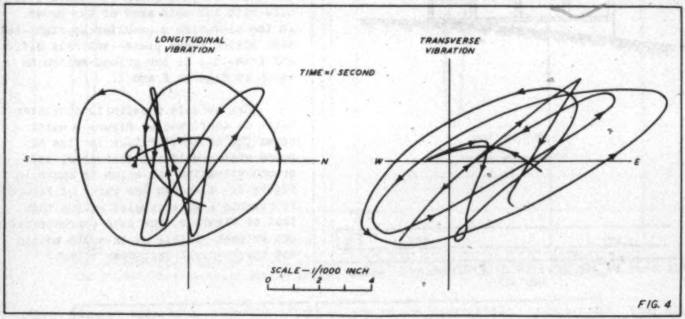


FIG. 4

FIG. 3—PARTICLE-PATH FOR ONE SECOND RESULTING FROM A 13400-POUND SHOT 3600 FEET DISTANT
FIG. 4—PARTICLE-PATH FOR THE SECOND FOLLOWING THAT OF FIGURE 3

In spite of the complexity of these traces they are indicative of both the physical characteristics of the medium at the recording station and the direction from which the disturbance came. For example, Figure 5 is a sketch of the relative positions of instruments and an adit face being blasted Recording Station A is on the ground in front of the house, whereas the other station is on the second floor.

Figure 6 shows the particle-path at Station A caused by a 60-pound shot made in the adit 70 feet below. Note that the upper view, which is a plan, indicates that the direction of disturbance was approximately southwest of the recording point. A survey locating the shot confirmed this. In fact, the tangent to the curve at the origin, if extended, cuts within a few feet of the shot-location.

From the lower view, or south elevation, in Figure 6 one would conclude the initial vibration arrived from a point almost below the station. If the tangent to this curve is constructed it falls between the actual shot-location and a point directly under the station. As the shots are in rock and the station on overburden, a refraction of the waves is expected and it is likely that the tangent does not intersect the shot-point simply because of the bending of the seismic waves.

The identifying characteristics first noted on Figure 6 are the restriction of motion to essentially one plane for the first (and major) cycle and the figure 8 effect in this plane as seen in the elevation. Also the motion becomes erratic after the first three-fourths of a cycle.

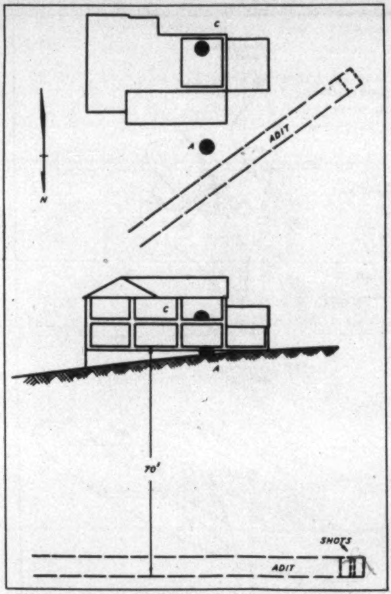


FIG. 5—PLAN AND SECTION OF EXPERIMENTAL HOUSE AND ADIT

The particle-path of the disturbance from a 100-pound shot is shown in Figure 7. This shot is slightly advanced in the face and is 103 feet from Station A. Again the tangents projected give the same results as on the previous shot, and a repetition of the plane-restriction with the figure 8 effect is seen.

This same 100-pound shot also produced a vibration in the house at the same time the vibration shown in Figure 7 was taking place. The path a particle took on the second floor is seen on Figure 8. One notices immediately the plane of motion has shifted to more of an east-west line. It should be remembered that in this case the shot is practically west of the recording point and that the axes shown in Figure 8 coincide with the main axes of the house. In the elevation a peculiar up-right-left-down motion takes place--entirely different from that of the ground-motion as shown in Figures 6 and 7.

That this is a definite characteristic is confirmed by Figure 9 which shows the house-vibration for the 60-pound shot previously discussed, the ground-vibration for which is shown in Figure 6. Although the curve of Figure 9 represents a more complex motion than that of Figure 8, the same characteristics are evident--little north-south motion and the up-right-left-down effect.

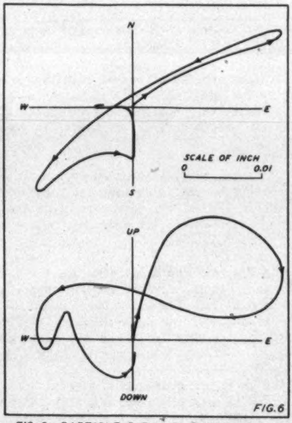


FIG.6

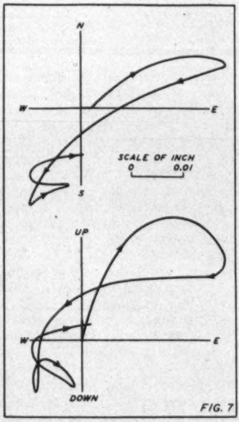
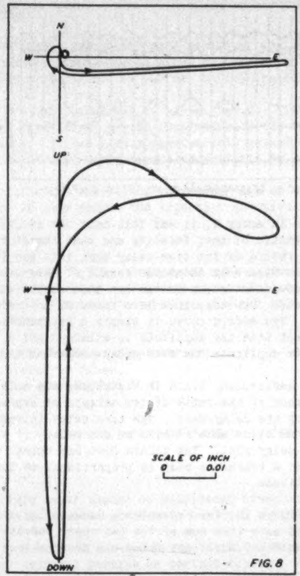


FIG. 7

FIG. 6—PARTICLE-PATH AT GROUND, STATION A, 60-POUND SHOT 378
FIG. 7—PARTICLE-PATH AT GROUND, STATION A, 100-POUND SHOT 382



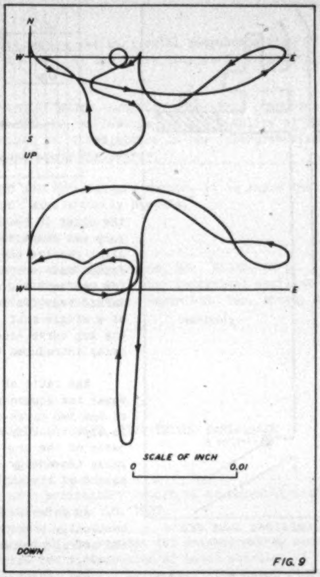


FIG. 8—PARTICLE-PATH AT HOUSE SECOND FLOOR, STATION C, 100-POUND SHOT 382
FIG. 9—PARTICLE-PATH AT HOUSE SECOND FLOOR, STATION C, 60-POUND SHOT 378

These Figures apparently show the effect of the house-construction. Furthermore, they suggest a method of locating the direction of the disturbing source by means of tangents drawn to the initial part of the curve when only one recording station is available.

(2) Wave-shape as a function of the locality of the source of vibration--In general, the wave-shape recorded from a blast depends upon the stratigraphic and geometric relation between the recording station and the shot-point. The Figures that follow will show how a particular wave-shape may identify a certain locality.

Figure 10 is a sketch of the test-area in a limestone mine. The stratum being mined is horizontal, approximately 20 feet thick, and is 200 feet below the surface in the area shown. In entry 1 six different kinds of dynamite were used in charges ranging from 26 to 80 pounds. A total of 20 shots was made in this entry. Figure 11 shows a typical seismic record. On this record one can note on the third curve from the top (vertical vibration at Station A of Figure 10) that the first negative or downward loop predominates in the first few hundredths of a second. It was found that this particular characteristic occurred on every record of the vertical seismometer from shots in entry 1 except two time-delay shots and no shot other than those in entry 1 showed this feature, yet over 100 records were made from shots within 500 feet but outside of entry 1. Moreover, the same characteristic appeared in the record of every shot in the cross-cut numbered 2; however, in the case of the cross-cut shot-records the second positive loop did not cross the zero-axis. It so happened that one shot listed as in cross-cut 2 did have the second positive loop crossing the axis but reexamination of the field-notes revealed that it was the initial shot that started the cross-cut and therefore was really a shot in entry 1.

There were two time-delay shots in entry 1 that did not produce the characteristic loop. These time-delay shots were made with a delay-blasting machine and were essentially two distinct blasts fired within a few thousandths of a second of one another. In view of the evidence of

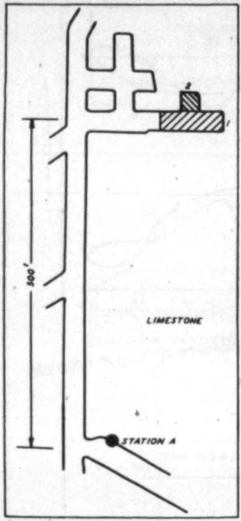


FIG. 10—LIMESTONE MINE TEST-AREA

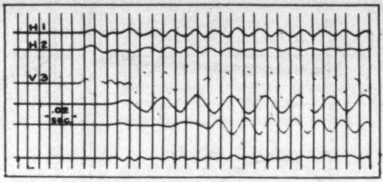
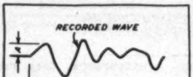


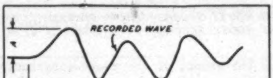
FIG. 11—TYPICAL SEISMOGRAM OF BLAST IN ENTRY 1

the other 18 shots in entry 1, it was felt that the identified loop was characteristic of that locality and that therefore one might resolve the record of the time-delay shot into the fundamental wave-shape. Figure 12 shows the result of this analysis for the vertical component which is the one that exhibited the marked negative loop. The top curve is a trace of the record of a single shot. The second curve is simply a reproduction of the top curve except that the amplitude is enlarged and a time-delay introduced to duplicate the time-delay shot-conditions.

The ratio of amplitudes, 1:1.5 in Figure 12, was made to equal the square root of the ratio of the weights of explosive of the two parts of the delay-shot. The time-delay introduced is approximately the difference in time of detonation of the parts of the time-delay shot. The square root was chosen because the energy of a vibrating body is proportional to the square of its amplitude.

As shown in Figure 12, the resemblance between the synthetically produced wave (the sum of the two upper curves) and the actual recorded wave, while not exact, is nevertheless





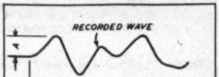


FIG. 12—SYNTHETIC WAVE FOR VERTICAL VIBRATION, SEISMOMETER V3
FIG. 13—SYNTHETIC WAVE FOR HORIZONTAL VIBRATION, SEISMOMETER H2
FIG. 14—SYNTHETIC WAVE FOR HORIZONTAL VIBRATION AT RIGHT ANGLES TO THAT OF FIGURE 13, SEISMOMETER H1

striking.

A repetition of the same method of analysis on the two horizontal components also produced remarkable resemblance as shown in Figures 13 and 14. As the three components duplicate, the resultant motion will, of course, be duplicated.

From this analysis it seems that the time-delay shots, although apparently producing different wave-forms, may be resolved into the fundamental wave-shape for the locality of the disturbance. It has been suggested this method might be of assistance in the interpretation of deep-focus earthquakes where very little is known about the source.

While the results of this paper are of the most preliminary nature, it is hoped that they answer in some degree the important question of "what actually happens?".

References

[1] George A. Irland, A study of some seismometers, Tech. Paper 556, Bur. Mines, 48 pp., 1934. See also F. W. Lee and George A. Irland, Construction of master mechanical oscillator for testing seismic recorders and other allied apparatus, Tech. Paper 518, Bur. Mines, 17 pp., 1932.

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THE EPICENTER AND P-CURVE OF THE APRIL 16, 1937, FIJI ISLAND EARTHQUAKE

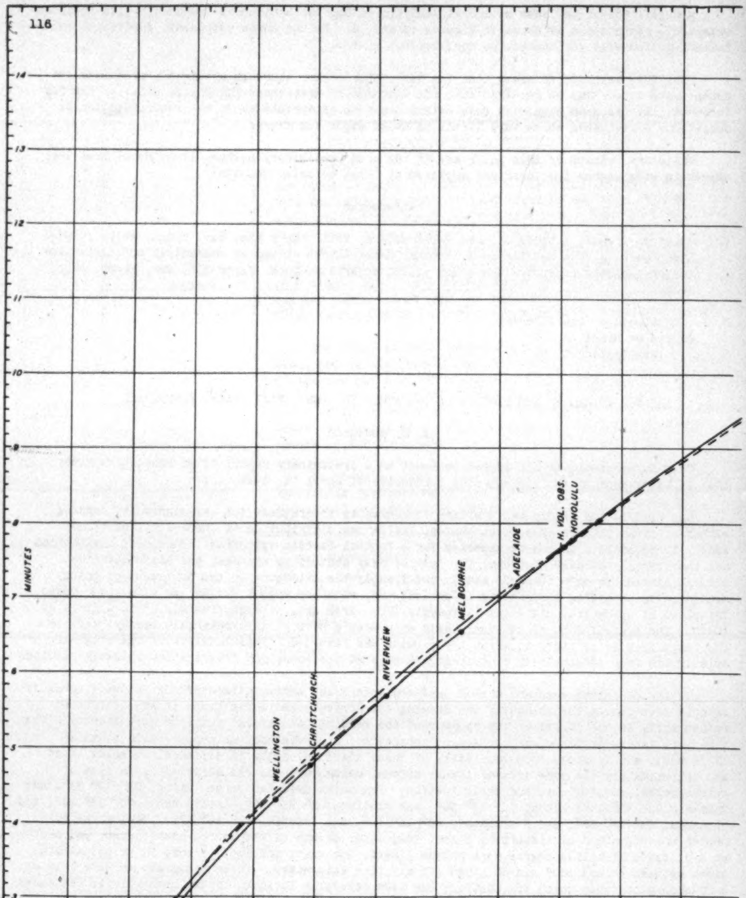
A. J. Westland

This paper should be considered somewhat as a preliminary report of an exhaustive study that is being made of the violent deep earthquake of April 16, 1937.

One hundred and twenty-two stations responded to the request for seismograms by sending either original records or contact-copies. While the distribution of these stations is not ideal, it is as good as can be expected for a Central Pacific epicenter. The chief limitations are the scarcity of near stations; the lack of near control to the east and southeast (the closest station is more than 90° away); and finally the existence to the northwest of mostly mechanically recording instruments or optical ones with low magnification and slow paper-speed. The range of epicentral distances is roughly 155°, from Apia at approximately 8° to Algiers at 164°.8. The present report, however, deals only with a part of this material, namely, that in the range from 8° to 115°, in which the P-phase was recorded. This consists of the original seismograms from 46 stations, contact-copies from 20 stations, and five station-bulletin readings.

Of the 66 seismograms and copies just mentioned, the arrival times of only 41 were actually used in determining the epicenter and drawing the arrival-time curve since it was discovered rather early in the course of the study that the readings of several stations were doubtful, the time unreliable, or the clock-correction uncertain. A provisional epicenter, 20°.0 south at 177°.8 west, was obtained from four pairs of these stations, lying in different azimuths, that had approximately the same arrival-times, account being taken of the differences in type of seismographs, sensitivity, and amplification. The pairs selected were: Ukiah, $03^h\ 13^m\ 03^s$, and Zikawei, $03^h\ 13^m\ 04^s$; Sitka, $03^h\ 13^m\ 36^s$, and Phulien, $03^h\ 13^m\ 38^s$; Little Rock, $03^h\ 13^m\ 40^s$, and Huancayo, $03^h\ 13^m\ 39^s$; and Florissant, $03^h\ 14^m\ 45^s$, and Irkutsk, $03^h\ 14^m\ 47^s$. Taking the epicenter so determined as a starting point, four other points to the north, south, east, and west at a distance of half a degree were chosen. Next, the exact distance to some 30 principal stations between 8° and 100° was obtained for all five epicenters. These measurements were made on a 30-inch globe upon which the stations had been carefully located. These great-circle distances are believed to be accurate to within 0°.2. With these arcual distances as abscissas and the arrival-times as read from the original seismograms as ordinates, arrival-time curves were drawn for all points thus chosen. In order to better bring out the irregularities in the curves a very large scale, 2.5 cm to the degree and 15 cm to the minute, was used which meant a P-curve some ten feet in length. The results indicated that a shift to the southeast would give a smoother curve so the epicenter 20°.5 south at 177°.3 west was next chosen.

The P-curve of this epicenter was then compared with the curves of Brunner and Gutenberg-Richter drawn to the same large scale. Some difference in curvature appeared so further shift-


14
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MINUTES
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WELLINGTON
CHRISTCHURCH
RIVERVIEW
MELBOURNE
ADELAIDE
H. VOL. OBS.
HONOLULU
DISTANCE
5°
10°
15°
20°
25°
30°
35°
40°
45°
50°
55°

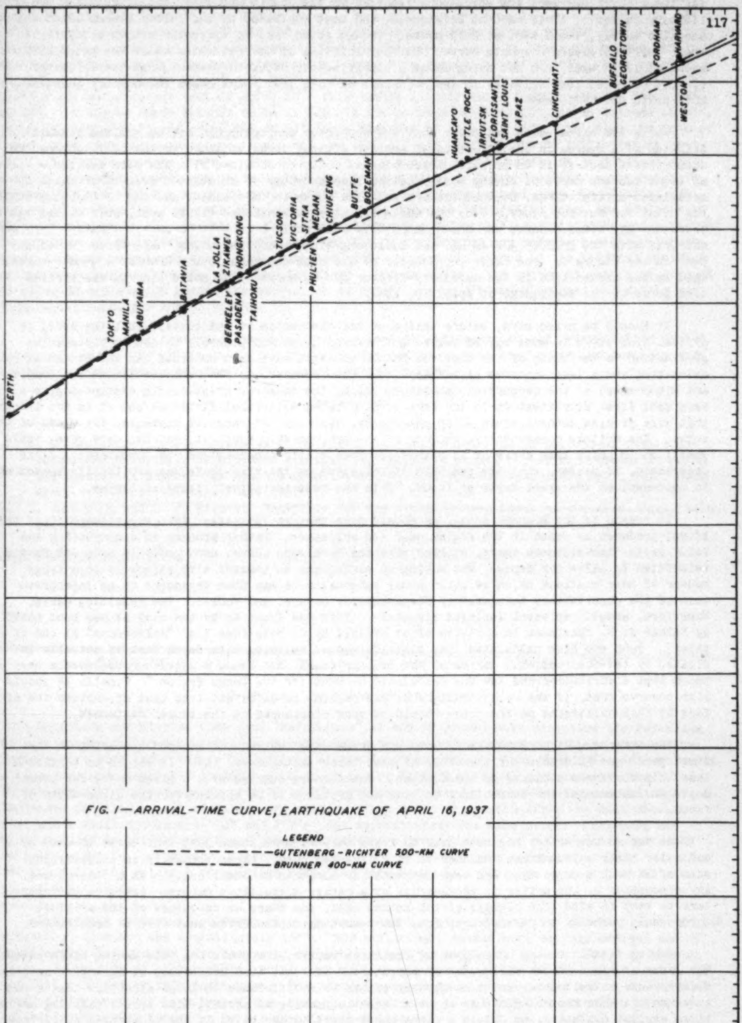

FIG. 1—ARRIVAL-TIME CURVE, EARTHQUAKE OF APRIL 16, 1937

ing of the epicenter was made in an endeavor to obtain a better fit. The points of departure for these shiftings were the epicenters reported in the Jesuit Seismological Association Preliminary Bulletin, 179°0 east at 22°2 south, and that published by the United States Coast and Geodetic Survey, 174°0 west at 22°0 south. It was found that no epicenter within a degree of these points produced a smooth curve. Further shifting of the epicenter about the point 20°5 south at 177°3 west, did not bring about a significantly better agreement; but the epicenter finally selected lies slightly to the northwest of this last point named and has the coordinates 20°3 south and 177°5 west.

While the writer does not claim that the epicenter adopted could not be shifted a small fraction of a degree in one direction or another without doing violence to the observations, he does believe that it is as probable a location as could be obtained with the data available. It at least has the merit of having been obtained independently of existent travel-time tables or calculated arrival-times, so that the arrival-time curve is independent and can be made the starting point for further study. Once the epicenter already mentioned (177°5 west, 20°3 south) was selected, the final P-curve was drawn, this time on a scale 1 cm = 1°, 6 cm = 1 minute. This was compared with the Brunner 400-km and the Gutenberg-Richter 500-km curves. All three curves appear in the Figure 1. The curve represented by the dash-dot-dash is the Brunner 400-km P-curve, that by the broken line is the Gutenberg-Richter 500-km curve. The solid line is the arrival-time curve of the earthquake of April 16, 1937.

It should be noted here, before taking up the discussion of the curves, that the April 16 arrival-time curve is what may be called a free-hand curve and possibly is not as close an approximation to the locus of the observed points as might have been obtained had the distances been calculated and a least-squares method applied. Due, however, to the relative dimensions (major and minor axes) of the Geophysics Laboratory globe, the measured great-circle distances give a very good first approximation to the true or calculated epicentral distances and it is not thought that more precise methods would alter materially, over any 10°-range of distance, the slope of the curve. The writer, consequently, because of the surprisingly good fit that the data give, feels justified in using this curve as it stands for preliminary observations and comparison. It is understood, of course, that the position of the curve on the time-scale was arbitrarily chosen and is dependent on the exact depth of focus. This has been tentatively fixed at 418 km.

In regard to the Brunner curve, we should note that it is weakest from the standpoint of the effect produced by depth in the region near the epicenter. Father Brunner in constructing the focal depth-time-distance chart, started with the Macelwane curve, used Wadati's data and Dahm's velocities to allow for depth. Not having an earthquake so located with reference to a large number of near stations which he could study personally he was thus dependent on an interpretation of the observations tabulated by Stechschulte, Scrase, and Wadati. The resulting curve, therefore, should represent Japanese structure. This was found to be the case as has been noted by Father J. B. Macelwane in a review of an article by C. Bois [see 1 of "References" at end of paper]. Dahm and Blum calculated "the distribution of velocity with depth that is actually indicated by the observational curves of the Brunner chart, and found a close correspondence between that distribution and the one calculated by Dahm for the Tango Region." Finally we should also observe that, if the deep structure of one region is different from that of another the effect of this difference on the curve should be more pronounced at the lesser distances.

The Gutenberg-Richter 500-km P-curve was drawn with the data of Table 6 appearing in the first paper on "Materials for the study of deep-focus earthquakes" [2]. It should be mentioned that this curve was obtained by calculation. The authors started with a given curve for normal depth and calculated the corrections in time and distance to be applied for the given depth of focus.

As far as the writer has been able to learn they nowhere state just what curve is used as a basis for their calculations, whether it is an average curve, based primarily on observational data or on such a curve that had been corrected or smoothed mathematically. As a consequence, any discussion by the writer is necessarily of a rather speculative nature. Still it is important to keep in mind that because of the method used, the shape or curvature of the original curve would normally be carried over into the resulting depth-curves that were so constructed.

Coming finally to the comparison of the three curves, there are two outstanding differences. The first of these is the difference of curvature. If real, this difference of curvature bespeaks a difference in velocities and consequently points to a difference in local structure that seems to extend to some depth. Since as is well known in drawing an arrival-time curve, plotting arrival-times against distances, we obtain a curve whose slope at any point is the reciprocal of the apparent speed over the curved surface of the Earth, not only depth of penetration of the ray but velocity of the ray at various depths is involved.

The difference of curvature between the three curves stands out more clearly if we compare them section by section. The April 16 curve starts out at 8°.5, ten seconds lower on the time-scale than the Gutenberg-Richter 500-km curve. It approaches and practically coincides for 13° (30° - 43°), crosses between 43° and 45°, and parallels the 500-km curve, for 20° (46° - 66°) lying some two seconds higher. From 69° to 72° the two curves again coincide but beyond 72° separate rather rapidly--the solid-line curve, lying five seconds higher at 85°, eight seconds at 100°, and some eleven seconds at 115°. A smaller variation in curvature is found between the Brunner 400-km curve and that of April 16. This latter is noticeably flatter for the first 45° to 48°. It starts three seconds below at 8°.5, is six seconds below at 15°, seven seconds at 30°, three seconds at 44°, and coincides at 50° for a distance of 10°. At 65° it is below the Brunner curve again some two seconds and then continues more or less parallel out to 120° being at that point some three seconds lower. The difference in the shape of the curves is, all things considered, remarkably small and while there seems to be a slightly faster velocity represented in the smaller slope of the April 16 arrival-time curve near the epicentral area, the limited number of points available to determine the shape of the curve in this region makes one hesitate to be too confident in any assertion. The second outstanding difference in addition to the variation in curvature is the difference in relative position of the Brunner and Gutenberg-Richter-curves. It is somewhat surprising that there should be such a marked difference in the relative position of these two curves, the one a 400-km, the other a 500-km curve. The writer is unable at present to offer an explanation beyond the suggestion of dependence on different assumed velocities and fundamental curve for normal shocks.

It is hoped with continued investigation and further refinement in the determination of the epicenter, depth, and origin-time, to obtain an even better arrival-time curve and possibly discover more about the velocities of the Central Pacific region.

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[1] J. B. Macelwane, S. J., Review of "Comparison entre les valeurs de la profondeur focale des tremblements de terre determinées a l'aide des tables de Wadati et celles obtenues au moyen des courbes de Brunner" by Charles Bois (C.-R., v. 204, pp. 608-610, 1937, Bull. Seis. Soc. Amer. v. 27, pp. 261-262, July, 1937.

[2] B. Gutenberg and C. F. Richter, Materials for the study of deep-focus earthquakes, Bull. Seis. Soc. Amer., v. 26, pp. 341-390, 1936.

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BODY-WAVES OF LONG PATH IN THE BANDA SEA EARTHQUAKE OF FEBRUARY 1, 1938

William A. Lynch and Joseph Lynch

Gutenberg and Richter [see 1 of "References" at end of paper] have discussed the recording of body-waves that have travelled long paths and have been refracted several times through the Earth's core with intermediate reflections at the Earth's surface. They have detected such waves at Pasadena and associated stations with short-period Benioff vertical seismometers; they cite 52 instances of the appearance of P'P' in 27 earthquakes recorded at seven stations and eight instances of P'P'P'. All of these were written about 30 or 45 minutes after the main shocks, P'P' has a focal distance of 72° and P'P'P' has approximately the same focus. Gutenberg and Richter found no evidence of these phases at distances greater than 91° but expected that such evidence might be forthcoming, especially since P'P'P'P' should have a focus near 145°. We offer the following evidence of multiple reflected core-waves at a distance greater than 91°, namely, at about 140°.

The strong earthquake of February 1 was well recorded at Fordham on the vertical Benioff; its epicentral distance was approximately 138°. The preliminary phases were sharply defined and of considerable energy; two late phases of comparatively large amplitude appeared about 45 minutes after the main shock on the short-period component, superimposed on the surface-waves, which are not well recorded by this instrument. The general appearance of these phases led us to think of them as body-waves of long travel-time through the core and to believe that attention should be called to them even though we do not identify them exactly.

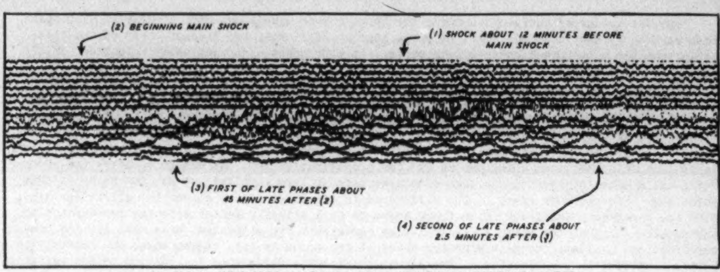

FIG. 1—SEISMOGRAM BANDA SEA EARTHQUAKE, FEBRUARY 1, 1938

Our readings for the main shock and the two refracted phases are:

eP'	19h	23m	32s
iP'	19	23	42
e	20	09	12
i	20	09	31
e	20	11	44
i	20	11	54

The travel-times for the late phases are 64^m 48^s and 67^m 20^s with an interval between them of 2^m 32^s; these times are based on a time of origin of 19^h 04^m4 calculated by the United States Coast and Geodetic Survey. Although we are near the focal zone of P'P'P'P' we did not record this phase which should have a travel-time of 76^m with reflection at 124°5.

We have considered the following possibilities as explanations for our readings:

(1) $P_1'P_1'P_1'$ and $P_2'P_2'P_2'$--The travel-times for these phases with reflection at 166° would be 60^m 03^s and 62^m 57^s with an interval of 2^m 54^s between their arrivals; these values are taken from Macelwane's tables. The disagreement is rather formidable; furthermore we are removed by about 66° from the predicted focal zone of these phases.

(2) $SKPP_1'P_1'$ and $SKPP_2'P_2'$--These have travel-times of 63^m 36^s and 65^m 32^s with an interval of of 1^m 56^s.

(3) $SKSP_1'P_1'$ and $SKSP_2'P_2'$--These have travel-times of 66^m 53^s and 68^m 49^s, with an interval of 1^m 56^s. Because of the change in type of wave from transverse to longitudinal at the surface-reflection, only a small amount of energy would be reflected so that it seems doubtful if these are the phases that were registered.

All of the above calculated travel-times would be altered if the depth of focus were known; the third possibility would be changed most favorably, if the earthquake was of deep focus, so that the measured and the calculated travel-times might be brought into closer agreement. Gutenberg and Richter [2] have recently proposed a new velocity-distribution in the core of the Earth; the travel-times of SKP and P'P' are particulary affected by their new calculations; hence our values tabulated above may be subject to considerable variation.

A private communication from Professor Gutenberg states that Riverside detected a strong phase in the Banda Sea earthquake at 20^h 08^m 50^s and Tinemaha likewise recorded phases at 20^h 04^m 12^s, 20^h 08^m 20^s, and 20^h 08^m 42^s, the last probably corresponding to that at Riverside. He suggests that they may be aftershocks, P'P'P', or shocks from a different source entirely.

They can scarcely be considered aftershocks since the origin-time from the Riverside data would be 19^h 54^m 20^s and that from the Fordham data 19^h 49^m 48^s or 19^h 52^m 20^s--not very good agreement.

If they be considered P'P'P', the approximate travel-times for the California stations would be $P_1'P_1'P_1'$ = 59^m 33^s and $P_2'P_2'P_2'$ = 60^m 51^s with reflections at about 156°5. The actual travel-times are $64^m\ 21^s$ for Riverside and $59^m\ 43^s$ for the first phase at Tinemaha. Thus the latter phase could very well be $P_1'P_1'P_1'$; Tinemaha is between 35° and 40° from the focal zone of P'P'P' and the doubt arises whether sufficient energy would be concentrated at this region to give a clear impulse.

Thus the data at hand are not adequate to resolve completely the question of what the phases are that were recorded at Fordham; a shock some twelve minutes earlier than the main shock was clearly recorded and served to complicate the picture still more. We are particularly interested in the problem because of its bearing on the whole subject of the Earth's core. We incline to the belief that the phases are $P_1'P_1'P_1'$ and $P_2'P_2'P_2'$.

References

[1] B. Gutenberg and C. F. Richter, Beitr. Geophysik, v. 41, p. 149, 1934.
[2] B. Gutenberg and C. F. Richter, Nature, v. 141, p. 371, 1938.

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RELATIONS OF TRAVEL-TIME CURVES TO THE SEISMIC WAVE-VELOCITIES IN THE CONTINENTAL AND SUBOCEANIC LITHOSPHERE

H. Landsberg and H. Neuberger

The velocities of seismic waves and their changes are the only reliable information on structural elements of the inaccessible depth of the Earth. Interpretations of seismic wave-speeds have been used as arguments for and against geotectonic hypotheses. Captain N. H. Heck and Dr. R. M. Field, of Princeton University, suggested that further investigations be made on seismic wave-velocities in suboceanic territories in order to reveal possible differences to continental areas.

This paper is a preliminary report on a project finally proposed by the Commission on Continental and Oceanic Structure and by the Association of Seismology of the International Union of Geodesy and Geophysics. Captain Heck suggested also that earthquakes from the mid-Atlantic region be examined and the speed of the surface-waves be scrutinized. For this study only longitudinal waves were used because in evaluating seismograms, the exact onset of surface-waves is not always well marked; the effects of dispersion and selective magnification of the seismographs also enter into the picture. The study of surface-waves, however, is contemplated in continuation of the present investigation.

Longitudinal waves had been used for the particular purpose only in local investigations because it was assumed that in greater distances any differences in surface-layers would be obscured by the greater and uniform speeds in the deep layer to which the waves penetrate. Gutenberg [see 1 of "References" at end of paper] contended from data on the Tango earthquake of 1927 that no differences could be found larger than the "limits of observational errors." The assumption of a uniform speed in deep concentric shells around the core of the Earth has never been verified and recently the suspicion was expressed by Macelwane [2] that structural differences reach to a considerable depth, and as a reason for this the deep-focus earthquakes have been mentioned, under the assumption that they might be essentially tectonic. Without underwriting this last assertion, it was felt that longitudinal waves should be scrutinized more for differences in wave-velocities. On a previous occasion [3] the travel-times of waves originating from the focus 34° south and 57° east had been examined and as a preliminary result it was found that two branches of the ordinary P-travel-time curve exist. The evidence pointed toward an azimuthal difference in speed. In the meantime Jeffreys [4] and Bullen [5] have called attention to the necessity of correcting travel-times for the ellipticity of the Earth in order to get comparable values. Therefore the whole previous work was repeated and all travel-times for the earthquakes from the mentioned focus were corrected according to Bullen's tables [6]. One reason for using the particular epicenter was the frequent repetition of world-shaking shocks from this focus. Forty-two earthquakes as tabulated in the International Seismological Summary were used in the survey. Even after the ellipticity-corrections are applied there still remain azimuthal differences in travel-times. This is best demonstrated by choosing groups of stations in equal distances in the same or different azimuth. Table 1 gives a selected list of such stations and the mean P-time residuals against the travel-time curve (Turner 1) used in computing the epicenter.

It is evident from Table 1 that in the groups 1, 7, and 9 the arrival-times at the stations agree with each other and the travel-time curves within one second. In all cases the azimuths are nearly the same. In the groups 2, 3, 5, and 8 the differences of the residuals are considerable, ranging from five to eight seconds. Stations of northern azimuth are compared here with

Table 1--Travel-time residuals to different stations for earthquakes from the focus 34° south and 57° east

Group	Station	Distance	Azimuth	Residuals
		°	°	s
1	Batavia	53.7	70	+0.5
	Malabar	53.7	72	-0.4
2	Melbourne	68.6	122	+5
	Helwan	68.3	337	+10
3	Baku	74.7	355	+9
	Riverview	74.9	121	+3
4	Hong Kong	78.3	53	0
	Athens	78.3	335	+2
5	Belgrade	85.5	336	+3
	Wellington	85.5	139	-2
6	Zagreb	88.0	334	0
	La Plata	88.0	229	+3
7	Budapest	88.2	337	0
	Florence	88.2	330	+1
8	Venice	89.0	331	+1
	Zi-Ka-Wei	89.0	50	-7
9	Moncalieri	90.7	328	-0.5
	Barcelona	90.7	324	+0.5
10	Granada	90.8	317	-2
	Ekaterinburg	90.9	3	-4

those of eastern azimuth and in all cases the travel-time to the east is less than to the north. Not quite so large are the differences in the groups 4 and 10 with only two seconds, but again the stations in the northwestern sector show the slower travel-times in comparison to the northeasterly located stations. In case 6 a western station has the slow travel-time. Even with a change in the position of the epicenter not much is gained and therefore differential speeds actually suggest themselves. Before the final verdict on this question can be given a few other considerations have to be made. If the velocity-differences to the various stations are computed it is found that a variability of ± 200 meters/sec results in the extremes. In the case in question there is a tendency visible that these differences are negative up to a distance of 85° and thereafter become positive. In these computations the original curve of Turner was used again and the differences found from the formula $dV = \Delta/t - \Delta/P$, where Δ is the distance, t is the observed travel-times, and P is the postulated travel-time of the curve.

A comparison of the observed travel-times from the assumed focus and a number of often used travel-time curves was made. The result of this comparison is shown in Figure 1, where the differences against the curves of Gutenberg 1929, Turner 1, Turner 2, Jeffreys 1932, Macelwane, Brunner, Hodgson are plotted. The means of the absolute values of the residuals are given in Table 2.

Table 2--Mean of absolute values of residuals against various travel-time curves

Author	Mean residual
	seconds
Turner 1	3.7
Gutenberg	5.8
Hodgson	7.0
Brunner	13.0
Turner 2	16.3
Macelwane	16.7
Jeffreys	17.5

It is not surprising that the residual against the original curve of Turner is small because the epicentre was computed by means of this curve. The table, nevertheless, shows that the travel-time curves as gauged by the earthquakes from this particular focus are arranged in two groups, approximately ten seconds apart. These ten seconds, therefore, seem to establish the width of the band of actually occurring travel-times within the direct longitudinal waves. All deliberations based on these average curves, therefore, have to figure on actual deviations of this order of magnitude. Therefore the use of these curves tends to obscure the small differences in speed sought. Figure 1 shows also abrupt changes in the direction of the difference-curves. One is tempted to explain such changes by proposing that the travel-time curve of these (as of really any other) earthquakes is not a monotonously increasing function of the distance but a function of distance and azimuth. In other words the average travel-time curves smooth out actually present differences and thus partly defeat their purpose of serving for analysis of seismic wave velocities.

Another obvious drawback of the present travel-time curves and their use for epicentral determinations is the preponderance of the group of European stations. Their number is so largely

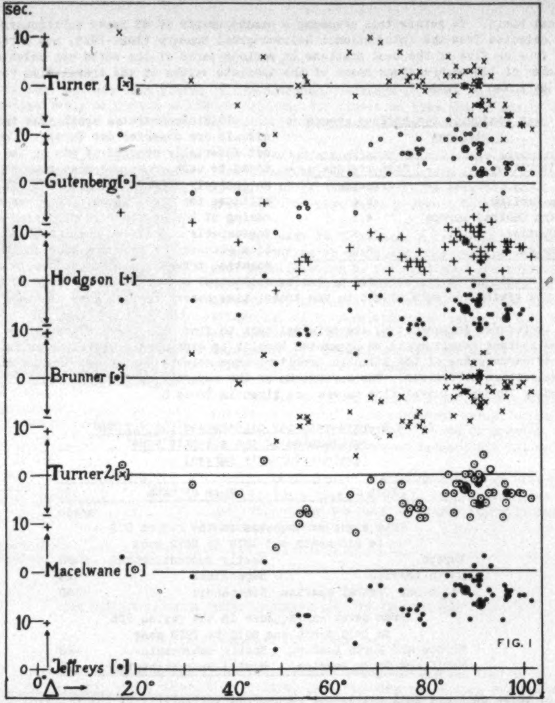

FIG. I

superior to that of most other areas that every use of a least-square adaption will result in an epicenter really fitted only to the European stations, not to speak of their influence in the original computation of the travel-time curves. This is easily demonstrated by the mean absolute residuals for 13 heavy earthquakes from the focus 16°.2 north and 97°.2 west as listed in the International Seismological Summary, corrected for ellipticity and under exclusion of residuals of more than ±20 seconds. The result for three main groups of azimuths and intervals of distances is shown in Table 3.

Table 3--Mean residuals of observed travel-times for various distances and azimuths from the focus 16°.2 north and 97°.2 west

Azimuths	Distance										
	0°-9°.9	10°-19°.9	20°-29°.9	30°-39°.9	40°-49°.9	50°-59°.9	60°-69°.9	70°-79°.9	80°-89°.9	90°-99°.9	100°-109°.9
1°-90°	9.8(10)	--	5.3(41)	10.5(16)	--	--	6.3(9)	3.1(13)	2.7(129)	5.7(42)	10.8(6)
91°-180°	--	--	--	--	6.5(16)	--	6.7(6)	--	--	--	--
271°-360°	9.0(26)	10.1(4)	6.5(13)	8.5(15)	--	--	--	--	--	--	--

The number in parentheses indicates the number of observations available in the particular group. There can be little doubt that the minimum of the residuals is noted for the European group of stations in the first azimuthal column and at distances from 70° to 100°. It might be suggested that the unequal number of observations could have produced this result on a purely statistical basis. To refute this argument a random sample of 65 heavy earthquakes from various foci was selected from the International Seismological Summary (1920-1928) and a representative group of four or five of the best stations in various parts of the world was selected. For an equal number of 169 observations means of the absolute values of the travel-time residuals were computed as shown in Table 4.

Table 4--Mean residuals for various groups of stations

Territory	Mean residuals
	seconds
East Asia Pacific	6.4
Western and Central Europe	4.1
Central Russia	5.3
North America	5.5

This demonstrates again that the minimum residuals are observed for Europe. The result is not materially changed if one or two stations are added to each group; it also remains unchanged if an entirely different group of stations is substituted for the original one. For this reason naming of the stations is dispensed with. The inescapable conclusion remains that, since the time accuracy of numerous good stations cannot be doubted, travel-time differences in different directions exist, which are obscured by basing epicentral computations too much on a cluster of stations and giving too much weight in the travel-time curves for the same cluster.

If, therefore, in pursuit of the original task to find velocity-differences in various substructures another result shall be presented here, it is with some reservation as to the finality. A number of earthquakes of the Atlantic territory were selected and the subcontinental and suboceanic velocities determined. The deviations of the apparent surface-velocities from those postulated by Turner's travel-time curves are given in Table 5.

Table 5--Wave-velocity differences for various earthquakes in the Atlantic zone (against Turner's curves)

Path to	Type of path	dV
		m/sec
From eight earthquakes in the region 0°0 to 2°0 south and 13°4 to 28°2 west		
Europe	Mostly subcontinental	-50
North America	Suboceanic	-20
South and Central America	Suboceanic	+100
From seven earthquakes in the region 7°5 to 19°0 north and 60°0 to 79°0 west		
Europe and North America	Mostly suboceanic	+50
North and South America	Mostly subcontinental	-70

All of these data are well within the recognized variability of the P-travel-times and carry weight only as a statistical result, because as such they should at least eliminate accidental errors and underline systematic deviations. In this connection the results of Krumbach [7] for the fast surface-waves should be mentioned, because they reach similar conclusions on the wave-velocities.

One fundamental thing, however, seems to be clear from our studies: For detailed investigations of the propagation (and speed) of earthquake-waves of distant earthquakes, travel-time curves separated according to azimuths and station groups have to be postulated. Furthermore, clusters of stations should in all error-computations (least-square method, etc.) be treated as a single element rather than as separate stations. Otherwise the small differences searched for will be buried in the inhomogeneity of the material.

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[7] G. Krumbach, Reichsanst. Erdbebenforschung Veröff., 16a, 1934.

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A SIMPLE SEISMOSCOPE

T. A. Jaggar

This instrument has now been placed in the telephone-exchange in Hilo, Hawaii, and the radio rooms respectively of Canton and Baker islands, for report on time and intensity of local earthquakes by postal card to the Hawaiian Volcano Observatory (or by radiophone).

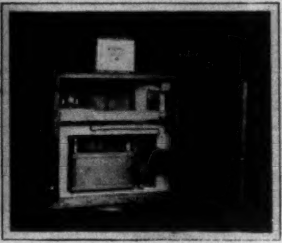

FIG. 1—BOX SCREWED ON A SHELF OR FLOOR; SIX-POINT INDICATOR OR EDWARDS "ANNUNCIETTE" IS ON TOP; HEAVY MASS ENCASED IN WOOD, HANGING BELOW (SIX MERCURY-CONTACTS ARE AT RIGHT, ABOVE; A BOOM, NOT SHOWN, RISES FROM THE MASS, AND OPERATES STYLUS CLOSE TO ITS PIVOT, ON THE LEFT ABOVE

It is an annunciator (Fig. 1) with electric bell to show earthquake-amplitudes 1 to 6 in the range approximately I to IV Rossi-Forel. The original model has done good work for some years in Hawaii National Park.

The mass is 30 pounds, a rectangular normal pendulum swinging in one plane only, period one-half second, magnification by vertical boom and horizontal stylus 100x, tip of stylus ranging over six cm of six mercury-cup contacts. These are connected through either 110-volt transformer, or battery, to a common six-point wall-annunciator. Each mercury-meniscus is adjustable for height, and the stylus is adjustable in three directions with thumbscrews.

The postal card reports from these instruments will furnish a numerical one-component value for maximum amplitude. Only air-damping is used at present, though the original seismoscope at Kilauea was oil-damped.

It is hoped the postal-card returns will be more reliable than those dependent on sensation and that they will cover the smaller quakes and will record the night shocks.

National Park Service,
Hawaii National Park, Hawaii

THE ELECTROMAGNETIC MICROBAROGRAPH AND ITS PERFORMANCE

J. B. Macelwane and J. E. Ramirez

At the Ninth Annual Meeting of the Eastern Section of the Seismological Society of America held at Fordham University, April 30 and May 1, 1934, the following resolution was passed:

"RESOLVED, That the Eastern Section of the Seismological Society of America, cognizant of the importance of increasing our knowledge of the nature and causes of microseisms, urges the development of suitable recording instruments and the continuances of studies of this phenomenon, and that the Section urges the United States Coast and Geodetic Survey and the Jesuit Seismological Association to set up necessary instruments for recording microseisms at the Hawaiian Islands and at Saint Louis University, respectively."

A paper had been read at that meeting by Donald C. Bradford, then a graduate student at Saint Louis University, entitled "The significance of barometric oscillations in the propagation of microseisms, with special reference to ground unrest during the years 1929-31, inclusive, at Sitka, Alaska." In this paper and in another read at the Tenth Annual Meeting in Ottawa, May 27-28, 1935, by Bradford and Macelwane entitled "The relation between changing meteorological conditions and microseisms recorded at Saint Louis University," the possibility was suggested that microseisms might be caused by microbarographic oscillations of similar period. In order to test this hypothesis the authors in 1934 designed an electromagnetic microbarograph with a relatively high sensitivity to air-oscillations of the same order of frequency as the microseisms and with an open recording time-scale of one millimeter per second.

Description--This instrument was built by W. F. Sprengnether of Saint Louis with the collaboration of D. C. Bradford and Father Vincent M. O'Flaherty. It consisted essentially of a large brass air-chamber over the open end of which was tightly stretched and clamped a rubber diaphragm (Fig. 1). The drum stood on its side so that the plane of the diaphragm was vertical. A short solenoid was attached to the diaphragm at its center with its axis normal to the rubber

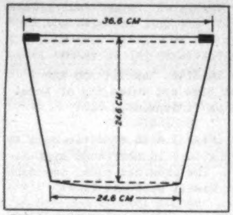

FIG. 1—SHAPE AND DIMENSIONS OF BRASS KETTLE

surface and its weight supported from the frame by two vertical wires balanced by pairs of rubber stays below spaced at angles of 120° each and stretched parallel to the diaphragm (Fig. 2). Into the air-core of the solenoid was inserted without contact one pole of a cobalt-steel bar-magnet rigidly supported by the frame. The air-chamber was provided with an adjustable needle-valve so that the pressure within the chamber slowly adjusted itself at any predetermined rate to the average atmospheric pressure outside. The diaphragm and the coil attached to it were thus forced to respond faithfully to any relatively rapid fluctuation in atmospheric pressure lasting seconds or minutes; but the diaphragm was not permanently distended or forced inward by the major waves of pressure in cyclones and anticyclones which last hours or days. The atmospheric oscillations of short period thus forced a relative motion between the moving coil and the stationary magnet and in this way generated an electromotive force roughly proportional to the velocity of the center of the diaphragm.

The leads from the coil were connected for varying periods of time (1) to a Moll galvanometer whose free period was 1.3 seconds, (2) to a Leeds and Northrup type-R galvanometer of seven-second period, and (3) to a Hartmann and Braun galvanometer of about 13-second period. The Leeds and Northrup combination gave the best results. The oscillations were recorded photographically (Fig. 3) on a Henson seismographic drum in the seismic vault and the same time-signals were used for the minute-marks as in the case of the seismographs.

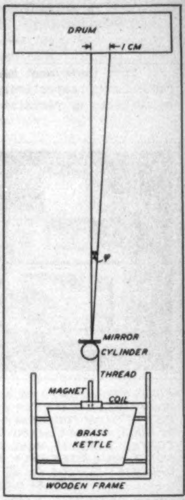

FIG. 2—DEVICE FOR TESTING THE MAGNIFICATION

The microbarograph worked so well that a second instrument similar to the first was constructed with the help of a grant from the American Association for the Advancement of Science through the Missouri Academy of Science to be placed at Florissant.

Details of the microbarograph--Among the elements that may be of interest in the construction of the microbarograph are the following:

Coil--The coil is wound on a hard cardboard tube with two side-walls of bakelite; the tube is 5.8 cm long with an outside diameter of 2.7 cm. The wire of the coil is enameled copper wire No. 37 B. and S. The total number of turns is 41,000 and the resistance between the two terminals of the coil is 10,700 ohms.

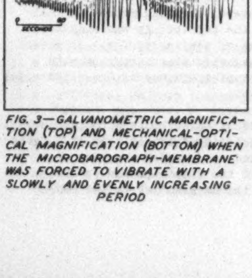

FIG. 3—GALVANOMETRIC MAGNIFICATION (TOP) AND MECHANICAL-OPTICAL MAGNIFICATION (BOTTOM) WHEN THE MICROBAROGRAPH-MEMBRANE WAS FORCED TO VIBRATE WITH A SLOWLY AND EVENLY INCREASING PERIOD

Magnet--The magnet used with the coil is a cobalt-steel bar-type obtained from the Central Scientific Company. The length of the magnet is 15.2 cm and the diameter of its cross-section is 1.6 cm.

Volume of the brass drum--The volume of the drum was calculated in two different ways: (1) By measuring the amount of water that a similar kettle would hold giving as a result 18.15 liters; (2) by a geometrical calculation considering the kettle as composed of the frustrum of a cone and-of a spherical sector--the result was 18.3 liters.

Magnification--In order to determine the total magnification of the small movements of the membrane due to air-oscillations, a mechanical-optical device was employed. It consisted in attaching one end of a thread to the coil of the microbarograph, the other end being wound around a tiny cylinder which rested on agate-bearings. The thread was kept taut by means of a hair-spring which tended to rotate the cylinder and a plane mirror glued to it with shellac. Finally a beam of light was directed into the mirror and reflected back onto the photographic paper and made to record beside the beam of light from the mirror of the galvanometer. This magnified the motion of the coil about 2,200 times.

By comparing the mechanical-optical magnification with the galvanometric magnification, the latter was found to vary with the period of membrane-vibrations, being greatest for those vibrations of the membrane which had about the same period as the galvanometer, that is, around seven seconds. For such periods the galvanometric magnification was about three times as great as the mechanical-optical magnification so that the galvanometer magnified the membrane-vibrations about 6,600 times.

Good records were obtained of both magnification-curves, the mechanical optical on top and the galvanometric at the bottom of the records. These records were taken while the membrane of the microbarograph was being forced to vibrate with constant amplitude but with a slow and even increase in the period. The forced vibrations were caused by the pressure against the membrane of a slightly twisted axis of a fly-wheel, which was set in rotation and allowed to die down in two or three minutes.

Certain peculiarities could be observed in the curves: (1) The amplitudes of the mechanical-optical magnification-curves were very much the same throughout; (2) the amplitudes due to the galvanometer were of about the same size as long as the period of the membrane-vibrations were far below the natural period of the galvanometer; (3) the amplitudes began to increase slowly as the period of the membrane-vibrations increased, reaching a maximum a little before resonance with the galvanometer; (4) as soon as the period of the membrane-vibrations passed the natural period of the galvanometer, that is, about seven seconds, the amplitudes decreased rapidly down to zero.

Stray fields--In order to eliminate the possibility that stray magnetic fields or currents might be affecting the galvanometer of the microbarograph, and also the leads either of the galvanometer or of the microbarograph, the galvanometer was shielded with a double metallic cover and tinfoil was placed around the coil of the microbarograph with a connection to the ground. No change at all was noticed in the character of the records.

Mechanical vibrations--For some time short-period oscillations consisting of a long series of short vibrations of small amplitudes were a puzzle without reasonable explanation. These series of vibrations start little by little and acquire a maximum of 3, 4, or more mm in amplitude and die out slowly almost unnoticeably. The problem was solved when the microbarograph began to show that it could act also as a seismograph for very short-period vibrations. The series of puzzling short oscillations were possibly due to vibrations caused by fast-moving trains transmitted through the ground. The tracks of the Wabash Railway are about four miles from the vault at Florissant. Blasts and nearby earthquakes have been recorded by the microbarograph when the vibrations of the ground had more or less the same period as the natural vibration of the membrane which is about 0.1 second.

The microbarograph has been also set up side by side with seismographs of periods of seven seconds and more, and, while distant earthquakes have affected the seismographs, no visible traces were left on the microbarograms.

Performance of the microbarograph--A close examination of the records has lead to an arbitrary classification of relatively rapid atmospheric oscillations according to the different periods:

(1) Very fast oscillations of extremely short periods and varying amplitudes, the periods being of the order of fractions of a second to two seconds. These strong short-period oscillations lasting for several seconds have been identified as local disturbance in the microbarograph-room. Each time the door to the microbarograph-room was opened or closed a disturbance of this unvarying nature was registered.

(2) Oscillations of very irregular periods varying from two seconds to eight seconds. The trace amplitude of these oscillations varies within wide limits from a few millimeters up to 20 or more millimeters. These oscillations may occur at any particular time. However, when the wind-velocity is below seven or eight miles per hour, they fail to appear in the records. In calm weather the records consist of practically straight lines. To a maximum wind-velocity corresponds a maximum in amplitude in the microbarographic oscillations.

In this class of irregular oscillations, two types at least of air-disturbances play a leading role. The first type are wind-gusts, and seem to be of a rather local character depending entirely on the wind-strength. The second type seem to be air-pressure disturbances of a more general character.

In order to eliminate the wind-gusts, special precautions were taken to protect the instruments. One of the microbarographs was placed within a strong wooden box first on a well-exposed roof, then inside a large building, and finally inside the seismic vault in St. Louis where wind-gusts were not possible and only atmospheric pressure-changes could be recorded. The second microbarograph was placed in a similar box first on level ground outside, and then inside the subterranean seismic vault at Florissant.

(3) The third class of oscillations is made up of long periods varying from eight seconds to whole minutes. These long periods are not so common as the second class and seem to occur at the times of large atmospheric changes, for example, the arrival of a cool or warm air-front, the beginning of a wind-storm, etc. Then the shorter-period waves are often superimposed on the long oscillations. A remarkable feature is the transition from calm to strong breezes. The straight line of the record is then often broken by one or more long-period waves followed by irregular vibrations produced by wind-activity.

At the present time the microbarograph is being used for the study of atmospheric pressure-changes and their relationship to other meteorological and seismological phenomena, especially their connection with microseisms or ground-oscillations.

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A SEISMOGRAPH FOR MICROSEISMS

J. B. Macelwane and W. F. Sprengnether

At the Fordham meeting of the Eastern Section of the Seismological Society of America it was strongly urged by Dr. Frank Wenner that microseisms be studied by means of an instrument designed especially for that purpose. A small research-fund given by friends in Saint Louis has made it possible for the Department of Geophysics of Saint Louis University to carry out this suggestion. A comparatively simple and relatively inexpensive seismograph has been designed for this purpose by the senior author and built in the shops of the junior author.

The design chosen was a small horizontal pendulum with tension-hinge supports, induction-transducer, and electromagnetic damping. The relatively small dimensions and slight moving mass of only about two and one-half pounds make the instrument readily portable.

The boom of aluminum tubing carries a brass frame into which is set a brass box containing the two coils wound with a large number of turns of fine wire. This coil-box is supported in the field of a strong horseshoe magnet of Alnico alloy and the wires are led back through the hollow boom to binding posts on the frame. At the outer end of the boom is a copper damping-vane which moves in the fields of four opposing pairs of small commerical Alnico magnets, whose separation may be adjusted to secure critical damping at any period desired.

The current from the coils of the transducer is led to a Leeds and Northrup type-R galvanometer of seven-second period, whose critical resistance is that of the coils in series.

The period of the pendulum is adjusted to seven seconds, so that the whole system is in resonance with the microseisms that are to be studied.

The results have been very gratifying and excellent records are obtained with the magnification adjusted to about five thousand.

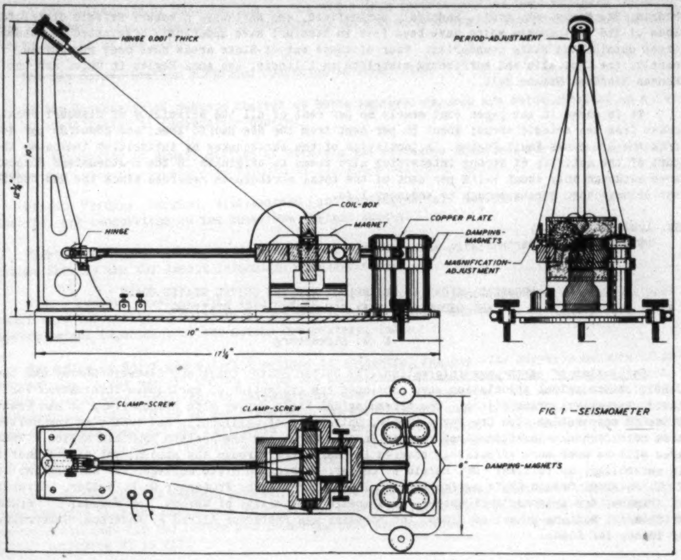
HINGE 0.001" THICK
PERIOD-ADJUSTMENT
COIL-BOX
MAGNET
COPPER PLATE
DAMPING-MAGNETS
HINGE
MAGNIFICATION-ADJUSTMENT
12½"
10"
10"
17½"
CLAMP-SCREW
CLAMP-SCREW
6"
DAMPING-MAGNETS

FIG. 1 —SEISMOMETER

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A CONTRIBUTION TO THE STUDY OF THE SEISMICITY OF MISSOURI

Ross R. Heinrich

(Abstract)

The status of Missouri as a State which contains active seismic areas has been a subject of discussion ever since the New Madrid earthquakes of 1811 and 1812. Local seismic research, however, was slow in starting due principally to the lack of available instruments to supply sufficiently accurate data for detailed work.

Two events which gave great impetus to the program of Missouri earthquake-study were: (1) The installation of the seismograph at Saint Louis University in 1909, and (2) the inauguration of the regional study-plan which was proposed by Reverend James B. Macelwane, S.J., in 1925.

The present paper is concerned with a few of the characteristic features of Missouri earthquakes which were noted during the compilation of a seismic history of Missouri.

By a tabulation of the data compiled it was found that the yearly average of Missouri earthquakes showed an increase from 0.8 in 1911 to 4.3 for the years 1930-1937. The paper emphasizes that this is an apparent increase brought about by the increase in available records in the more recent years.

An examination of the list of shocks further reveals that most of the earthquakes which have been felt in Missouri originate in more or less definite seismic districts. Six such districts are suggested for Missouri and they are called, because of their locations, the New

Madrid, St. Marys, St. Louis, Hannibal, Springfield, and Northwest Missouri seismic districts. Some of the earthquakes which have been felt in Missouri have apparently originated in seismic areas outside the State boundaries. Four of these out-of-State areas have been recognized, namely, the Centralia and Harrisburg districts in Illinois, the Anna Region in Ohio, and the Kansas-Nebraska Nemaha Belt.

It is shown in the paper that nearly 85 per cent of all the seismicity of Missouri origin comes from two seismic areas; about 60 per cent from the New Madrid Area, and about 25 per cent from the St. Marys Fault-Region. A tabulation of the earthquakes by intensities indicates that most of the activity of strong intensities also seems to originate in the southeastern Missouri area although only about 7-1/2 per cent of the total earthquakes recorded since the New Madrid series have been strong enough to endanger life.

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PROGRESS-REPORT IN SEISMOLOGY FOR THE UNITED STATES COAST AND GEODETIC SURVEY AND COOPERATING STATIONS

E. W. Eickelberg

Collection of earthquake-information--The United States Coast and Geodetic Survey and the Jesuit Seismological Association have continued the collection of earthquake-information for their respective jurisdictions. The Seismological Field-Survey with headquarters at San Francisco in cooperation with Dr. Perry Byerly, University of California, has continued the collection of earthquake-questionnaires for the Pacific Coast and the Western Mountain Region. This area will be much more effectively covered in the future through the appointment of collaborators in seismology, as follows: Dr. Harold E. Culver, Washington State College, for Washington; Dean E. L. Packard, Oregon State Agricultural College, for Oregon; Professor G. M. Butler, University of Arizona, for Arizona; Professor Y. P. Gianella, University of Nevada, for Nevada; Dr. Francis A. Thomson, Montana School of Mines, for Montana; and Professor Alfred F. Anderson, University of Idaho, for Idaho.

Others cooperating in the furnishing of earthquake-reports for the western area are: The Standard Oil Company of California and its subsidiaries; Southern Pacific Railroad; Pacific Gas and Electric Company; Seismological Laboratory at Pasadena, Jos. J. Bognuda; United States Weather Bureau; United States Bureau of Reclamation at Boulder Dam; and United States Forest Service.

These reports are appraised and condensed by the Seismological Field-Survey and the mimeographed assemblies are distributed quarterly from Washington. During 1937 the Seismological Field-Survey received 2477 questionnaire reports from 335 earthquakes, a major portion of that number applying to 27 shocks. For the same period 227 reports on 100 earthquakes were received by the Washington Office for the balance of the country. The only canvassing done by the Washington Office was for the Long Island earthquake of July 19, 1937, and also for the Fairbanks, Alaska, shock of July 22, 1937, and the Hawaiian shock of January 23, 1938.

Teleseismic work--The United States Coast and Geodetic Survey has assisted with plans for the establishment of new stations at Lincoln (Nebraska), Logan (Utah), and Mojave (California). Through cooperation of the Seismological Laboratory of the Carnegie Institution of Washington and the United States Coast and Geodetic Survey a new teleseismic station has been established for the Reclamation Service at Boulder City, Nevada, for the purpose of studying earthquakes originating near Boulder Dam. Only one component (Wood-Anderson, one-second period) has been installed to date, although a more extended program consisting of three complete stations suitably located is under consideration. Operation of the one component began on February 1, 1938, and a large number of weak local shocks are being recorded. One hundred and forty-three shocks were felt at Boulder Dam during 1937.

A special study was made for the Panama Canal Department of the problem of locating earthquakes in the vicinity of the Canal Zone.

One component of the Wenner seismometer at the San Juan Magnetic Observatory has been completely overhauled and has been equipped with new but narrower coils which it is believed will improve the performance of this instrument. As soon as the improved unit has been installed, the other component will be returned to Washington for similar repairs. No recorders have been provided for this station.

At the University of Chicago, one of the Milne-Shaw instruments has been replaced (June, 1937) by a small tilt-compensation instrument in an effort to overcome difficulties due to excessive tilting of the pier.

The Des Moines Station suspended operation in June, 1937.

At the University of Montana station at Butte improved records are being obtained as a result of installation of temperature-control.

The University of Hawaii station at Honolulu has requested equipment for recording local shocks.

Weston, Fordham, Harvard, Williamstown, and Burlington, all equipped with short-period Benioffs, are cooperating on the local New England shocks.

Thirty-nine epicenters were located from telegraphic data obtained in cooperation with Science Service and the Jesuit Seismological Association.

Ninety records covering nine earthquakes were loaned for special studies. Principal borrowers were University of California, University of St. Louis, and Dr. K. R. Ramanathan, India Meteorological Department of the Colaba Observatory, India.

Recording of Earth and building motions in epicentral regions--The Survey's network of accelerographs in the western United States now consists of 40 accelerographs, six displacement-meters, eleven Weed strong-motion seismographs, and three tiltmeters. All are serviced by the Seismological Field Survey at San Francisco, the more distant ones in Montana and Nevada about once or twice a year and the California stations about once a month. Personnel of the University of Montana, Bureau of Reclamation at Boulder City, and Naval Ammunition Depot at Hawthorne make the monthly inspections of their respective stations.

During the year 42 records were obtained from nine earthquakes. Twenty of these records were obtained from the earthquake in Riverside County on March 25, 1937. This was the most severe earthquake for the United States during 1937 and was felt over an area of 30,000 square miles, intensity VI to VII.

One of the accelerographs which recorded the Long Beach earthquake of 1933 has been tested on the shaking table at Massachusetts Institute of Technology. These tests were carried out as a cooperative program by Massachusetts Institute of Technology and the United States Coast and Geodetic Survey for the purpose of testing the accuracy of this equipment. The results of these tests, interpretations, and comparisons are about completed.

Two accelerographs are on exhibit at the Washington Office. These are available to be sent out in case of a strong earthquake in the east. Another accelerograph is on exhibit at the Museum of Science and Industry in Chicago and is also available for service in the event of an earthquake in the middle west.

Vibration-work--Thirty-two building vibration-records were obtained in 21 buildings. Five vibration-records were obtained on two bay bridges. Ground vibration-observations were made at 51 locations. Special vibration-observations were made on a 606-foot stack, two elevated concrete tanks, and at Boulder Dam.

Tiltmeters--Three tiltmeters have been kept in operation at the University of California during 1937.

Miscellaneous--A special hydrophone and recording equipment designed for the purpose of listening for fault-noises has been assembled by personnel of the United States Coast and Geodetic Survey Ship Guide in cooperation with a joint committee composed of Commander R. R. Lukens of the United States Coast and Geodetic Survey, H. M. Engle, and Dr. N. A. Bowers. Others cooperating in the project are Captain R. V. Labarre, David Hendrickson, George D. Louderback, and Dr. Frank Rieber. The preliminary results indicate that the method has considerable merit and may be of value in furnishing data for appraising the nature or extent of fault-activity.

Special earthquake-triangulation, levelling, and traverse are now in progress in southern California

Publications--The following publications were issued during the year:

United States Earthquakes, 1935.
Mimeographed, non-instrumental reports for the western United States through September, 1937.
Progress-reports in strong-motion work through 1937.
Mimeographed instrumental reports through 1936.
Preliminary determination of epicenter cards.

U. S. Coast and Geodetic Survey,
Washington, D. C.

SOME NEW PHASES APPEARING IN A DEEP EARTHQUAKE

G. J. Brunner

The manuscript for this paper was not submitted.

St. Louis University,
St. Louis, Missouri

AMERICAN GEOPHYSICAL UNION

Nineteenth Annual Meeting

SECTION OF METEOROLOGY

Washington, D. C.
April 28, 1938

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MEASUREMENTS OF ULTRA-VIOLET SOLAR RADIATION IN PUERTO RICO

G. W. Kenrick and H. Ortiz

It is the purpose of this paper to describe preliminary measurements of ultra-violet solar radiation made at the School of Tropical Medicine of the University of Puerto Rico under the auspices of Columbia University. This work was initiated by Dr. W. W. Coblentz of the National Bureau of Standards during a visit to Puerto Rico in 1935 (using measuring equipment developed by him at the Bureau of Standards). Subsequently, a Westinghouse ultra-violet recorder was calibrated, and after several delays it was placed in regular operation on October 28, 1937. The United States Weather Bureau has collaborated by initiating a series of measurements of total solar radiation at San Juan Weather Bureau which is located only a few hundred yards from the School of Tropical Medicine. Records of total and ultra-violet radiation taken at substantially the same point are thus available for comparison.

Due to problems of apparatus incident to the initiation of this program, the series of consistent data thus far obtained are of limited duration, and this report is hence essentially a preliminary report of progress, describing the apparatus and methods employed. The preliminary results obtained and proposals for the future development of the program are discussed.

Apparatus employed--The characteristics of the photoelectric cell utilized for the measurements of ultra-violet are shown in Figure 1. Because of the relatively small portion of the total solar radiation in the ultra-violet, it is extremely important to specify this characteristic in comparing ultra-violet intensity-measurements taken at various points. Otherwise the rapid rise of energy toward the visible region leads to different figures for ultra-violet radiation, if measuring devices of various response-characteristics are compared without correction to the same frequency-range. In the measurements reported here, the correction-factors determined by Coblentz were utilized. It is the object of these corrections to rectify the readings for equivalence with a uniform response-characteristic extending from 3132 Ångstroms down to atmospheric cut-off.

A schematic diagram showing the equipment with which the cell is associated is shown in Figure 2. The intensity of activating radiation impinging on the photoelectric cell determines the rate of charge of the condenser, C_1, to a threshold-voltage resulting in a discharge between the electrodes A and B of a gaseous tube. The ionizing leader-stroke causes a heavier discharge to take place from electrodes C to A and the current accompanying the condenser C_2 associated with the electrode C produces an impulse which is recorded in a chart-type recorder which, if desired, may be located at a remote point.

The photoelectric cell and the associated trigger tube are shown in Figure 3-A. The whole recorder-assembly is shown in Figure 3-B. Figure 4 shows a telescopic mounting adjustable for solar hour-angle and declination which is driven by a synchronous motor and kept normal to the solar radiation during the entire period of observation. This assembly is mounted on the roof of the School of Tropical Medicine and thus measures the ultra-violet radiation at a given surface normal to the Sun's rays at all times. The tube-equipment is connected by a cable to the recorder which is located at the observer's office. These units are shown in Figure 3-B.

Calibration of equipment--The equipment was initially calibrated in absolute units during the visit of Dr. Coblentz to Puerto Rico in 1935 [see 1 and 7 of "References" at end of paper], through the aid of simultaneous observations with his portable equipment and with the counter-recorder. Observations were continued after Dr. Coblentz' departure, but some difficulty was experienced due to leakage across the condenser. This was finally minimized by the use of a dehydrating agent in the sealed compartment containing the tubes and condensers. It was difficult to be certain of the calibration during these earlier observations since no calibrating standard was available after Dr. Coblentz' departure, and on several occasions the equipment ceased to function and was returned to the factory for repairs. The observations during this period were under the direction of the staff of the School of Tropical Medicine. After the equipment ceased to function a second time, Dr. Kenrick of the Department of Physics of the School of Arts and Sciences of the University of Puerto Rico was asked to undertake the servicing and the maintenance of the calibration of the equipment. After the replacement of a burned-out resistance, the equipment was again placed in operation in 1937. At this time it was also found necessary to increase the voltage on electrode C through the introduction of an external voltage of about 45 volts in addition to that normally available. While it was not anticipated that slight variations of potential in this marker-circuit would appreciably affect the calibration of the equipment, it was considered desirable to provide means of periodic checks of its sensitivity. Acting upon recommendation of Dr. Coblentz, a quartz-lamp secondary standard was secured and calibrated at the Bureau of Standards. The dependence of calibration of the recorder on impressed line-voltage was then determinable. Available research-funds of the School of Tropical Medicine were used to provide for Mr. Ortiz' time to maintain suitable operating voltages, calibrations, and to reduce the records. The relationship between line-voltage and click-rate (proportional to observed intensity) is shown in Figure 5, together with the point of indicated operating voltage to bring the calibration in accord with that indicated by Coblentz' calibrations. It will be noted that the curve is rather flat between 110 and 120 volts, indicating the satisfactory operating conditions with the external battery-supply and no important departures due to the introduction of external ionizing voltage. The validity of the calibrations was further checked by varying the distance of the cell from the calibrating lamp (assuming inverse-square law to hold) and at two lamp-operating voltages for which Coblentz supplied data. Good agreement with the inverse-square law was found as is indicated in Figure 4.

Since the calibrating equipment was not available until the fall of 1937, variations due to operating voltage-changes, etc., are considered possible in the data collected between the time

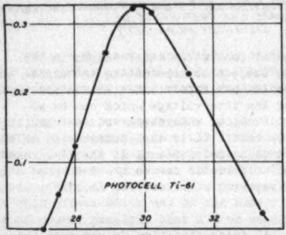


FIG. 1—ACTUAL RESPONSE-CHARACTERISTIC OF PHOTOCELL (MEASUREMENTS RECTIFIED TO EQUIVALENT-SQUARE CHARACTERISTICS FROM 3120Å TO ATMOSPHERIC CUT-OFF BY COBLENTZ CONVERSION-FACTORS)

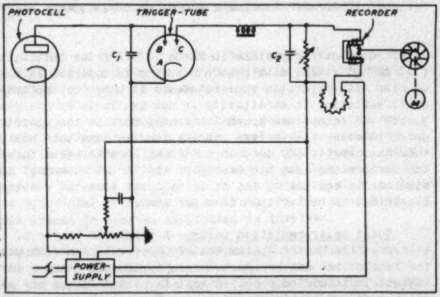
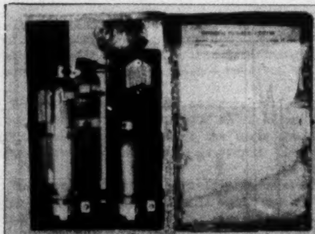
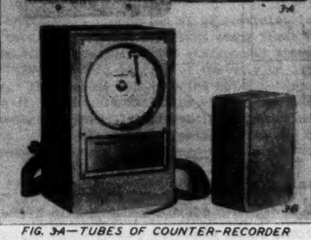


FIG. 2—DIAGRAM OF ULTRA-VIOLET RECORDER





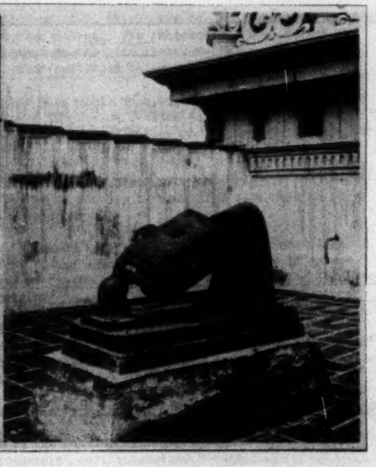


FIG. 3A—TUBES OF COUNTER-RECORDER
FIG. 3B—ACTIVE ELEMENTS OF WESTINGHOUSE RECORDER
FIG. 4 —TELESCOPIC MOUNTING, ASSEMBLY PHOTOCELL (INSURES CONSTANT NORMAL SURFACE TO RADIATION AT ALL OBSERVATIONS)

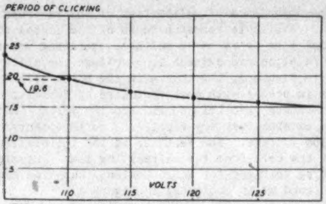


FIG. 5-A—RECORDER-RESPONSE VERSUS LINE-VOLTAGE DETERMINED BY STANDARD QUARTZ-LAMP ULTRA-VIOLET SOURCE (NOTE MINOR CHARGES IN CELL-SENSITIVITY MAY BE COMPENSATED BY VARYING OPERATING-VOLTAGE; NORMAL OPERATING-VOLTAGE SHOWN BY DOTTED-LINE INTERCEPT)

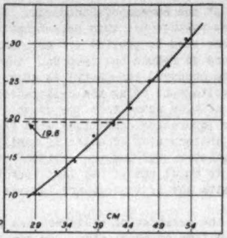


FIG. 5-B—VARIATION OF CLICK-RATE WITH DISTANCE (NOTE SATISFACTORY AGREEMENT WITH INVERSE-SQUARE LAW OVER RANGE OF DISTANCES MEASURED)

of Dr. Coblentz' departure in March 1935 and the initiation of consistent observations in the fall of 1937. The data obtained during this period is also subject to gaps due to the difficulties with apparatus enumerated and is hence not included in this report. Correction for any small variation in sensitivity is now available by changing the line-voltage which can be adjusted and maintained by an autotransformer in the output-circuit of a Raytheon voltage-regulator which reduces line-voltage fluctuations to less than one per cent. It is thus possible to maintain the sensitivity in accord with calibration-data furnished by Dr. Coblentz at the time when the calibrating lamp was tested by him in the summer of 1937. Periodic checks are now being made on the equipment, and it is believed that the results reported since October 28, 1937, are consistent to better than five per cent.

Total solar-radiation values--A companion series of total solar-radiation values were initiated at the United States Weather Bureau at San Juan utilizing an element which is mounted on the roof of the Weather Bureau in a horizontal plane. In comparing total and ultra-violet values, it should be recalled that the total values are as recorded on a horizontal surface which does not remain normal to the Sun. A correction should hence be made for the solar eleva-

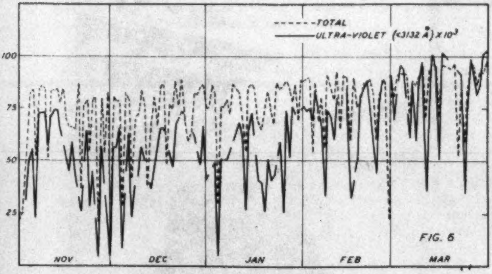


FIG. 6

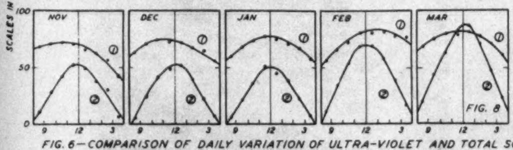


FIG. 8

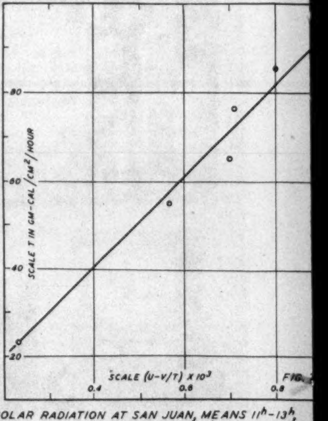


FIG.

FIG. 6—COMPARISON OF DAILY VARIATION OF ULTRA-VIOLET AND TOTAL SOLAR RADIATION AT SAN JUAN, MEANS 11^h-13^h, WINTER 1937-38, (TOTAL VALUES, UNCORRECTED FOR HORIZONTAL SURFACE AND SCATTERED LIGHT)

FIG. 7—RATIO OF ULTRA-VIOLET RADIATION AS A FUNCTION OF TOTAL SOLAR RADIATION, SAN JUAN, WINTER 1937-38

FIG. 8—MONTHLY MEANS, ULTRA-VIOLET AND TOTAL SOLAR RADIATION AT SAN JUAN, WINTER 1937-38, ① TOTAL, ② ULTRA-VIOLET (<3132 Å) X 10^3 (TOTAL VALUES CORRECTED TO ELIMINATE EFFECT OF HORIZONTAL SURFACE AND SCATTERED LIGHT)

tion in comparing the total and ultra-violet values. A correction-factor of 0.88 times secant of solar zenith-distance (air-mass) was applied.

Comparison of total and ultra-violet solar-radiation values at San Juan--Figure 6 shows a comparison of $11^h\ 00^m$ a.m. to $1^h\ 00^m$ p.m. values of ultra-violet and total solar radiation during the period of observations. Good agreement is noted and with very few exceptions the values move together. (Blanks in the ultra-violet curve are due to the fact that the charts have to be changed every few hours and observations are hence not taken on Sundays and holidays.)

An interesting relationship is to be noted between the values of the ratio of u-v/total radiation as a function of total radiation. This relation is plotted in Figure 7, and it should be noted that it comes out nearly a straight line. This indicates that, for the limited period of observation, $A_0T_1/B_0T_2 = KB_0T_2$ or $A_0T_1 = K(B_0^2)T_2^2$, where A_0 = intensity of ultra-violet outside atmosphere, T_1 = atmospheric transmission-coefficient for ultra-violet, B_0 = intensity of total radiation outside atmosphere, T_2 = atmospheric transmission-coefficient for total solar radiation, and K = constant. In other words, under the observed conditions, the atmospheric transmission-coefficient of the ultra-violet appears to vary as the square of the coefficient for the total radiation.

Figure 8 shows monthly means of the diurnal variation of ultra-violet and total radiation for the period available. Data obtained from the above curves are embodied in Figure 9 where the variation of ultra-violet radiation against air-mass is shown.

Complete curves for the seasonal variation in ultra-violet and total solar radiation are not as yet available, but the indicated trend in monthly means for the period of observations is shown in Figure 10-A. An interesting trend in the time of maximum total and ultra-violet values with respect to true noon is indicated in Figure 10-B. A much longer series of observations embracing several years will be necessary before it would be possible to attach any real significance to this effect which may be due merely to accidental or random variations. On the other hand, it is of course possible that a true seasonal change in diurnal absorption is involved.

Latitude-variations--In a priori comparisons of the relative amount of ultra-violet for various points on the Earth's surface, the primary consideration is of course the latitude of the observing point since this determines the directness of the Sun's rays and hence the air-mass absorption.

This is clearly shown in Figures 11-A, 11-B, and 11-C, which give the contours corresponding to various solar elevations for San Juan, Miami, and Boston. The interesting "bow" effect in the 90° curve for San Juan arises from the fact that Puerto Rico (latitude 18° north) is south of the Tropic of Cancer and hence receives 90° solar radiation twice a year.

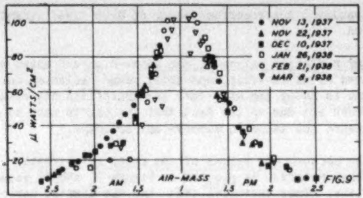
The period at which the solar radiation is at high angle thus varies much less with season than at more northerly latitudes as is shown by the 30° contours which have more or less arbitrarily been taken to indicate the lower bound of useful ultra-violet radiation (see the low 30° ultra-violet values at 3 p.m. in Figure 8).

The greater seasonal variation in noon-angle of solar radiation in non-tropical latitudes is also illustrated in Figure 12 which may of course be readily derived from data such as shown in Figure 11.

Atmospheric absorption of ultra-violet is, however, very much affected by factors other than merely latitude. Important among these factors are seasonal and other changes in ozone-absorption, tropospheric and atmospheric opacity as determined by smoke, water-vapor, clouds, etc. These factors in general can only be determined by careful measurements and statistical study. Thus Figure 13 shows the hours between sunrise and sunset and the hours when the Sun is above 30° (favorable for ultra-violet) for San Juan, Miami, and Chicago [2]. Actually only a percentage of this time (depending on cloudiness and atmospheric transparency) gives effective values. (The values of "effective" time are only approximate and perhaps not quite comparable; the values at Miami and Chicago are as reported by Sieplein.)

It will be noted that the relatively clear atmosphere and low cloudiness of Puerto Rico and the clear skies and absence of industrial smoke in Miami filter out less of the possible ultra-violet (as determined by air-mass values) than in the case of Chicago. (Chicago is probably typical of most of the relatively cloudy and industrial northern United States.)

Further examples of the actual relationship of winter values of ultra-violet for several



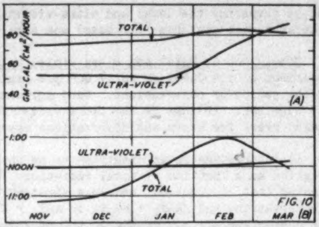


FIG. 9–AIR-MASS AND ULTRA-VIOLET RADIATION (COMPOSITE OF RECENT SAN JUAN VALUES)

FIG. 10-A–SEASONAL TREND OF MAXIMUM RECEIVED ULTRA-VIOLET AND TOTAL SOLAR RADIATION (SAN JUAN, WINTER, 1937-38)

FIG. 10-B–CONSISTANT VARIATION IN HOUR OF MAXIMUM TOTAL AND ULTRA-VIOLET SOLAR RADIATION WITH RESPECT TO TRUE SOLAR NOON (SAN JUAN, WINTER, 1937-38)

points are shown approximately in the bar diagrams of Figure 14 derived from results at San Juan and those reported by Brooks and Coblentz [3,4]. It will be noted that the effect of altitude and low cloudiness partly overcomes the disadvantage of Flagstaff as compared to San Juan but that the reduction due to the more northerly latitudes and atmospheric absorptions at Washington and Boston are strikingly evident.

Figure 15 indicates the comparison of winter diurnal curves taken at San Juan with early-fall diurnal curves at Blue Hill and Mount Washington. In these curves it will be noted that the increased elevation of Mount Washington more than compensates for its northerly latitude as compared with Boston, but that even September New England results are very low compared to San Juan values in winter. (It should be noted that the values in New England are taken with quite different equipment and hence may be far from comparable with results at San Juan due to differences in effective spectral response, calibrations, etc.)

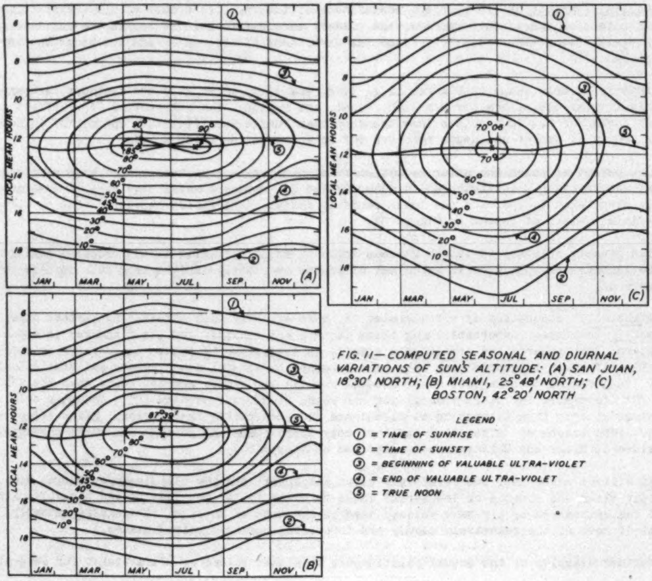
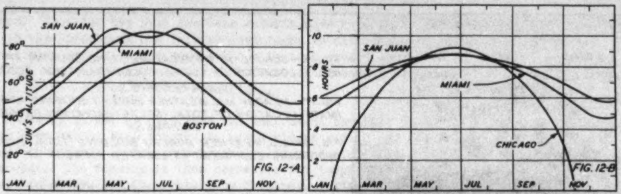
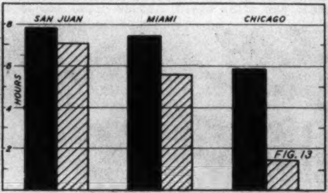


FIG. 11–COMPUTED SEASONAL AND DIURNAL VARIATIONS OF SUN'S ALTITUDE: (A) SAN JUAN, 18°30' NORTH; (B) MIAMI, 25°48' NORTH; (C) BOSTON, 42°20' NORTH

LEGEND

① = TIME OF SUNRISE
② = TIME OF SUNSET
③ = BEGINNING OF VALUABLE ULTRA-VIOLET
④ = END OF VALUABLE ULTRA-VIOLET
⑤ = TRUE NOON





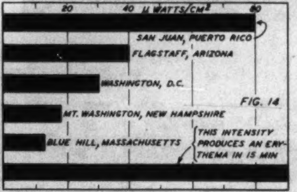


FIG. 12-A B—VARIATION OF SOLAR ALTITUDE WITH SEASON AND LATITUDE
FIG. 13—POSSIBLE AND ACTUALLY RECEIVED "EFFECTIVE" ULTRA-VIOLET AT VARIOUS POINTS
FIG. 14—DIAGRAM SHOWING RELATIVE ULTRA-VIOLET, SEVERAL POINTS WHERE OBSERVATIONS AVAILABLE

Future program--It is, of course, essential that the observations continue for an adequate period to complete the seasonal curves and determine mean figures for "effective" values which will be reasonably free from sampling errors, etc.

During the past winter preliminary spectroscopic observations on both the scattered light of the sky and direct solar radiation have been made; it is hoped that a spectroscope may be acquired which will permit determinations of selective absorption of ultra-violet in the ozone-bands [6]. Thus it would be interesting to compare such data with measurements carried out with the equipment described above and also to provide for measurements of relative absorption permitting a determination of ozone-variations which cannot effectively be made from the total ultra-violet values [5]. It is hoped that these variations of ozone may be correlated with weather-phenomena in the tropics by applications of Dobson's methods.

It is believed that the spectroscopic methods will greatly assist in the determination of ultra-violet in the region below 3,100 Ångstroms where the rapid decline in solar energy results in a rapid decline in integrated photoelectric response which renders difficult an accurate rectification of the response of a T-61 cell to the equivalent of from 3,132 Ångstroms to atmospheric cut-off [1].

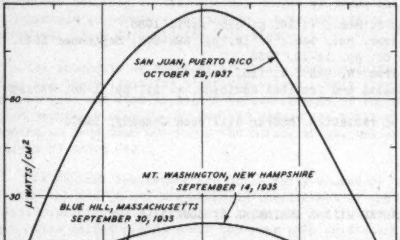


FIG. 15—FALL DIURNAL CURVES OF ULTRA-VIOLET IN TROPICS AND IN TEMPERATE LATITUDES (SAN JUAN AND NEW ENGLAND)

Applications of the data--As an adequate store of data becomes available, it is also hoped that the measurements may form a nucleus for the development of a program relating the study of ultra-violet and total radiation to complex and important biological problems of climate and health [7,8]. To this end plans are being made for extended facilities for the program in the projected Physiology Building of the School of Tropical Medicine.

Acknowledgments--The writers wish to express their appreciation for the cooperation rendered by numerous individuals and organizations. In particular, thanks are due to Dr. W. W. Bachman and the staff of the School of Tropical Medicine for their cordial interest and

FIG. 16—SCHOOL OF TROPICAL MEDICINE SHOWING APPROXIMATE LOCATION OF ULTRA-VIOLET-RADIATION RECORDING-EQUIPMENT

FIG. 17—NEARBY U.S. WEATHER BUREAU SHOWING APPROXIMATE LOCATION OF TOTAL-SOLAR-RADIATION RECORDING-EQUIPMENT

FIG. 18—U.S. WEATHER BUREAU BUILDING (TOTAL-RADIATION RECORDING-EQUIPMENT IS LOCATED ON ROOF AT LEFT)

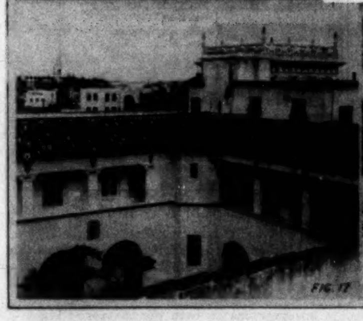


FIG. 17

cooperation, to Mr. Richard Gray and the staff of the San Juan Weather Bureau, and to Dr. O. J. Sieplein of Miami, Florida, who arranged the loan of a quartz-spectroscope owned by the St. Francis Hospital in Miami. This spectroscope has been used in preliminary spectroscopic observations which suggested the extension planned for next year. Dr. Sieplein also provided Figures 11-A and 11-B (Fig. 11-C was computed in Puerto Rico for comparison with Sieplein's curves) showing the solar-altitude comparisons for San Juan and Miami, the values of effective time of ultra-violet for Chicago, and many other data and suggestions helpful in the preparation of the discussion of latitude-effects. Gerard Lavergne has generously given of his personal time and has been of great assistance in the preparation of the figures for the paper.

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A YEAR OF ULTRA-VIOLET MEASURED WITH A DOSIMETER AT BLUE HILL

David L. Arenberg

The ultra-violet dosimeter has already received much attention in Europe since it was first described by Frankenburger [see 1 of "References" at end of paper] and Weyde [2], and extended series of observations have been taken for some years by Pollak and Fuchs [3], Israel-Kohler [4], and others. Thus far the only American reports have been those of Landsberg [5,6] in Pennsylvania. As the instrument has various advantages in climatological work, a study of the problem of ultra-violet radiation was undertaken with this apparatus at Blue Hill Observatory from February, 1937, and is being continued. Substantial agreement was found with previous reports, the results of which will be briefly summarized.

In passing, it may be said that the instrument consists of a quartz-tube containing an organic compound which, when exposed to ultra-violet radiation, changes to a red color. The intensity of the change is measured by neutralizing against twelve standardized green color-filters in the ocular. The reading is then corrected for temperature and length of exposure from tables which were furnished the Observatory by Professor Pollak to determine the radiation-intensity in ultra-violet-dosimeter units. These units are based on the sensitivity of the human skin to erythemal-producing wave-lengths.

We found in comparing data taken under the same conditions that an agreement of better than ten per cent could be attained and in some cases it was within five per cent. As a fifth of a division can cause a difference of that amount, the chief sources of error lie in matching the colors of the field-pieces and estimating their values to fifths of a division, although these may be distinguishable with practice. As a difference of 0°5 C in temperature may cause a change of five per cent in value, the temperature-correction must also be correctly known.

Figure 1 illustrates a method that is proposed to determine the dosimeter-readings with more accuracy. After taking an observation in the usual manner and observing the dosimeter-setting, readings to one-tenth division are taken every 30 seconds as the dye returns to its original colorless state. The series obtained by two different observers are plotted here and show the degree of variability possible in a single estimation. However, by averaging the first few results or drawing the most representative line through them, as has been done between AB and A'B', the probable error in estimating a single point of observation might be reduced. It must be pointed out, however, that the decay-curve has different rates of curvature between certain filter-fields. The sharp drop between 5.0 and 3.5 is persistent and may be due to differences in transmission in the green filters or complex chemical reactions in the dye. Pollak [7] has successfully used the photoelectric cell to match the color-intensities in testing instruments but this method limits the freedom of operation.

Exposures were made at an elevation of 220 meters at least once daily at mid-day and oftener whenever clear-sky conditions prevailed. Figure 2 shows the intensities on clear days from February, 1937, to April, 1938, uncorrected for mean solar distance. Values were taken at eleven o'clock, noon, and one o'clock, since the maximum value occurred between these limits. The upper dotted line is drawn through the maximum monthly values, the middle solid line through the daily maximum values, and the lower solid line through the minimum monthly values. The seasonal effect is very marked, increasing values occurring with increasing solar altitude. The minor daily variations dependent on local conditions are quite large, and adjacent days may be found with a difference of 25 per cent. The absolute maximum dosimeter-reading obtained was 20.5 units at 1 o'clock, July 14, 1937, and the minimum was 2.2 at noon, January 28, 1938. These values compare with extremes of 27.8 and 0.61 in July and December, 1935, under different conditions by Landsberg. The autumn values averaged higher than those in spring, as Phillipsborn has found and attributed possibly to the seasonal course of ozone but which Israel-Kohler maintains are due to an incorrect temperature-correction. A recent article by Frankenburger and Hammerschmid [8] shows that there is basis for this view.

The intensity on overcast or cloudy days is quite variable but in general is found to lie below the lower limit for clear-day values. Thin cirrus or high clouds near but not over the Sun may give the same or higher values because of reflections from the cloud-surface. Clouds thick enough to obscure the Sun and reduce the total visible daylight may reduce the dosimeter-reading to less than one-tenth the normal value, the extreme case that was found on several occasions of dense fog.

The diurnal variations are principally caused by changes in the Sun's altitude since hour-to-hour changes in the transmission-coefficient of the clear sky are comparatively slight but definitely noticeable. Figure 3 shows the daily variations in ultra-violet radiation for certain dates selected throughout the year when sufficient observations were taken to draw a smooth curve. The curves are of the general type for total ultra-violet radiation such as Brooks and Wexler [9] found using the General Electric ultra-violet meter at Blue Hill and Mount Washington in 1936. Besides showing the seasonal variations, certain days indicate differences in the location of the maximum and degree of curvature. These are undoubtedly connected with changes of the transmission-coefficient of the atmosphere. By grouping the data for the same solar altitude to eliminate the effects of lengthening air-path, analysis was made for correlation with the differing atmospheric elements. The results may be only briefly mentioned here since no exact values can be given for so short a series of measurements.

The absorption by ozone is important in the ultra-violet region but no means were available for correlating this separately. The effect of turbidity and water-vapor in the air above on absorption of ultra-violet was shown from Kimball's values [10] of β and w based on measurements of the complete solar spectrum made at Blue Hill at the same time as the ultra-violet observations. A decrease in dosimeter-values occurred with an increase of both of Ångstrom's coefficients β and w, which was more marked with the former since the aerosols are of chief importance in short-wave absorption. Perfect correlation was far from obtained because one method measured total radiation and the other only direct sunlight. The dosimeter-values increased with increasing total solar radiation perpendicular to a flat surface, although at a greater rate for increasing solar altitudes.

Correlation between the dosimeter-readings was also obtained with increasing visibility, the amount varying with conditions, but often readily noticeable in successive readings on the same day.

A portion of this study was devoted to the relation between synoptic air-masses and short-wave transmission but no definite conclusion can be given as yet due to the lack of clear-cut air-mass types with clear skies. The value appears to vary within the air-mass, with the time of year, and previous history. The only air-masses having sufficiently clear-sky conditions for numerous observations were Pc, Npc, and some Tg. Since these are more or less limited to different seasons it is difficult to compare them without possibly revising the data further than is logical from the limits of error. There does seem to be a tendency towards maximum transmission with the Pc-type.

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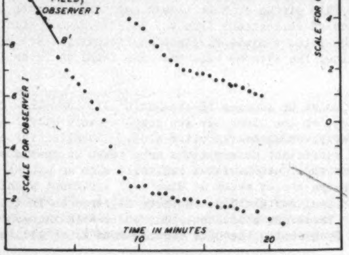

FIG. 1—RATE OF DECAY OF DOSIMETER-COLOR AFTER EXPOSURE

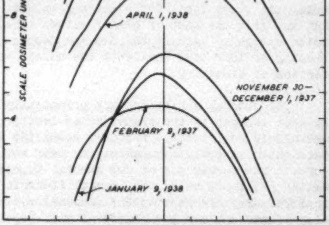

FIG. 3—DAILY VARIATIONS IN ULTRA-VIOLET RADIATION FOR SELECTED DAYS

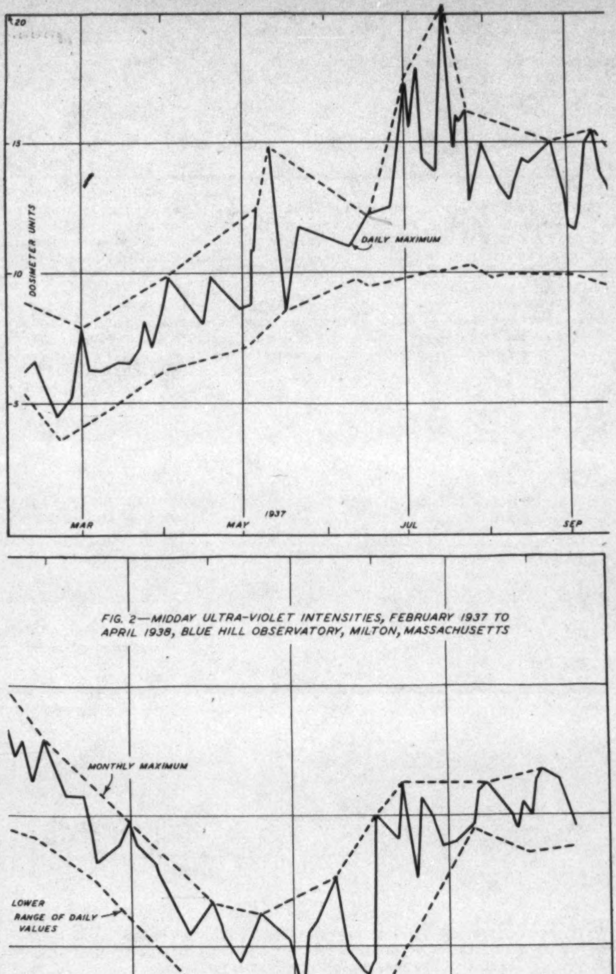

FIG. 2—MIDDAY ULTRA-VIOLET INTENSITIES, FEBRUARY 1937 TO APRIL 1938, BLUE HILL OBSERVATORY, MILTON, MASSACHUSETTS

[9] C. F. Brooks and R. Wexler, Bull. Amer. Met. Soc., v. 18, pp. 298-302, 1937.
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THE METEOROLOGICAL ASPECTS OF CERTAIN RADIO-TRANSMISSION PHENOMENA

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During the past few years, the interest of meteorologists has been increasingly directed to radio transmission by reason of the development of the radio meteorograph and the other services by which radio has come to play an important part in the dissemination of meteorological data. Broadly viewed, in nearly all of these services the primary use of radio has been for the transmission of data obtained by other classical methods.

It is the purpose of this summary to review the present status of work directed toward the utilization of the phenomena of radio transmission in meteorological studies. Some of these possibilities appear to have been relatively neglected although some interesting literature dealing with various aspects of this subject has accumulated during the past ten or fifteen years. Recent additional results give promise of presaging an accelerated future development, and a survey of the field and of these phenomena hence seems appropriate.

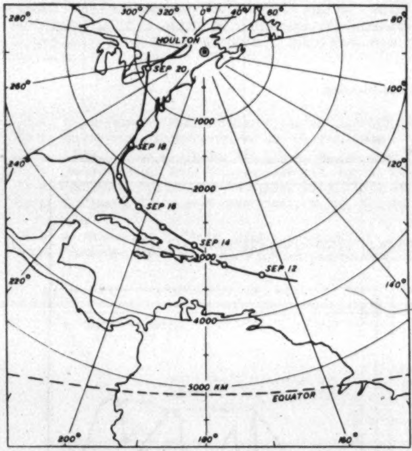


FIG. 1—STATIC AGREEMENT WITH HURRICANE-TRACK AS REPORTED BY DEAN

Proposals for the correlation of radio-transmission studies with meteorology may, for convenienee, be grouped into two general classifications:

(1) Methods for the location and/or the identification of meteorological phenomena, such as highs and lows, by directional observations on associated electrical phenomena such as radio static.

(2) Methods for the location and/or identification of meteorological phenomena by changes in radio signal-intensity due to the reflection, refractions, or extinction of radio signals in the presence of tropospheric discontinuities.

It may be of interest to review some of the history and present status of both of these lines of investigation.

(I) Static studies

History--The quantitative study of the relationship of radio static and meteorological phenomena (particularly lightning) was initiated in England by R. A. Watson Watt and coworkers more than a decade ago [see 1 of "References" at end of paper]. Investigators in North America and Australia, have in most cases reported positive, but not necessarily simple, results [2, 3, 4, 5]. Early methods required: (a) A visual identification of the same static at two or more widely separated points where observer judged simultaneity from voice-calls over a telephone-line connecting the observers [1]; this will be called "Method A." (b) Another method early employed [4] was to study the statistical distribution of static at a single point; this will be called "Method B." During the past ten years, the exacting demands of television for electron-tubes have led to great improvements and refinements in the precision of both the methods employed, but the fundamental principle remains essentially the same.

The most convincing suggestion of the possibilities of extending the above methods to the tracking of tropical cyclonic storms was contained in the paper of S. W. Dean in which the path of an important hurricane in September 1928 was found to coincide with a maximum in the statistical distribution of static [4]. A copy of the agreement thus found is shown in the map borrowed from Dean's paper (see Fig. 1). Kincaid and other observers utilizing merely the bearings obtained by classical loop-direction-finder methods also had reported positive qualitative results.

About the same time the Bureau of Aeronautics of the United States Navy purchased several of the original Watson-Watt low-frequency goniometer-type direction-finders with a view to studying their utility in meteorological services. This study was later continued at the Naval Research Laboratory where more modern equipment was developed. A complete report of the results of these studies has not as yet been published.

Recent static studies in North America--The importance of supplementary methods to aid in the location and tracing of tropical storms, particularly in sea-areas, is generally recognized. Hence, in view of the results just reviewed, a more extended and quantitative study of the possibilities of static methods seemed desirable at points centrally located with respect to the usual paths of these tropical disturbances.

Collaborate observations were hence undertaken at the University of Puerto Rico, near San Juan, Puerto Rico, and at the University of Florida, Gainesville, Florida. (During the past season the observations in Florida were extended to several other stations.) These observations were possible through grants from the Works Progress Administration for the construction and modernization of the necessary equipment. The work was further facilitated with the kind cooperation of the United States Navy and United States Weather Bureau who made available the obsolete Watson-Watt units as a nuclei for the equipment developed. The most important necessary improvement consisted in the utilization of photographic methods both for the recording of the static impulses and of the synchronizing signals. These changes were made possible by the intervening development in cathode-ray tubes. The equipment was thus brought into essentially the same form as that at present employed in England.

A more detailed discussion of the details of the apparatus as applied to our problem is contained in a publication of the University of Florida [6]. A further discussion of the techniques may be found in a recent publication of Watson-Watt and his coworkers [7] so that only brief and incidental mention will be made to the equipment development. The modified Watson-Watt recorder, now adapted to modern tubes, the camera, and cathode-ray equipment located in Puerto Rico are shown by B and C of Figure 2. The building housing the equipment is shown in Figure 2-A. It will be noted that it is located in a field well removed from local disturbances.

Results of recent studies--As a result of the developments outlined, observations have been possible on a number of hurricanes which have passed near Puerto Rico and Florida. During 1935, the available observations were limited to a study of the statistical distribution of the received static which is conveniently called the method of composites (extension of "Method A"). In this method, at regular intervals, time-exposures are taken of the end of the cathode-ray tube which is calibrated like a compass. Each static crash appears as a bidirectional line on the film, and the relative activity of the sources is determined from the density of the film at various angles.

A sample of film thus obtained is shown in Figure 3. Figure 4 shows apparent coincidences of the hurricane-track and statistical peaks (not necessarily the strongest maximum) of static. It will be noted that this is essentially an extension of Dean's method, taking advantage of the improvements arising from the photographic methods made possible by brighter cathode-ray tubes. Another example of the use of the composite method to test the existence of a static maximum in the direction of a storm is shown in the Florida publication already mentioned [6].

Had it developed that, in the tropics where one may be relatively close to a hurricane, an overwhelmingly marked and unmistakably identifiable maximum invariably accompanied the direction of the hurricane-path, the method of composites would offer an ideally simple means of hurricane-location, and two or three records taken at adequately separated points would be sufficient to locate the storm at any time by triangulation. It would only be necessary to use the statistical maximum thus observed at the various points. Only rough timing such as obtained from good chronometers would be required to synchronize the taking of the pictures.

In view of the fairly important maxima indicated on Dean's curves resulting from his early observations (taken at a considerable distance from the path) such a result might a priori have been anticipated. However, the work to date indicates that no such result can invariably be expected. At least, none of the hurricanes thus far observed has been shown to be the dominant source of static on frequencies of the order of 15 kc. This matter will be further discussed and summarized later.

In view of the evidence for the existence of a small static activity accompanying hurricane-centers, it was highly desirable to extend and refine the methods to permit the location of the sources of individual static crashes with considerably certainty.

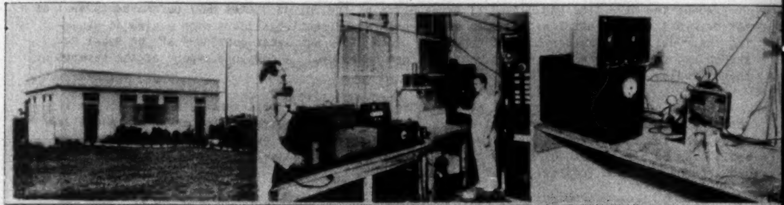
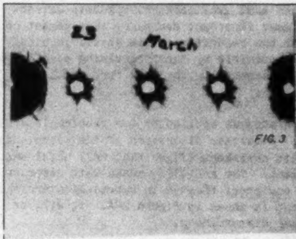
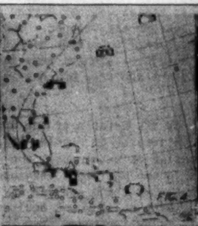


FIG. 2— GENERAL VIEWS: (A) RADIO LABORATORY AT UNIVERSITY OF PUERTO RICO, NOTE LOOPS ON ROOF UTILIZED IN STATIC OBSERVATIONS; (B) STATIC AMPLIFIERS WITH MODERN TUBES AND MODIFIED CIRCUITS BUILT INTO ORIGINAL SET-UP OF WATSON-WATT'S AMPLIFIERS; (C) CAMERAS AND CATHODE-RAY TUBES USED IN COMPOSITE PHOTOGRAPHI RECORDING OF STATIC





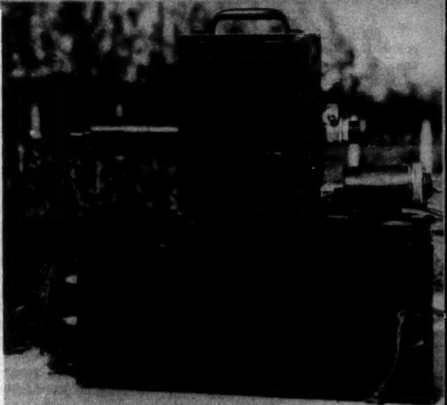


FIG. 3—EXAMPLE COMPOSITE FILM, SHOWING STATIC AS OBSERVED BY TIME-EXPOSURE METHOD (FOGGED FRAMES FOR TIMING)

FIG. 4—EXAMPLE COMPOSITE METHOD IN TRACKING HURRICANE (NOTE COINCIDENCES WITH PATH OF HURRICANE OF AUGUST 29 TO SEPTEMBER 6, 1935)

FIG. 5—EXAMPLE COINCIDENT STATIC OBSERVED BY SYNCHRONIZED-FILM OBSERVATIONS, UNIVERSITY OF FLORIDA AND PUERTO RICO, SEPTEMBER 4, 1937

FIG. 6—MOVING-FILM CAMERA AND TIMING LAMP

By the season of 1936, our development had reached a point where it was possible to employ "Method B." In this method, two or more moving-film records are taken of the end of a cathode-ray tube showing the static received. The films taken at widely separated points are synchronized by signals simultaneously received at all observing points, and automatically registered on the edge of the film by a special timing light. An example of the coincidence observed on such films is shown in Figure 5. The camera set-up is shown in Figure 6. Records taken by this method during the season of 1936, using Florida and Puerto Rico, served to confirm beyond doubt the fact that a substantial portion (but not all) of the static observed on either moving-film record could be undoubtedly identified on both films. Many of the groups observed were so characteristic as to leave no doubt of their identical origin. An example of such a coincidence is shown in Figure 5. Certain (but, in general, only a minority) of these coincident crashes were observed to give bearings intersecting along the path of the storm.

With only two points of observation, it was impossible to know with certainty in-how-far observed departures were due to quadrantal errors (due to local conductors, etc.). At least three observing points hence seemed highly desirable, although it should be pointed out that scatter-phenomena cannot be imputed to quadrantal errors, which vary but slowly with bearing.

During 1937, observations were continued, and the National Research Council of Canada joined in the collaborate program. This made possible observations at Ottawa (Ontario) and Forrest (Manitoba) which could be synchronized with each other and with the observations at San Juan, but not with the observations at Florida (due to poor radio conditions for transmission between Florida and Canada on the available frequencies with the available power). As a result of these additional facilities a much more extended set of data was secured than was previously available. In fact, the total film taken ran into many miles. (A weakness of the moving-film method is unquestionably the labor required for reduction as one minute of run gives five feet of film and contains hundreds of crashes to be located and checked.)

This more comprehensive observational program yielded considerably improved results, and checks of three-point precision also indicated that quadrantal errors were not of major importance at this stage of the development when emphasis was still directed to a study of the activity and scatter of the static sources.

A particularly good agreement was found for the hurricane of August 24-29, 1937, which passed close to Puerto Rico. A comparison of the two-point static fixes (Gainesville-San Juan) is shown in Figure 7. Indicated departures may be due in part to difficulties of precision resulting from oblique intercepts when the storm was nearly in line with the base of triangulation. Some indication of the relative activity of the source can be gathered from reference to Table 1, where the number and percentage of analyzed crashes found along the path are indicated. (In considering the number of crashes of course it should be recalled that the data are based on a limited number of samples of static derived from runs of about one-minute duration--five feet of film.) The morning hours were found to give the easiest film to analyze and to be most productive, since it was much more difficult to separate the storm static from general thunder-storm disturbance in the afternoon. Such activity is, of course, particularly important in the tropics.

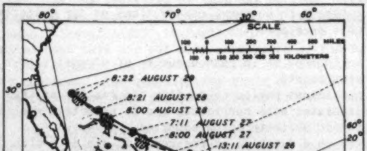


FIG. 7—COMPARISON TRACK OBTAINED FROM MEAN OF STATIC PATH AND THAT REPORTED BY WEATHER BUREAU FOR HURRICANE OF AUGUST 24-29, 1937

Observations have been conducted winter and summer during the years 1936 and 1937, and it is evident that coincident crashes of static can be identified which come from sources so distant that accurate triangulation is impossible over base-lines now available. Some of this static apparently comes from Africa; the most consistent and important sources the year round are the northern portion of the Continent of South America and the Isthmus of Panama. This of course confirms the results of previous investigations [5].

Table 1--Hurricane, August 24-29, 1937, San Juan, Puerto Rico

Date	75° west meridian time	Coincidences	P. R.	Per cent
1937	h m			
Aug. 25	14 59	15	500	3
26	13 11	3	400	0.75
27	7 11	25	150	16.7
28	8 21	20	175	11.4
29	8 12	16	150	10.7

This very great effective range of static on 15 kc, however, naturally serves to reduce the percentage observed from relatively local sources such as hurricanes, and raises a question as to whether some other frequency-range may not present better propagation-characteristics for differentiating the storm-static from distance sources. Thus, even during periods of hurricanes, the South American sources show a dominant activity as will evidently be seen from reference to Table 1.

Summary of observations so far conducted on tropical static indicate:

(1) That the method of composites in general gives a small statistical maximum which follows hurricane paths.
(2) That the intensity of the static following the hurricane is relatively small compared to the total static observed at 15 kc.
(3) That the method of synchronized moving-film observations on 15 kc confirms the existence of a large amount of static which may be simultaneously recorded at points thousands of miles apart.
(4) That static from sources as distant as 8000 or 10,000 miles is frequently recordable, particularly in winter.
(5) That the distant static (and, in summer, local thunder-storms) renders more difficult the identification of hurricane-static on moving-film records and serves to seriously confuse composites.
(6) That the region of emission of hurricane-static is in general a considerable area in the vicinity of the storm.
(7) That the activity of the storms, both as to frequency and intensity of static, varies considerably and that the activity of a given storm varies over its path, being greatest in the tropics and generally less at northern latitudes.
(8) That the observed activity may be due to lightning in accompanying squalls (this is not yet certain).
(9) That a considerable amount of static is frequently observed in the direction of the storm at the nearer observing point, but corresponding crashes cannot invariably be located at the more distant point. If this static is in fact due to the hurricane, it indicates low-intensity sources which may not be due to lightning. This static may, of course, be due to more local effects, and in the absence of two-point fixes it has not been considered as hurricane-static. Under some conditions such static crashes are much more frequent than static giving the two-point fixes.

Outline of desirable future static investigations--In summary, observations so far conducted on tropical static indicate the following desirable developments:

(1) The extension of the triangulation-program to include an increased number of stragetically located and universally synchronized observing points.
(2) The continuation of the observations for a sufficient period to confirm the reality of the apparent existence of the static sources associated with hurricanes and to ascertain statistically the relative activity and variability in activity to be expected.
(3) A comparative study of the intensity of static as a function of frequency to see if a high correlation exists between the static on various frequencies and to determine the optimum frequency for the highest ratio of desired to other static sources. Thus, if some of the static arises from low-energy short-path discharges, an increase in relative intensity should be found on the higher frequencies (say 150 kc). In this connection, it is of interest to note that Dean's results were for about 50 kc.

During the coming hurricane-season, it is expected that increasing emphasis will be placed on these lines of development.

(II) Radio refraction- and reflection-phenomena

Inasmuch as it is the intent of this summary to present a survey of work relating radio phenomena to meteorological problems, it may seem that disproportionate emphasis has been placed on the static investigation with which the authors are, of course, most intimately familiar.

However, while the amount of published material and available data on tropospheric refraction-

and reflection-phenomena is as yet somewhat limited, there appears to be well-sustained evidence for important radio-transmission phenomena which are intimately correlatable with weather-phenomena. While many informed radio engineers for many years doubted the existence of such a relationship, the phenomena recently reported seem to indicate that history is about to repeat itself, and that we are going to find it necessary to considerably revise and extend our concepts of the part played by the troposphere in radio transmission. In this connection, a brief historical survey of the gradual development of the appreciation of the importance of the Earth's atmosphere as a whole in radio transmission is of interest.

Radio-transmission phenomena and the atmosphere

History--In the early years of the present century, within a decade of Marconi's experimental work, two noted scientists, A. E. Kennelly [8] and Oliver Heaviside [9] suggested that the Earth's atmosphere might play an important, if not (as we now know) dominant role in radio transmission. Nearly a quarter of a century elapsed, however, before the development of the art permitted a quantitative verification of the importance of these atmospheric effects. During the major portion of this period, the early suggestions relative to the importance of atmospheric phenomena were largely forgotten, or viewed with incredulity, and the mathematical physicists of the day sought with but little success to explain the observed facts by considering the Earth as an isolated conducting sphere [10, 11, 12, 13].

The rapid development of the early 1920's, however, extended the useful radio-frequency spectrum into the range embracing from two to 20 megacycles per second where the atmospheric phenomena played such a dominant part that their identification and subsequent quantitative study progressed rapidly in the decade between 1920 and 1930. These studies disclosed the dominant phenomena in this frequency-region to be associated with stratospheric ionization-phenomena.

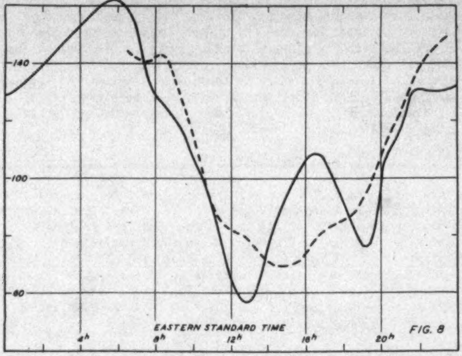
During this period various investigators tried rather unsuccessfully to correlate the observed phenomena of transmission with tropospheric atmospheric phenomena, notably those associated with weather. We now suspect that much of this failure was due to inexperience in the choice of the variables to be correlated, insufficient quantitative data, and, for the most part, inadequate statistical treatment. These are all pertinent and vital considerations in this frequency-region for, just as stratospheric phenomena had been masked by ground-wave propagation in the low-frequency era of 1900-20, so tropospheric phenomena were masked in the two to 20 megacycles per second era of 1920-30 by dominant stratospheric phenomena.

The decade of the 1930's, however, has seen a rapid development in the region of 20 to 200 megacycles per second, in which stratospheric phenomena are frequently absent, and where tropospheric phenomena hence begin to manifest themselves to almost as striking an extent as the so-called (stratospheric) sky-waves had in the 1920's in the region of two to 20 megacycles per second. At the beginning of this era of 1930, transmission at frequencies above 30 megacycles per second was known as "transmission in the quasi-optical range", which was at first construed to mean "line-of-sight" transmission. Pickard, Hull, Brooks, and others, however, soon demonstrated that this did not mean an absence of fading, for the troposphere proved capable of exhibiting diffraction-phenomena which extended the ultra-frequency line-of-sight range to some five horizons. The quantitative study of these tropospheric radio-transmission phenomena appears to be one of the most promising fields for the next decade's development.

Radio transmission and the weather--With the appreciation of (and means for the quantitative study of) tropospheric radio-transmission phenomena, the paths of the radio physicist and the meteorologists are converging far more rapidly than ever before, for most of the enormous changes observable in tropospheric radio-transmission appear to be associated with temperature-inversions associated with upper-air movements. The possibilities for the use of such phenomena in connection with the study of weather and of weather-prediction remain a relatively unexplored field.

We are indebted to G. W. Pickard for some specific examples of defraction-phenomena resulting from observations of radio transmission between Blue Hill and Mount Washington. Figure 8 shows indisputably that ultra-frequency signals possess fading characteristics of considerable magnitude which apparently can only be attributed to tropospheric phenomena. The meteorological relationship is shown by the marked thunder-storm correlation shown in Figure 9. The reality of these phenomena and their weather-correlations have been further confirmed by Ross Hull by field-intensity observations on Boston ultra-frequency stations at Hartford at more than five times the optical path. The amount of bending required to produce these phenomena is of course only of the order of the earth-curvature due to the low-angle paths.

A far more startling group of phenomena have, however, been reported by Friend and Colwell



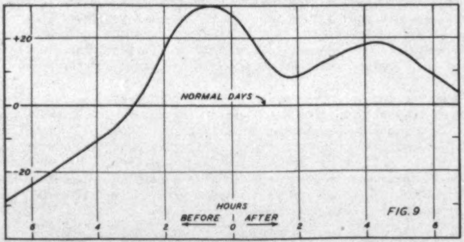


FIG. 8—FADING CHARACTERISTIC OBSERVED BY PICKARD IN 60-MEGACYCLE TRANSMISSION FROM BLUE HILL TO MT. WASHINGTON
FIG. 9—RELATION MEAN SIGNAL-INTENSITY OBSERVED BY PICKARD FOR BLUE HILL-MT. WASHINGTON PATH AND THUNDER-STORM ACTIVITY

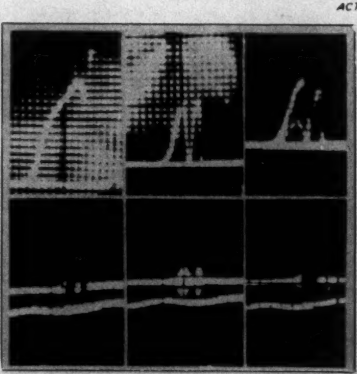


FIG. 10—EXAMPLE TROPOSPHERIC REFLECTIONS OBSERVED BY FRIEND AND COLWELL: (A) 15^h15^m, OCTOBER 22, 1936, 5.1 KM; (B) 20^h55^m, OCTOBER 26, 1936, 3.7 KM; (C) 16^h00m, JANUARY 23, 1937, 3.6 KM; (D) 21^h00^m, JUNE 23, 1936, 5.8 AND 8.0 KM; (E) 20^h30^m, JUNE 23, 1936, 6.4 KM; (F) 11^h45^m, JUNE 19, 1936, 3.3 KM (WITHOUT LOOP, DURING ECLIPSE-PERIOD)

[14, 15, 16], who observe echoes of radio signals from heights as low as 0.7 km and 10 km (see Figures 10 and 11). It is very difficult to see how such reflections at apparently normal incidence can exist in the troposphere where the ionization is generally considered to be many orders of magnitude too small. Some sort of directive diffraction- or reflection-phenomena appears to be involved in the explanation of these results. This view is further sustained by observed weather-correlations accompanying both major lows and local thunder-showers. In view of the importance of these phenomena, preparations are now being made to make similar observations in Puerto Rico as soon as the considerable development of apparatus required can be carried out. This will permit us to see if the phenomena can be reproduced in a tropical-island climate.

In the absence of a more complete study of the nature of the phenomena, it is difficult to anticipate what type of discontinuities may be expected to produce such reflections, but it is believed worth while to investigate the possibility of securing them

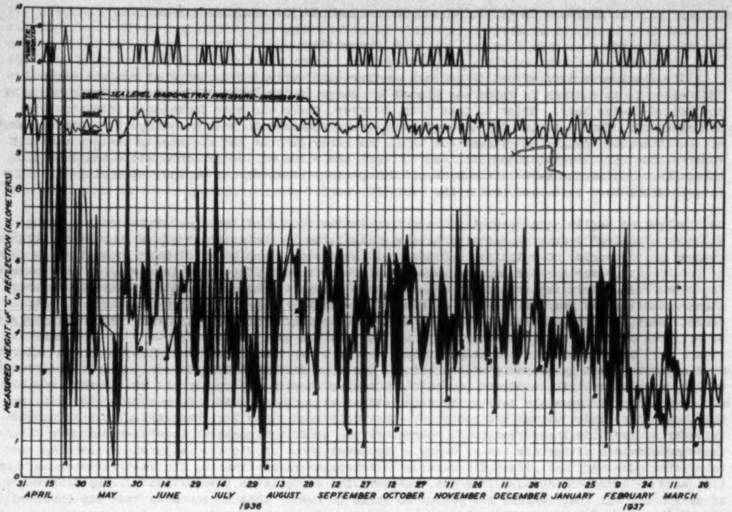


FIG. 11—EXAMPLE LONG-PERIOD VARIATION IN HEIGHT OF REGION GIVING TROPOSPHERIC REFLECTIONS (FRIEND AND COLWELL)

from hurricanes. If the atmospheric gradients accompanying these storms give rise to such reflections, a directional-receiving system should permit a ready determination of both the distance and bearing of the storm from a single receiving point. In any case, if their reality is finally established (and the evidence is now quite strong) the study of tropospheric reflections during the next few years bids fair to be even more fruitful to meteorology than the ionospheric studies have been to terrestrial magnetism and allied sciences during the last decade.

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PROGRESS IN COMPILING AND SUMMARIZING SHIPS' WEATHER-OBSERVATIONS

I. R. Tannehill

The first organized effort to collect weather observations from ships at sea was begun in 1841 when Lieut. Matthew F. Maury was placed in charge of the Depot of Charts and Instruments of the United States Navy. Prior to that time, however, there was some individual investigation and research. For centuries it had been customary for seamen to keep some form of record of weather-conditions on voyages but it was not until the early part of the nineteenth century that the observations were assembled and utilized to chart the winds of the cyclonic storm and to depict other phenomena of the Earth's atmosphere. Our present knowledge of marine meteorology has in a large measure been developed during the past 100 years.

Since the beginning of organized reporting in the middle of the nineteenth century, many millions of ships' weather-observations have accumulated in the archives of various meteorological bureaus of the world. Compilation and study of the observations have not kept pace with the flow of data into the various bureaus, because there are serious difficulties in compiling and summarizing them. The chief complication is that the ship's report frequently contains several observations--sometimes as many as 20--on a single record-sheet. Each observation is taken at a different location during the ship's voyage, hence each one must be separated and reassembled together with the observations from other ships so that all the data which refer to a given ocean-square will be brought into one compilation. On the other hand, when each observation is entered on a separate sheet by the ship's observer, as has been done at intervals in the past, the records become so bulky that it is almost impossible to handle them. Consequently all this work of sorting and reassembly is necessary before the observations are organized in a form similar to that in which data for land-stations are recorded. Many different plans have been tried such as manual transfer of individual observations to cards, slips, or to the pages of log-books but none of these methods was altogether satisfactory. The development of electrical punching, sorting, and tabulating machinery has provided a more satisfactory means of treating the observations. By the time when machine methods came into use, however, the accumulated observations in the archives of meteorological institutions had become so large in number that an enormous task was involved even with the most modern equipment.

By 1932, the Weather Bureau had in its files approximately 5,500,000 observations from ships. In a project of the Civil Works Administration, the observations during 1912 to 1931, inclusive, about 2,000,000 in number, were transferred to punch-cards. This work was described by W. F. McDonald at the meeting of the Section of Meteorology of the American Geophysical Union in 1934.

Beginning in March 1936, a project of the Works Progress Administration was organized at New Orleans to compile all of the observations. The data prior to 1912 were transferred to individual slips, tabulated and combined with the punch-card data for the years 1912 to 1931. In this way the entire collection covering a period of more than 50 years was sorted, tabulated, and summarized by 5° squares for all the navigable parts of the oceans of the world. This work was done under the technical supervision of W. F. McDonald.

In all, nearly 50,000 pounds of reports, charts, transcription-slips, punch-cards, tabulation-sheets, and summary-cards were treated in this project. The number of workers varied from about 200 to 250. About two years' time has been required, although some of the workers were placed on other phases of Weather Bureau work at intervals during the two years.

The elements included in the summarization are: Direction and force of wind; temperature of air, water, and wet bulb; amount and kind of clouds; fog, mist, and haze; frequency of gales and thunder-storms; exceptional visibility; and precipitation.

Barometric data have not been compiled because an enormous amount of investigation into the errors of barometers would be involved. In the average monthly distribution of barometric pressure over the oceans, the differences are relatively small and there is insufficient information as to the accuracy of a large percentage of the barometers, especially in the earlier records, to justify use of the data in determining barometric-pressure distribution in a project of this character.

The summarized records have been transferred to cards, one for each month for each 5° square. The averages have also been entered on world-maps which show the distribution of each element by months or seasons. This phase of the work is nearly completed.

There is one noteworthy deficiency in the world-picture shown in these charts. Practically all of the observations were taken at Greenwich mean noon, hence the local time of day varies with the longitude and there is a resultant diurnal variation which can not be evaluated with complete satisfaction. Since the diurnal variation of meteorological elements is not large over the oceans, this deficiency is not so important as would be the case with similar charts of continental conditions. In more recent years ships' weather-observations have been taken regularly at other hours. When sufficient numbers of these observations at other hours are accumulated it will be possible to determine the character of the diurnal variation of the several elements.

A number of special studies have been undertaken in connection with the project. These include: Temperature-history tabulations; wind-circulation over the oceans in relation to dry and wet periods over the Southern Plains States; compiling of resultant winds; determination of extremes of low temperature in certain ocean-areas; and assembly of observations for hours other than Greenwich noon to study diurnal effects in a few regions where ample data are now available.

Much of the early material had been transferred from the Hydrographic Office to the Weather Bureau in 1904. This material was included in the general compilation excepting about 2200 log-books which were indexed in the projects of the Civil Works Administration and the Works Progress Administration. After indexing, the books were transferred to the National Archives. Most of these log-books contain records of weather-conditions on voyages about the middle of the nineteenth century but many were of earlier dates. The earliest of the logs was that of the Empress of China on the first voyage of a merchant ship of the United States to China. The voyage was made in 1784.

With the completion of this task of sorting, compiling, and summarizing ships' weather-observations, the United States Weather Bureau Marine Division is furnishing data to the Hydrographic Office for revision of the wind-roses, gale-charts, fog-frequencies, and weather-summaries on the Pilot Charts. The data for the Pilot Charts of the North Pacific Ocean are complete and publication is expected to begin in January 1939. New wind-roses for other oceans will follow. The summarized data are also proving extremely valuable in connection with conditioning of ships' cargoes, naval aviation, and maritime navigation and commerce in general. Publication of the summarized data in the form of world-charts, tables, and descriptive text is making progress.

As hundreds of nimble fingers have sorted these millions of weather-observations and indexed the old logs, many interesting items concerning shipping of the last century have come to light. One Navy officer finished his weather-report with the remark that he had entered no barometer-readings because the instruments were all broken in the bombardment of Santiago de Cuba. There are sketches of whales and insects and of the people seen on voyages; notes on approaches to harbors; meteors observed; small boats capsized in the surf; the quality of the food; the furling of topgallant sails; the passage of a man-of-war; cases of scurvy aboard ship; and a host of other subjects that seemed important to the ship's weather-observer at the moment. Interesting as much of this material is, there has been no time to compile anything other than useful information about the weather and the sea.

One strong impression that is left after this wholesale examination of ships' weather-reports is that the work of the mariner has always been strongly marked by good will and cooperation. No pains were spared in the earlier days to provide data that would be helpful to others who might traverse the same route at a later data. The needs of the meteorologist in nearly all instances have been met as fully and carefully as circumstances permitted.

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METEOROLOGICAL RESEARCH CONTEMPLATED FOR AN EXPEDITION TO NORTH GREENLAND

Max Demorest

Introduction--Meteorological studies in Greenland are certain of leading to interesting results of both practical and theoretical value. These values arise in part from the fact that Greenland is unique in the Northern Hemisphere, having a great cap of inland-ice which covers all but a narrow border of land about its margin.

Professor Hobbs in his theory of the glacial anticyclone has considered this continental glacier to be overlain by a persistent system of descending air-currents, making Greenland the wind-pole of the Northern Hemisphere and, at the same time, accounting for the accumulation and distribution of snow which maintains the great ice-cap. Others who oppose or would greatly modify this theory have suggested that any such high-pressure system over a continental glacier must be ephemeral and of little significance in either wide-spread general wind-circulation or

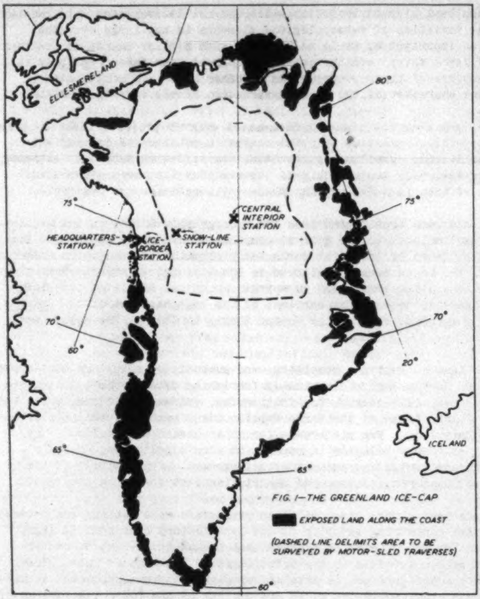

FIG. 1—THE GREENLAND ICE-CAP

■ EXPOSED LAND ALONG THE COAST

(DASHED LINE DELIMITS AREA TO BE SURVEYED BY MOTOR-SLED TRAVERSES)

in the maintenance of the ice-cap. They consider that marginal low-pressure disturbances migrate onto the glacier, penetrating far inland and carrying great amounts of moisture which are there precipitated, thus accounting for the snow-supply which maintains the ice-cap, and emphasizing the weakness of any anticyclonic circulation.

There exists a body of factual evidence, part of which seems to support one idea, and part the other, and yet another part which seems to support both. Perhaps the truth lies in a middle-ground interpretation that recognizes both extremes. In any case it is important that we should arrive at a correct understanding of this atmospheric circulation over Greenland. Not only does this bear directly on the problems of growth and maintenance of the ice-mass, and therefore on theories of continental glaciation and the paleoclimatology of ancient ice-ages, but it also has a highly significant bearing on the general problems of weather-forecasting in high latitudes, which is important to shipping and commercial flying.

The meteorological program--The proposed expedition will be established on the west coast of Greenland in approximate latitude 74° 30' north (see Fig. 1). It will be in the field from late June or early July of 1940 until October of 1941, a total of about 15 months. Half of the staff of ten or twelve men will be employed in meteorological and glacio-meteorological research. Two meteorological observatories will be maintained throughout the duration of the Expedition, one, the headquarters-station, on a coastal island lying at some distance from the ice-border, and the second at the ice-border. Synoptic ground-observations will be carried on in the routine manner, and aerological observations made by pilot-balloon and radiometeorograph.

Early in the spring of 1941, probably in late April, it is proposed to establish two additional stations on the ice-cap. One will be located at or just above the snow-line which is here 40 to 50 miles inland and at an elevation of about 4,000 feet. Since the snow-line can not be determined at that season of the year, its approximate location will be found during the preceding summer when a party of glaciologists will devote their time to studies of the ice-border. The other inland-station will be located near the middle of the ice-cap in approximate latitude 76° north. At these two stations routine ground-observations will be carried on synoptically with those of the permanent stations. In addition, the central station will carry out aerological observations with pilot-balloons and radiometeorographs. All stations will be kept in communication by means of short-wave radio so that data at any one station will always be available to the others for immediate comparison.

The synoptic ground-observations at all stations will include determinations of temperature, air-pressure, humidity, precipitation (including frost-accumulations of all sorts), evaporation from snow- and water-surfaces, types and movements of clouds, and solar radiation. Self-recording instruments will be used to give a continuous record of as many of these phenomena as possible.

In addition to this program, parties of glaciologists and geologists, which will be traveling on the inland-ice and along the coast, will make regular meteorological observations while en route, thus supplementing the work of the more permanent stations. Of particular importance in this respect will be a party of glaciologists which, by means of motor-sleds of a new design, will make at least six and probably more traverses of the northern third of the ice-cap.

The importance of the synoptic observations at the four stations and the wide-spread data to be gathered by the motor-sled party may be emphasized, for it is in this manner only that studies of the general circulation over the ice-cap can be made. In this respect the distribution of the stations is of particular interest, the four of them being so placed that they will give an atmospheric cross-section reaching from the coastal islands to the center of the ice-cap. The distance between any two of the stations will be short enough to eliminate the dangers of interpolation and extrapolation--factors that have led to conflicting interpretations of results from more wide-spread stations maintained on previous occasions.

Such a distribution of the stations is particularly to be desired on the west side of the ice-cap, for small "lows" are known to migrate northward along the west coast. Synoptic observations will reveal the distance inland to which these lows penetrate, and will therefore be of critical importance in substantiating or modifying either the anticyclonic or other theories of air-circulation over an ice-cap.

It is expected that the meteorological staff of the expedition will be under the immediate direction of H. G. Dorsey, Jr., who is experienced in the "arctic" conditions of Mount Washington, and who is now studying engineering and meteorology at Harvard and Massachusetts Institute of Technology; he is also an observer and research-assistant at the Blue Hill Observatory of Harvard University. Dorsey and the rest of the staff will keep their data summarized as far as possible while in the field and should therefore be able to publish the results within a short time after the return of the Expedition.

Glacio-meteorological program--Since the glacio-meteorological program contemplated by the Expedition is discussed in detail in a paper presented to the Section of Hydrology of the American Geophysical Union at this meeting, it needs little more than brief mention here. The object of this work is to gain quantitative as well as qualitative data on the manner in which meteorological phenomena affect the "economy" (particularly ablation and alimentation) of the glacier in its different parts. The methods used will include those developed by Professor J. E. Church (known as the Mount Rose snow-survey system) and those more recently worked out by H. W. Ahlmann and H. U. Sverdrup for glacial studies in Svalbard. The three meteorological stations established on the ice-cap (the border-station, the snow-line station, and the central interior station) will be of chief importance in carrying out these studies. Significant data will also be gathered during the winter at the headquarters-station erected on one of the coastal islands.

Glacio-meteorological problems that might be neglected in a strictly meteorological study are determinations of evaporation and melting on snow- and ice-surfaces and the measurements of temperature-gradients within the neve of the interior. The techniques of Church, Ahlmann, and Sverdrup are especially designed for these problems and will be used extensively. In addition, a new technique, as yet untried, will be used in attempting to determine the temperature-gradient of the neve to an extreme depth. It is hoped that temperatures at the central interior station can be measured to the lower limit of unconsolidated neve. The instrument designed for this purpose is described in detail in the paper presented before the Section of Hydrology.

Another problem which will be studied, and which is as yet unsolved, is the measurement of erosion and transportation of snow by wind-drift. So far as snow carried in the air is concerned, this will probably remain an unsolved problem, for it is impossible to distinguish high-drifting snow from falling snow. However, there is considerable hope that significant determinations of wind-erosion can be made from given measured areas, and perhaps some knowledge of the quantities of snow swept along on the surface in moderate winds can be obtained. Such erosion and transportation is unquestionably an important ablation-factor of the interior regions of the ice-cap.

In closing it may be stated that the complete plans for this Expedition, including other programs of study, have been drawn up by the author and approved by the American Geographical Society and the Danish Commission of Scientific Exploration in Greenland. The program of meteorological studies has been presented here with the hope that authoritative criticism and suggestions may be offered, for in that way the value of the program may be enhanced.

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THE VELOCITY OF SOUND-WAVES FROM GUN-FIRE IN SOUTHERN CALIFORNIA

B. Gutenberg

Since October, 1937, a sensitive instrument for recording changes in air-pressure has been in operation at the Seismological Laboratory in Pasadena. The instrument was designed by Dr. H. Benioff, who will describe it more fully elsewhere. He gives here the following short description of the instrument:

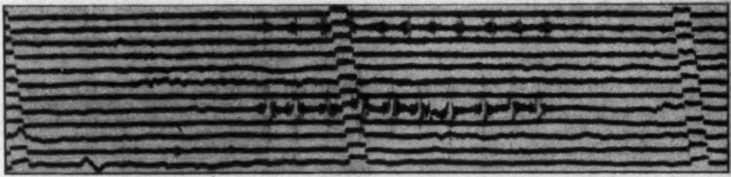

FIG. 1—SOUND-WAVES RECORDED BY SHORT-PERIOD BENIOFF BAROGRAPH, PASADENA, NOVEMBER 9, 1937; TIME INCREASES FROM LEFT TO RIGHT; MINUTE MARK IN UPPER GROUP OF SOUND-WAVES BEGINS AT $9^h 51^m 22^s$, CORRESPONDING MARK IN SECOND GROUP AT $11^h 21^m 22^s$, PACIFIC STANDARD TIME

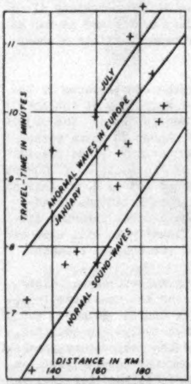

FIG. 2—TRAVEL-TIMES OF SOUND-WAVES IN SOUTHERN CALIFORNIA, NOVEMBER 9-10, 1937

"The responding element consists of a permanent magnet moving-conductor-type loudspeaker mounted in one of the sides of a sealed container of approximately one-fifth cubic meter capacity. The effective diameter of the loudspeaker cone is six inches. The natural period of vibration of the cone assembly is approximately 150 cycles per second. Hence for frequencies less than 50 cycles per second the cone-displacement is proportional to the atmospheric pressure increment and the electromotive force induced in the coil is proportional to the rate of change of atmospheric pressure. The output-currents are recorded on a standard siesmograph recorder having a galvanometer of 0.25-second period, somewhat over-damped and a drum-speed of one mm per second at the periphery."

This instrument recorded sound-waves on November 9 and 10, 1937, an example of which is given in Figure 1. As it seemed very probable that these sound-waves originated from gun-fire in target-practice by the United States Navy, a request for details was addressed to the Navy. The locations of the firing ships, which were approximately south of Pasadena, were kindly furnished by Captain H. Leary to the nearest minute in longitude and latitude, and the "time of commence firing" and of "cease firing" to the nearest minute. The correlation between these data and the time at which the sound-waves were recorded at Pasadena leaves no doubt that all the instances of recorded impulses like those shown in Figure 1 actually originated at this target-practice. The travel-times for the first sound-waves in each instance are plotted in Figure 2. As may be seen from Figure 2, the observed sound-waves are too late for normal sound-waves by an average of about 1.5 minutes and there are only a few observations which may belong to normal waves. The points scatter about the curve found as an average for the propagation of sound-waves in Europe during winter (see, for example, B. Gutenberg, Die Schallausbreitung in der Atmosphäre, Handbuch Geophysik, v. 9, Berlin, 1932). The scattering of points rarely exceeds 40 seconds. Such differences are to be expected as the time was given only to the nearest minute. The results leave no doubt that the propagation of sound-waves in the stratosphere here is similar to that in Europe, which means that in all probability the temperature increases in the stratosphere with increasing height beginning at a level of the order of 30 km, and at higher levels exceeds the temperature near the ground.

In the interim a large number of additional instances of these sound-waves has been recorded in Pasadena. These data promise further results as soon as the data concerning the location of the firing ships and the time of firing are made available by the Navy.

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JOINT MEETING

SECTIONS OF METEOROLOGY AND OCEANOGRAPHY

Washington, D. C.
April 28, 1938

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RECENT OBSERVATIONS FROM THE GULF STREAM AND NEIGHBORING WATERS

C. O'D. Iselin

Since 1931 the Atlantis has been collecting data on the internal distribution of temperature and salinity in the western half of the North Atlantic Ocean. In 1936 a report [see 1 of "References" at end of paper] was published dealing in a preliminary way with the then available observations. In particular, a number of sections crossing the Gulf Stream System at various points between the Straits of Florida and Cape Sable, Nova Scotia, were discussed. For two of the lines, Cape Sable-Bermuda and Chesapeake Bay-Bermuda, several sets of data had been obtained so that not only could the internal structure of the Gulf Stream be examined in cross-section and longitudinally, but also it seemed probable that the Current holds to a fairly definite path and varies only slightly in strength from season to season. On the whole these and other general conclusions have been supported by more recent observations.

Since 1935 the Woods Hole Oceanographic Institution has continued to investigate the Gulf Stream and especially observations have been collected for two special studies. First, during 1936 a series of five offshore-sections between Georges Banks and the Grand Banks were occupied on three different occasions (March, June, and September) in order to more fully explore the so-called eastern slope-water area. Secondly, beginning in June 1937 a section extending from Montauk Point (Long Island) to Bermuda has been repeated at intervals of about six weeks in order to learn whether or not the volume of the Gulf Stream has a significant annual variation. The latter investigation will have to be continued for several years before any positive answer can be given. However, a preliminary study of both series of data has shown that on both sides of the Gulf Stream deep, and relatively intense, disturbances are more common than was at first thought.

The western North Atlantic, between the Straits of Florida and the Grand Banks, apparently differs in one important respect from other parts of the Ocean. Here the main current is so powerful that there can be little doubt that the resulting internal distribution of density closely follows the principles embodied in the circulation theorem of Bjerknes. In short, here we have an ocean-current for which the available methods of investigation are relatively satisfactory.

With currents weaker and broader than the Gulf Stream the ordinary subsurface-temperature and salinity-observations are often for a number of reasons difficult to interpret dynamically. In oceanographic sections the stations can, of course, never be occupied simultaneously. Because of internal waves and other short-period fluctuations, dynamic calculations from station-data made in regions of weak currents are often inconsistant with other lines of evidence. In addition, the local winds and the local absorption and radiation of heat may influence the computed topography of the sea-surface. But in the Gulf Stream and in some of the disturbances of the neighboring waters the dynamic gradients are so steep that they cannot often be in doubt.

All modern sections across the Gulf Stream's path show that the dynamic height increases steadily from west to east. The computed velocity-profiles seem consistant with the distribution of temperature, salinity, and dissolved oxygen. The only doubt that has been raised involves the choice of the base-plane or level of zero-velocity. What is the proper limit to set for the current? Opinions differ and direct observations to settle this important point are difficult to secure. The navigational evidence collected during recent cruises of the Atlantis seems to demand a fairly deep current, at least 2000 meters deep, from Cape Hatteras to the Grand Banks.

In contrast with the relatively steady main current, the movements in the water-masses on either side of it seem to be much more variable. For example, shifting and strong anticlockwise eddies apparently frequently develop between the Gulf Stream and Bermuda. In the colder seas northwest of the Current, especially from Georges Banks eastward, clockwise whirls are often observed. The Atlantis sections of 1936 and 1937 provide excellent material for the study of these lateral disturbances, very probably associated with the dissipation of the Gulf Stream's energy.

A single section, of course, cannot tell us whether these disturbances are eddies or elongated troughs and ridges. However, in the Atlantis survey of 1936 of the eastern slope-water area, the sections were spaced about 60 miles apart and in most cases provide a sufficiently close network of observations to demonstrate that there at least such disturbances are indeed rotary. In all cases so far examined the navigational reckoning confirms the current-direction demanded by the trends of the isopicnals.

Concerning the duration of such eddies, all that can now be said is that they are not fixed in location and that probably they die away gradually over a period of a month or more, while drifting along with the slower, general circulation. On several occasions the surface-velocities experienced in crossing these eddies approached that of the Gulf Stream itself. In both the slope-water and in the Sargasso Sea the sections indicate a diameter for such disturbances of about 40 miles, the isopicnals being sharply slanted down to depths well below the main thermocline.

From time to time it has been suggested, both by navigators and scientists, that the Gulf Stream was flanked by counter-currents. These reports from navigators can probably be explained as eddies. However, counter-currents at the surface are sometimes called for by the dynamic contours, especially when a shallow base-plane is used [2]. An examination of the many Atlantis sections reveals that if counter-currents are present in the surface-layer on either side of the Gulf Stream, they must be relatively shallow and weak in comparison with the eddies. Especially on the Sargasso Sea side, no general southwesterly movement is called for by the sections except the slow return-flow from the Grand Banks Region [1] that passes just west of Bermuda. Between the Gulf Stream and the continental shelf, except when an eddy is met with, the trend of the isopicnals calls for a slow movement, in the same direction as the Gulf Stream, right in to the contact of the coastal water and the slope-water.

The processes whereby intermediate values in temperature and salinity are produced in the surface-layer (down to 200 meters) of the slope-water through the mixing of coastal water and bodies of Gulf Stream water that have escaped north of the main Current, are extremely complex. The more closely the stations are located, the more complicated the sections become to draw and to interpret.

The many interesting points that have been raised by the Atlantis observations of 1936 and 1937 from the Gulf Stream Area are now being studied and it is hoped that a report can be completed in the course of the next few months.

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SOLENOIDAL CIRCULATIONS RESULTING FROM LATERAL MIXING

C.-G. Rossby

Within the broad current-systems of the atmosphere, lateral (isentropic) shearing stresses must develop whenever the gradient wind-velocity varies from one current-filament to the next within the same isentropic sheet. If such a current-filament moves more rapidly than its isentropic environment, it will be subjected to a retarding force; if it moves more slowly, it will be subjected to an accelerating force. It is evident that the frictional volume-forces resulting from isentropic miximg must be very nearly parallel to the current-axis whenever steady motion prevails.

It has been pointed out by Ekman [1932] that on a rotating plane, such as one may use to represent a small portion of the Earth's surface, the effect of a constant horizontal force P per unit-mass is to produce a steady horizontal velocity v perpendicular to the force and (on the Northern Hemisphere) to the right thereof. The velocity v may be computed from the equation

$$P = fv \tag{1}$$

where f is the Coriolis' parameter. (Superimposed on this steady displacement is an inertia-oscillation, which may play an important rôle in the mechanism of lateral turbulence.) It follows that a broad, steady atmospheric current within which the axial velocity varies from point to point in a plane normal to the current-axis, must possess a transversal circulation the intensity of which may be computed from (1) as soon as the mass-forces resulting from isentropic mixing have been determined.

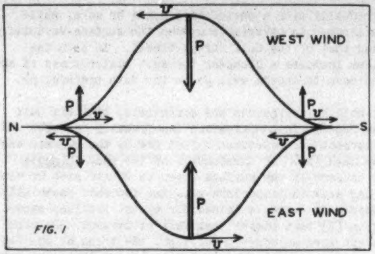

FIG. 1

The preceding points are illustrated by the sketch in Figure 1. The upper curve represents the assumed velocity-distribution in a broad current from the west, measured along an isentropic line in a vertical section normal to the current-axis. The lower curve represents the corresponding velocity-distribution in a broad current from the east. On both curves vertical arrows have been drawn to indicate the direction of the frictional volume-forces resulting from isentropic mixing. The horizontal arrows give the direction of the corresponding transversal velocity-components as computed with the aid of (1).

It is apparent that these transversal components lead to convergence in certain regions, divergence in other regions. From the requirement of continuity one may compute the resulting vertical velocities (w).

As soon as v and w are known it is possible to determine the stream-lines in the transversal plane. These stream-lines will normally intersect the isentropic surfaces (represented by lines in the transversal plane). From the speed at which the air crosses the isentropic surfaces one may finally compute the amount of heat which has to be added from the outside or given off at each point to maintain a constant potential temperature-distribution and hence a constant gradient current-distribution.

The transversal section in Figure 2 represents the result of such a calculation. The system consists of a broad current from the west, extending downward from the 12-km level to the isentropic surface $\theta = 304°$ (approximately), and of an equally broad current from the east between this surface and the sea-level. The potential temperature-distribution is prescribed by the velocity-distribution and by the additional condition that both pressure and potential temperature are constant at 12 km. The velocity-distribution was chosen in such a fashion as to give a constant potential temperature at sea-level.

The stream-lines of the transversal circulation are given by broken lines. It appears that three separate cells must develop--a center cell in which the circulation is counter-clockwise and two surrounding cells in which the circulation is clockwise. The boundaries between the regions of heat-loss and heat-gain (cold-sources and heat-sources) are given by thin, dotted, vertical lines. The analysis indicates the existence of a principal heat-source just south of the current-center, a principal cold-source just north thereof, a secondary cold-source far to the south and a secondary

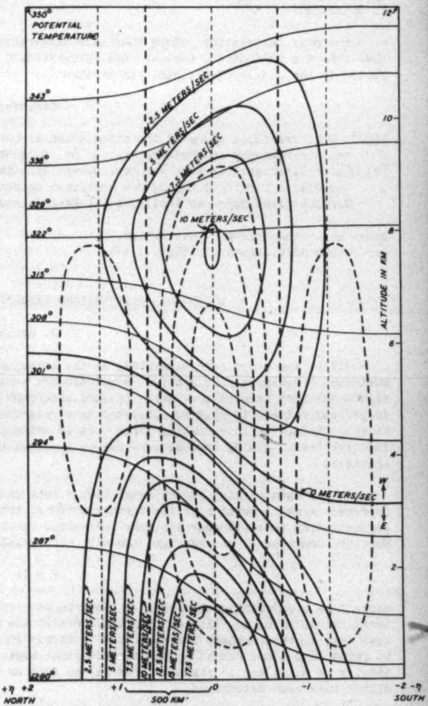

FIG. 2—STEADY SOLENOIDAL CIRCULATION REGULATED BY ISENTROPIC FRICTIONAL FORCES

heat-source far to the north.

It is evident that the circulation in the center cell is a direct one, that, it has the same sense as the circulation-acceleration due to the solenoids, while the two surrounding cells are circulating in reverse. Thus kinetic energy is all the time generated in the center cell, destroyed in the surrounding cells. Considering now the momentum, it is evident that the circulation in the center cell, with the aid of the Coriolis' force steadily produces momentum to the east above the "frontal" surface $\theta = 304°$, while momentum westward is produced below the front.

Through lateral diffusion the momentum thus generated is scattered both to the north and to the south. Since there are no lateral boundaries at which momentum can be destroyed, it follows that the surrounding cells of reverse circulation represent dynamically necessary brakes in the circulation-pattern outlined above.

This pattern is inadequate in several respects:

(1) The distribution of heat- and cold-sources is unreasonable. The most objectionable feature is the existence of a secondary heat-source far to the north. This heat-source is obviously a necessary element of the reverse circulation cell to the north. If the need for this cell is eliminated there is no need for the associated heat-source.

Following a suggestion from my colleague, Professor J. Holmboe, I have successfully tried to eliminate the northernmost cell and its heat-source by considering an annular, instead of a straight, current, thus simulating actual terrestrial conditions more closely. The momentum generated through solenoidal action in the center of the current will diffuse southward just as in the case of a straight current. Diffusion northward is no longer necessarily required since one may assume that the limited body of air inside the annular current now rotates as a solid next to the center of the plane. The assumption of solid rotation near the center is in good agreement with observations (compare Shaw, 1936, p. 279, Fig. 180). A complete discussion of this modified solution will be given in our final report on these investigations. At present it is sufficient to point out that the modified solution is characterized by widespread cold-sources everywhere except in the region of strong ascending motion just south of the main current. The secondary cold-source to the south is in qualitative agreement with the results obtained by Sverdrup [1917] in his study of the trade-wind circulation, which clearly indicates a widespread cold-source throughout the Azores High.

Möller [1935] concludes that there is a net loss of heat almost everywhere in the free atmosphere above the ground friction-layer, except in those regions where heat is realized through condensation of water-vapor. His result and the idealized picture presented above are qualitatively compatible.

(2) If the latent heat is the principal source of heat, one should expect the ascending branch of the transversal stream-lines to follow condensation-adiabatic lines instead of verticals, as in the solution here presented. This objection can doubtless be met through the device of a more suitable basic current-distribution.

(3) The assumption of a constant potential temperature at sea-level across the entire width of the current is incorrect. If one permits the isentropic lines to intersect the sea-level, one must introduce a new boundary-condition, stating that isentropic stresses and ground-stresses must balance each other. This condition implies a restriction in the choice of basic current-distribution which the author has not yet been able to cope with.

(4) The solution presented above does not account for the belt of westerlies reaching sea-level in middle latitudes. It is the author's opinion, that this west wind-belt, for which no adequate theory exists [compare Brunt, 1934, pp. 394-398] may be explained through the interaction of both lateral stresses and ground-stresses in the region to the south of the principal cell if direct solenoidal circulation (polar-front region). A calculation in support of this contention will be published in our final report. The present solution, which does not include the effect of ground-stresses, is incapable of explaining the westerlies.

Principal gain of the present analysis lies in the establishment of the necessity of a system of brakes south of the main cell of direct solenoidal action (the polar-front cell). These brakes are represented above in the form of a solenoidal circulation in reverse. It is evident, however, that these thermal brakes will be helped by ground-friction, which all the time tends to reduce the mass and (relative) momentum of the easterlies.

If the southern cold-source is inadequate for the full development of the thermal brakes, or if the direct solenoidal circulation in the center cell (polar-front region) has been unusually strong, more and more momentum eastward will accumulate in the upper portion of the troposphere in middle latitudes. The upper isentropic layers must then bank themselves to the south, in such a fashion that a trough of low pressure will be formed to the north, a ridge of high pressure to the south. Counter-currents must develop within the isentropic sheet, to the north and to the south of the main current. The nature of the mechanism of this adjustment process has been discussed in another place [Rossby, 1937, 1938].

The shear-zones between the current itself and the surrounding counter-currents are dynamically unstable, even when one allows for the stabilizing influence of the Coriolis' force [Pekeris, 1938]. One should therefore expect the current-system to break up in two rows of vortices--cyclonic on the northern side, anticyclonic on the southern side. It would seem reasonable to assume that the anticyclonic eddies so abundantly observed on our isentropic charts [Wexler and Namias, 1938] represent such a system of thermo-mechanical brakes.

It should finally be pointed out that the internal deformation of the isentropic surfaces associated with the banking referred to above necessarily must affect the distribution of heat- and cold-sources by changing the vertical temperature-distribution; the dynamic establishment of a high-pressure ridge to the south of the main current may be necessary in order to raise the mean temperature of this region so much as to produce an adequate cold-source.

The speed of the transversal circulation indicated in Figure 2 is proportional to the lateral eddy-viscosity coefficient. No attempt will be made here to discuss numerical values of this speed. It is sufficient to point out that calculations such as those of Grimminger [1938] are necessary before the problem of the general circulation can be attacked numerically by the method outlined in this paper. A value for the lateral eddy-viscosity coefficient of about 10^7 CGS units, which falls within the limits found by Grimminger, seems to give a reasonable rate for the transversal circulation analysed above.

The transversal circulation in Figure 2 is far from isentropic and would thus seem to rob isentropic analysis of its raison d'être. It is undoubtedly possible, and preferable, to develop a solution in which the angle between transversal stream-lines and isentropic lines is smaller in the regions of swift circulation. However, it must not be forgotten that some kind of a slow non-adiabatic transversal circulation is needed to feet energy into the general zonal circulation. The proponents of isentropic analysis do not claim that the air in a given isentropic sheet remains the same from week to week. It is claimed, however, that the succession of individual cyclonic and anticyclonic eddies is best followed from day to day by isentropic analysis.

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THE INTENSITY OF LATERAL MIXING IN THE ATMOSPHERE AS DETERMINED FROM ISENTROPIC CHARTS

G. Grimminger

(Abstract)

Since the inauguration of the analysis of isentropic charts at the Massachusetts Institute of Technology it has been found that certain well-defined properties are exhibited by these charts, the most important of which is the occurrence of well-marked currents of moist air which can be traced in their course across the whole United States and which have a characteristic distribution of specific humidity q. In the center of the moist currents, the specific humidity is a maximum where the current first appears on the chart and slowly decreases downstream while at the same time there is a gradual lateral spreading apart downstream of the q-lines normal to the axis of the current. This distribution of specific humidity is about what one might expect to find when there is a transport of moisture from a moist current to the surrounding drier atmosphere brought about by turbulent mixing, or diffusion. As Rossby has already pointed out [see 1 of "References" at end of abstract] and as can be seen from the daily isentropic charts, the currents of maximum moisture advance less rapidly than would be expected from the observed wind-distribution, thus indicating lateral, isentropic mixing with the environment. It thus appears that the observed distribution of specific humidity in well-marked currents on the isentropic chart must be largely the result of an isentropic mixing-process. Once this is recognized it is then possible to arrive at the intensity of the mixing expressed as a coefficient of eddy-diffusion.

If the distribution of q depends mainly on the process of diffusion then this must be governed by the equation of diffusion. It is shown how, under certain reasonable simplifying assumptions, the kinematic coefficient of eddy-diffusion can be evaluated from this equation in terms of quantities which can be obtained from the isentropic chart and from regularly available aerological data.

Values of the kinematic eddy-diffusion evaluated in this way are found to vary from 0.3×10^9 to $39 \times 10^9 \mathrm{cm}^2/\mathrm{sec}$ with most values falling in the range from 10^9 to 10^{10}; the mean of all values is 5.5×10^9. This is therefore the order of magnitude of the intensity of lateral mixing in the atmosphere on a scale comparable to that of the daily weather-processes, (that is, the scale of the isentropic chart). This value fits in nicely with values for the eddy-diffusion corresponding to scales of turbulence smaller and larger than that considered here. The values derived here are thus intermediate between the value of from 2×10^6 to 1.3×10^9 found by Richardson and Proctor [2] for diffusion over distances of 100 km and the value of 10^{11} found be Defant [3] by considering cyclones as turbulent elements in the general circulation.

The isentropic chart is ideally suited for the study of lateral mixing and the corresponding coefficient of eddy-diffusion is something which can be definitely and conveniently determined from such charts.

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WAVE-DISTRIBUTION IN A HOMOGENEOUS CURRENT

C. L. Pekeris

(Abstract)

In this paper an attempt is made to find a dynamic explanation for the large-scale horizontal mixing which is observed in the atmosphere and in the oceans. The primary mechanism in Rossby's theory of atmospheric and oceanic circulations is the injection of a jet into a resting environment and its subsequent modification by horizontal mixing. The question arises as to whether the observed turbulent state of such a stream is due to dynamic instability. In the case of a homogeneous current of infinite thickness which is bounded on each side by a resting fluid, Rayleigh found that horizontal disturbances of all wave-lengths are unstable. When the whole system rotates

about a vertical axis, it is found that, providing the thickness of the layer is infinite, all wave-lengths are still unstable, the effect of the rotation being merely the addition of a geostropic pressure-term. The effect of the finite thickness of the layers has been investigated in the case of two currents of opposite directions which are bounded by a vertical surface. It is found that for wave-lengths which are long in comparison with the thickness of the layers, the instability is decreased but not eliminated.

An examination of the stream-lines of these unstable disturbances shows a tendency for the formation of eddies having the vorticity of the original vertex-sheet at the boundary. Thus, in a westerly current there is a tendency for the formation of cyclonic eddies at the northern boundary and anticyclonic eddies at the southern boundary, and vice versa for an easterly current.

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MEAN MONTHLY ISENTROPIC CHARTS AND THEIR RELATION TO DEPARTURES OF SUMMER RAINFALL

H. Wexler and J. Namias

Reed in 1933 and again in 1937 [see 1 of "References" at end of paper] pointed out the relationship between the summer high-level anticyclone over the United States and rainfall- and temperature-departures. His charts were constructed for fixed levels and were based on winds only. Last summer and fall the authors constructed mean monthly charts on isentropic surfaces which give a much better picture of flow-pattern than fixed-level charts and show the dry and moist tongues. This form of analysis allows a much better correlation of rainfall-patterns with upper-air phenomena than was possible in Reed's study and also throws new light on the maintenance of these anticyclonic eddies.

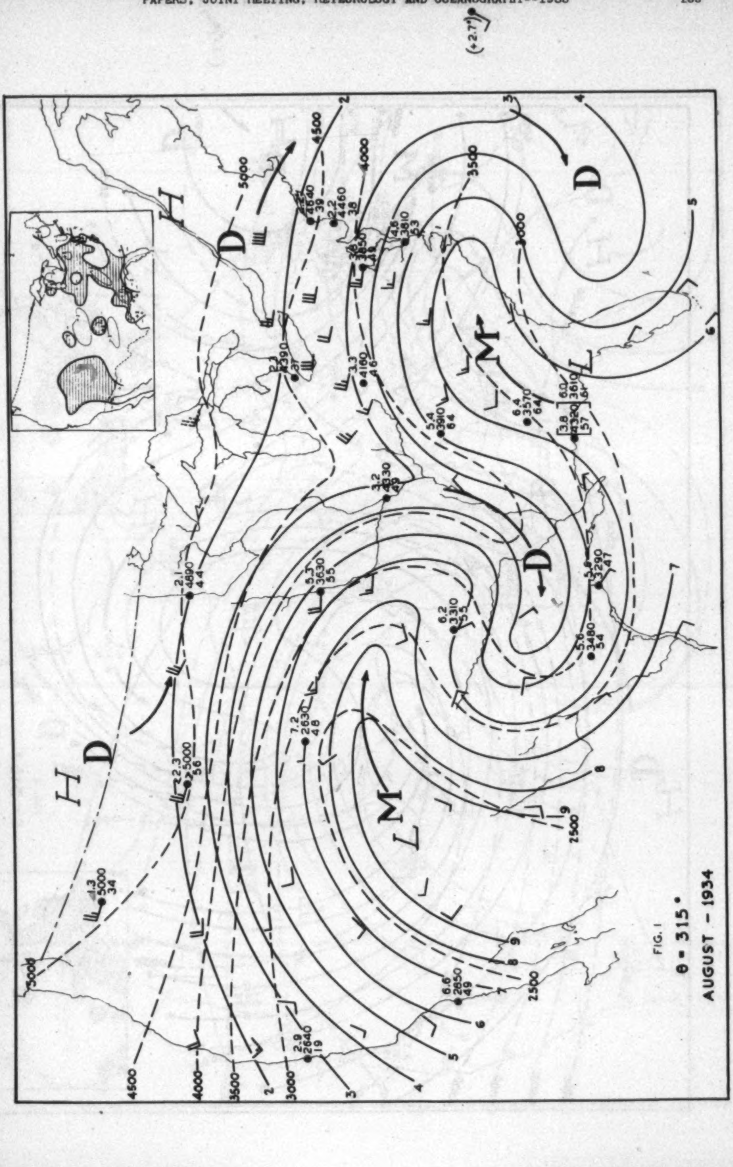
The monthly mean isentropic charts (Figs. 1 to 4) are drawn from the data published in the Monthly Weather Review for August, 1934, 1935, 1936, 1937, and the rainfall-departure charts in the upper right-hand corner are reproduced from those published in the Review.

Figure 1, the mean isentropic chart for August, 1934, shows two well-developed anticyclonic eddies, one centered over Oklahoma, and the other over the southeastern States. The eastern eddy is associated with a large rainfall-excess to the left of the moist tongue shown over the east Gulf states and a rainfall-deficit over Florida. A strong flow of cool dry air prevailing over the New England states is associated with a two-inch deficit of rainfall and a negative departure of 2°F in temperature. This same dry tongue is evidently the same as that found coming in from the east over Florida. However, Bermuda has a 2.7-inch rainfall-excess, is slightly warmer than normal, and has a mean resultant wind from the southwest, probably showing the existence of another anticyclonic eddy to the east, the moist tongue of which passes near Bermuda.

It is more difficult to explain from the chart the rainfall-departures in the Midwest, except the deficit in Texas.

To show a good contrast for the 1934 chart, especially for New England, the chart for August, 1937, is shown in Figure 2. In this month practically the whole Atlantic seaboard has excess rainfall--as much as four inches in some localities. The temperature is also above normal by as much as 4° in New England. These phenomena are associated with a marked moist tongue coming up the Appalachian Mountains from the south and southeast. For example, Boston and New York are warmer and moister than Washington and Norfolk at this surface. Bermuda has more than a two-inch deficit of rainfall and the mean resultant wind is from the southeast, while the temperature-departure is over +4°F, showing that Bermuda has a warm dry August which was associated with a dry tongue coming from the north and curving anticyclonically, so that it appeared over Bermuda from the southeast. (This interpretation was made even before the wind- and rain-data from Bermuda were known). This eastern cell is much larger than the one in 1934 and the winds in New England are much weaker. The western cell is quite similar to that in 1934 and is associated with practically normal rainfall in the west.

The chart for August 1935 (Fig. 3) is roughly similar to that for 1934 even in the rainfall-departures. The moist tongue entering over west Florida was associated with rainfall-departures of more than ten inches. Excess rainfall fell over Ohio, western Pennsylvania, West Virginia, and eastern Kentucky as it did in 1934. But southern New England has a two-inch deficit. Bermuda has a large excess of rainfall and a southwest wind which may be associated with a third cell to the east. In the midwest the region of two-inch deficit is well correlated with a dry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2.1 4890 44
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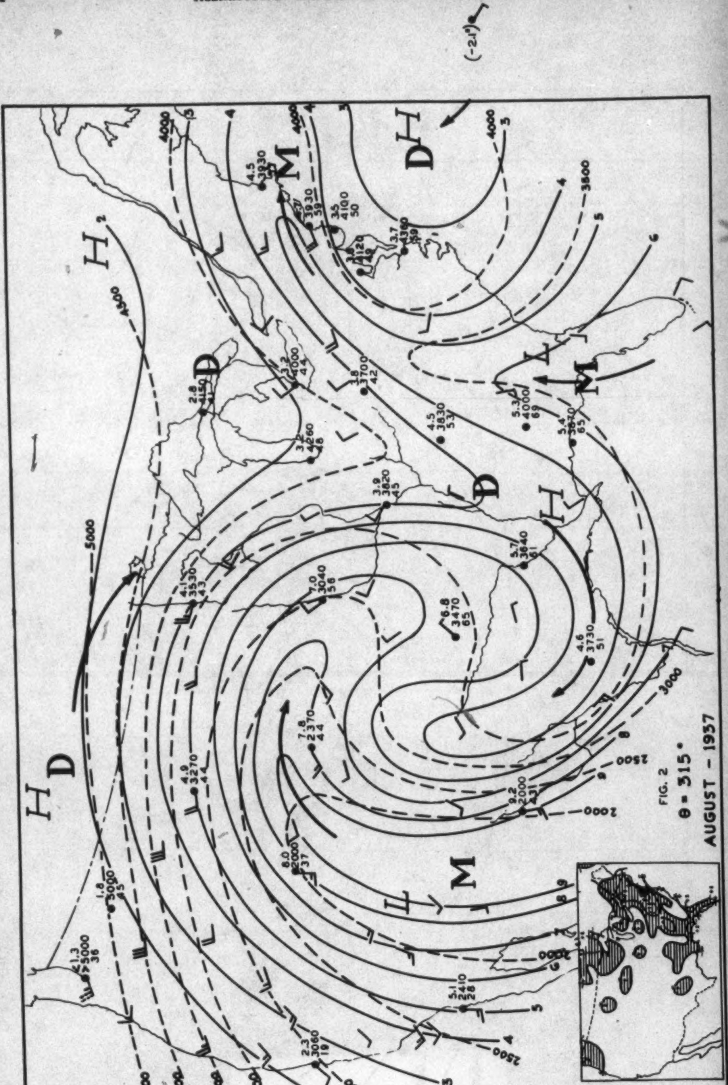
4.3 5000 34
2.9 2640 19
6.6 2850 49
7.2 2630 48
5.3 3630 55
3.2 4330 49
6.2 3310 55
5.8 3290 47
5.6 3480 54
3.8 4320 57
6.0 3610 61
6.4 3570 64
5.4 3910 64
3.3 4160 46
2.3 4390 37
3.8 3850 49
4.6 3810 53
2.2 4460 38
4640 39

FIG. 1

θ = 315°

AUGUST - 1934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1.8 3090 45
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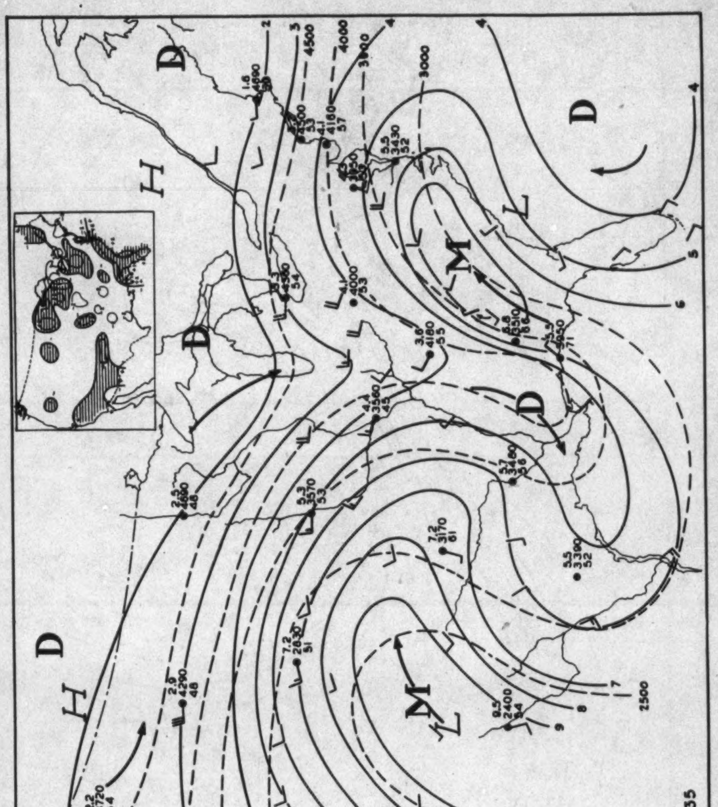
5.1 2410 28
8.0 2000 37
4.9 3270 44
7.8 2370 44
9.2 2000 31
4.1 3530 43
7.0 3040 56
6.8 3470 65
4.6 3730 51
5.7 3640 61
3.9 3820 45
3.2 4260 48
2.8 4150 41
3.2 4000 44
3.8 3700 42
4.5 3830 53
5.3 4000 69
5.4 3870 65
4.5 3930
3.7 3930 59
3.5 4100 50
3.7 4360 59
3.3 4120 49

FIG. 2
θ = 315°
AUGUST - 1937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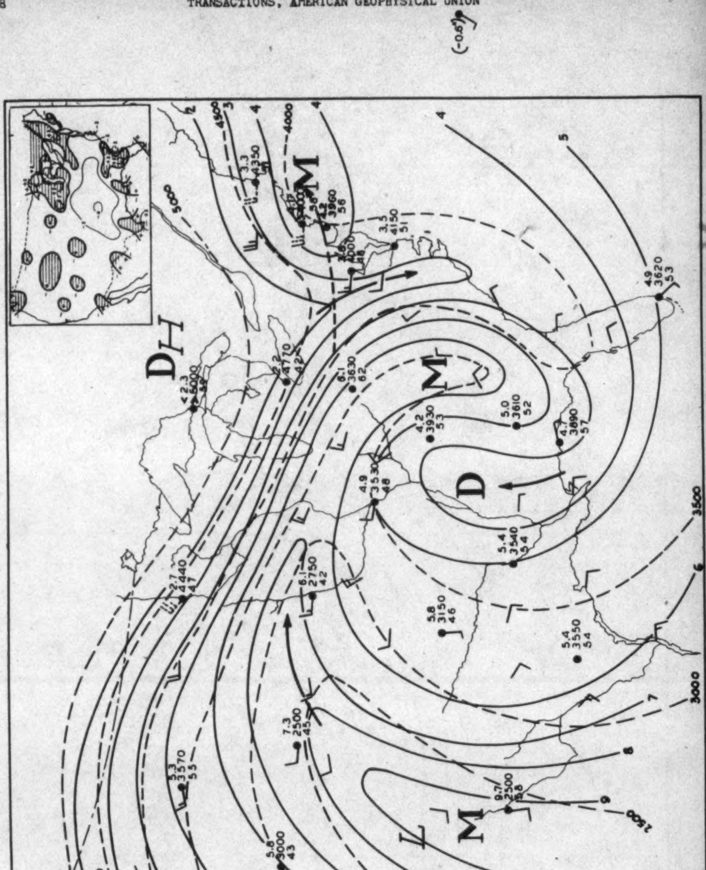
4000
4500
1.8 4690 38
4.5 4500 53
4.1 4160 57
5.5 3430 52
5.3 4380 54
4.1 4000 53
3.6 4180 55
5.8 3510 56
5.9 3650 71
4.4 3560 45
2.5 4680 48
5.3 3570 53
5.7 3460 56
7.2 3170 61
5.5 3390 52
7.2 2830 51
9.5 2400 54
2.9 4290 48
2.2 4720 44
2.2 4390 37
3.4 3000 29
5.6 3220 47

FIG. 3

θ - 315°

AUGUST - 1935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3.3 4350
4.2 3960 56
3.5 4150 51
<2.3 5000
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2.1 4440 41
6.1 3630 62
4.2 3930 53
5.0 3610 52
4.7 3690 57
4.9 3620 53
4.9 3530 48
5.4 3540 54
6.1 2750 42
5.8 3150 46
5.4 3550 54
5.3 3570 55
7.3 2500 45
9.7 2500 58
5.8 3000 43
5.9 3000 47

FIG. 4

θ = 315°

AUGUST - 1936

tongue coming down east of the Mississippi Valley, while the southwestern States have an excess of rain. The large area of excess rain west of the Great Lakes is not readily explicable but may be due to upglide motion to the left of the moist tongue. It may be pointed out now that these regions of rainfall-departure occurring to the north in the westerlies are not so readily correlated with flow-patterns as those to the south where the anticyclonic eddies are located. The rainfall in the latter case is mostly from persistent showers which according to Namias [2] are very definitely related to the presence of deep currents of moist air, while in the former case the rainfall is mostly frontal precipitation, which may fall in a few days and thus not be associated with a mean flow-pattern based on a month's observations.

The last chart (Fig. 4) is that for August 1936, when the severe drought occurred over the midwest. The flow-pattern on this chart differs radically from the others in that only one very large cell seems to exist. The moist tongue enters the United States from Mexico and turns to the east but not so rapidly as in the other charts, so that it does not turn southward until it reaches the Appalachian Mountains. However, the high moisture-contents at New York and Lakehurst seem to indicate that at certain times during the month the moist current continued flowing due east. Most of the month, however, the moist tongue turned southward and was undoubtedly associated with the excess rainfall over central and western Georgia and northern Florida. The large region of excess rainfall over the Great Lakes occurs to the left of the moist current as should be expected. A dry current is seen coming down from the eastern Great Lakes Region, leaving the Continent in North Carolina, and is undoubtedly associated with the rainfall-deficit along the southeastern coast. After passing over southern Florida it enters the continent from the south over the lower Mississippi Valley. Since the trajectory of this dry tongue is mostly over the relatively cooler sea-surface its moisture-content is not appreciably increased by convection and so it enters the southern midwestern region as a dry warm current, warm because of the gradual subsidence as it travels southwards.

Now a few general remarks can be made about these charts. First these eddies are difficult to explain as purely thermal phenomena since they are regions of convergence rather than divergence as one should expect from the classical thermal explanation. Also, it does not seem likely that they are caused by motion around solenoids since a closed circuit in an isentropic surface contains no solenoids. However, one might argue that the motion is due to circulation about a vertical circuit, the top of which is found in this surface. But as is seen from the topographical lines, the slope of the isentropic surfaces is so small that the number of solenoids would also be small. However, to the north where the slope of the surface is large, the number of solenoids is also large and the strong westerlies are created; this probably is the driving force for these eddies, the momentum being transported southward by the lateral frictional forces, as suggested by Professor Rossby [3].

Because of lack of data this analysis cannot now be extended to other regions, but it would appear from Professor Rossby's investigation that such anticyclonic eddies necessarily must exist to the south of the westerlies around the Earth. From the charts we have examined so far there seems to be a certain stability in position of the main moist and dry tongues of these eddies. The main moist tongue as we have seen on all four charts, enters the country from Mexico, which is evidently a normal state of affairs for summer. Originally it is brought to Mexico by the subtropical easterlies, but on climbing the high Mexican Plateau convection ensues which brings warmth and moisture to high levels. This is borne out by the fact that the rainy season for central and eastern Mexico is in summer [4] and that the rainfall-maximum in southwestern Texas, New Mexico, and eastern Arizona is also in the summer [5]. Also, level for level, the mean temperatures for August 1937 at El Paso (32° north, elevation 1194 meters) are higher than those at St. Thomas, Virgin Islands (18° north) and Coco Solo, Canal Zone (9° north) and this is also true for the specific humidities above 2500 meters. For some unknown reason, at the Plateau the deep moist current is deflected northward, then eastward, and finally southward over the United States. With the exception of 1936 a second cell was present to the east with a somewhat weaker moist tongue entering the United States between Florida and the Mississippi River. That this is probably its normal position for the summer is indicated by the extreme maximum summer rainfall and thunderstorms experienced at Tampa (Florida) and by smaller maxima in eastern Alabama, Georgia, South Carolina, and eastern North Carolina and Virginia [5]. This moist tongue may be due to the convective influence of the West Indian Islands and the Florida Peninsula on the easterlies, which should of course be much less than that of the high Mexican Plateau. The dry tongues enter the country mostly from the eastern Great Lakes Region. Just what is the explanation of these favorite places of entry of moist and dry tongues is still unknown. Why, for example, do we not observe moist tongues in the mean coming up the Mississippi Valley? (This query applies only to the high-level moist currents; of course in summer considerable moisture is transported up the Mississippi Valley in the frictional layer and this must contribute generously to the rainfall. However, according to Namias [2], showers are not likely to occur unless a high-level moist current displaces a good portion of the dry air above the frictional layer.) Do the topographical features not only warm and moisten the easterlies by convection but also tend to deflect them northward? If so, why in August 1936 was the eastern cell missing? Do the cool waters of the Great Lakes and perhaps Hudson Bay act as dry-air reservoirs? We hope to study these problems in the future.

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ON THE THERMODYNAMIC INTERPRETATION OF ISENTROPIC CHARTS

Horace R. Byers

(Abstract)

For the thermodynamic interpretation of isentropic charts it is preferable to use isobars in place of elevation-contours and isentropic-condensation pressures in place of specific humidities. This produces the same shape of flow-pattern as the other type, since pressure and altitude are closely related, and along an isentropic surface there is only one value of the condensation-pressure for each value of the specific humidity.

The ratio of the condensation-pressure to the actual pressure, which the author calls the "condensation-ratio" is shown to be analogous to the relative humidity, but preferable for isentropic considerations because it is the only measure of the nearness to saturation which has a direct thermodynamic significance in an adiabatic process.

Some of the advantages of the pressure and condensation-pressure type of chart are that it shows graphically the regions of saturation, it gives better representation of moisture-differences at low values and it facilitates measures of the stability of the air.

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U. S. Weather Bureau,
Washington, D. C.

ON THE USE OF ISENTROPIC CROSS-SECTIONS

C. H. Pierce

(Abstract)

Isentropic cross-sections drawn with isotherms of potential temperature and isotherms of of isentropic-condensation temperature serve as a very helpful link between the synoptic map and the isentropic chart. Isotherms of potential temperature are of course isentropic surfaces. Isentropic-condensation temperature is defined as that temperature a particle of air would have if it were raised adiabatically to the condensation-level. On any one isentropic surface this value is a direct function of specific humidity, and therefore shows the moisture-content of the air. In constructing cross-sections, however, we deal with many isentropic surfaces instead of one, so that for every condensation-termperature, the specific humdity is different for every level or pressure. This variation is about ten per cent and does not change the picture of the flow-pattern of the cross-section appreciably.

One of the advantages of using condensation-temperature is that the nearness to saturation can be determined by using it with the potential temperature. Since air cools approximately 1° C per 100 meters of ascent in an adiabatic process, the height of the condensation-level above 1000 mb can be determined by subtracting the isentropic-condensation temperature from the potential temperature at any point on the cross-section and multiplying the difference by 100. The level of condensation above sea-level can be determined by adding the height of the 1000 mb above sea-level (found roughly from the surface-map) to the original height. Another advantage of the use of isentropic-condensation temperature is that it gives a representative difference in moisture-content in cold air, and the isograms of moisture are not as crowded in the tropical maritime air.

Another use of the isentropic cross-section is that it shows the slopes of several isentropic surfaces along a vertical plane. It also depicts the layers of moist and dry air, which is important to forecasting both in winter and summer. Conditional and convective stability are roughly indicated by the cross-section.

The position of fronts and their changes is shown in the cross-section. Fronts generally follow isentropic surfaces with an allowance of two or three degrees for personal and instrumental error. Cold fronts below one km cross isentropic surfaces at a point of greatest curvature of the isentropic surfaces because of radiational cooling near the ground during the night. Warm fronts may entirely disappear below 800 or 1000 meters for the same reason, but are definitely found aloft. If fronts are placed along isentropic surfaces some difficulty is experienced in constructing occluded fronts and fronts aloft, but it seems that after occlusion takes place the original cold and warm front would be maintained for a while as a wave aloft. When the occlusion is new tne front might pass through the isentropic surfaces at their greatest curvature, but due to rapid horizontal mixing in this stable layer, the front would soon dissipate, and a new front would appear on the first isentropic surface that cuts the ground. A front aloft would be located at the top of an inversion below which the isentropic surfaces suddenly change slope.

[This paper is to be published in full in the Monthly Weather Review.]

U. S. Weather Bureau,
Washington, D. C.

ANALYSIS OF THE FLOW-PATTERNS ON TWO SURFACES OF CONSTANT POTENTIAL DENSITY IN THE SOUTHERN NORTH ATLANTIC

R. B. Montgomery

(Abstract)

The distributions of salinity- and oxygen-content on two surfaces of approximately constant potential density (actually for tne surfaces of $\sigma_t = 27$ and $\sigma_t = 26$) have been studied for the zone of the Atlantic Ocean between the equator and 30° north. It appears that flow normal to these surfaces and vertical mixing are of secondary importance compared with lateral mixing. It is found that all salinities on each of these surfaces lie within the range of salinities found at its intersections with the sea-surface in high northern and southern latitudes, and it appears that the major sources for the water on each surface are along its intersections. From the apparent advance of salt or relatively fresh, of O_2-rich or O_2-poor, tongues along these surfaces, certain features of the flow-patterns have been deduced.

This paper is comprised of excerpts from the more extensive "Isentropic analysis of the upper layers of the southern North Atlantic Ocean" [Papers Phys. Oceanogr. Met., v. 6, No. 1, June 1938].

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ON THE RELATIVE MERITS OF DYNAMIC TOPOGRAPHIES AND ISOPYCNIC DISTRIBUTIONS FOR THE STUDY OF OCEAN-CURRENT TRAJECTORIES

A. E. Parr

(Abstract)

A review of the objections to the use of dynamic topographies for the study of actual current-trajectories on the assumption that the lines of flow should run parallel with the dynamic contour-lines. The errors developing from this assumption when the topographic method is applied in the presence (1) of true convergence with significant components of motion in the vertical, (2) of changes in horizontal centrifugal forces, (3) of internal contraction or expansion of the moving liquid itself, and (4) of actual acceleration or retardation of motion whether indicated by the dynamic topography or not, were discussed and demonstrated by actual examples taken from available oceanographic records.

An hypothesis of the effects of general wind-acceleration in excess of retardation by friction in low latitudes and general retardation by friction in excess of acceleration in high latitudes is offered in explanation of the apparently normal distortion between dynamic contour-lines (closed curves of greater width in the north-south direction) and true trajectories (curves of narrower width in the north-south direction).

[The full report is published under the title "On the validity of the dynamic topographic method for the determination of ocean-current trajectories," Sears Foundation, J. Marine Res. v. 1, pp. 119-132 (1938).]

The superior merits of isopycnic analyses of the distribution of identifying properties were discussed, with a suggestion for the use of relative values determined with reference to maximum and minumum values in each isopycnic surface separately for the integration of two-dimensional transverse profiles and three-dimensional presentations of overlapping current-systems.

[The full report is published under the title "Isopycnic analysis of current-flow by means of identifying properties," Sears Foundation, J. Marine Res. v. 1, pp. 133-154 (1938).]

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CHARACTERISTICS OF THE LITTORAL WATER AND WEATHER ALONG THE PACIFIC COAST OF COLOMBIA AND ECUADOR

Robert Cushman Murphy

(Abstract)

During September and October, 1937, the author made a reconnaissance by launch from Panama to Guayaquil, along a coast from which meteorological and oceanographic data are extremely scanty. Records were made at a total of 51 stations, mostly within 25 miles of shore but extending to a maximum distance of ±70 miles. Photographic and diagrammatic information were presented relating to the rainfall- and cloud-patterns of the region, and other matters.

The northerly part of this coast is one of the world's centers of maximum precipitation and is also one of the few areas in which rainfall is determined largely by the relationship between mountain ranges and winds from the sea. The prevailing wind-systems blow onshore, with a southerly component during the greater part of the year and a northerly component during the remainder. Along the part of the coast known as the Colombian Chocó, both systems are rain-bearing because both carry air from warm oceanic districts toward land which is perpetually cooler than the ocean-water, at least at night. The southerly part of the region exhibits a remarkably sharp climatic transition into the seasonally arid zone of western Ecuador and the perennial aridity of coastal Peru.

The coastal water-circulation presents strong contrasts with that of the Humboldt Current Region to southward. The latter is in the main characterized by the divergence of surface-water from the Continent, whereas evidence of both hydrographic and biological nature combine to indicate that the current-movement between Ecuador and northern Colombia is convergent throughout

the year, with upwelling reduced to a minimum everywhere south of the Gulf of Panama.

The air-water anomaly along the coast is positive, thus differing markedly from regions to southward and westward. The September mean temperature for 44 observations of the sea-surface between the Gulf of Panama and Cape Santa Elena was 26°96 C; that for atmospheric readings at the same stations, 25°24 C. To southward of Santa Elena this ratio became rapidly inverted toward the marked negative anomaly of the Humboldt Current Region.

Coastwise salinities of surface-water, as determined by titration, reveal several facts of interest. To northward of Cape Corrientes, Colombia, the littoral ranges in September proved above 27 o/oo, despite extraordinarily heavy tropical precipitation. This was interpreted as due to the fact that the continental watershed is here close to the Pacific Coast, for which reason the greater part of the runoff reaches the Caribbean by way of the Atrato River. South of Cape Corrientes, where many rivers of large volume enter the Pacific along the length of the Chocó, the surface-salinities dropped below 21.4 o/oo, and even down to 14.16 o/oo. The season, it should be noted, was close to the annual precipitation-peak of October.

A very rapid offshore rise in salinities to values above 31 o/oo, in the clear indigo water that lies within about 30 miles of the coast, offered striking evidence of the convergence of the coastal current. To southward of the rain-limit, at Cape San Francisco, Ecuador, littoral salinities increased abruptly to values above 33 o/oo, and continued to rise steadily toward the Peruvian Coast.

[The complete paper is to be published in the Geographical Review.]

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OCEANOGRAPHIC PROBLEMS OF THE COAST OF CALIFORNIA

H. U. Sverdrup

(Abstract)

In another communication--"Research within physical oceanography and submarine geology at the Scripps Institution of Oceanography during April 1937 to April 1938"--at the 1938 meeting of the American Geological Union, it was pointed out that off the coast of Southern California we find in spring and early summer two types of surface-water, namely, up-welled water and offshore surface-water--both types of water are flowing towards southeast parallel to the coast (see Figs. 1 and 4 of the quoted paper). At a depth of 200 meters the water at some distance from the coast flows towards southeast, but inside of this current a narrow counter-current is running in the opposite direction (see Fig. 5 of the quoted paper). The in-shore counter-current carries a type of water which is characteristically different from the waters of the current offshore.

An intensive mixing of the various types of water takes place both within surface-layers and within sub-surface layers. The mixing within surface-layers can be described by means of a coefficient of diffusion which has a numerical value between 10^6 and 10^7 (in CGS units), and the mixing within the sub-surface layers, which is assumed to take place along isopycnal surfaces, can be described by means of a coefficient of diffusion which is about 10^6. It is probable that the mixing is due to eddies with vertical axes having dimensions up to 20 km. Recent work by C.-G. Rossby and his collaborators has shown that in the atmosphere a large-scale isopycnal mixing takes place through the action of eddies which have horizontal dimensions up to 20 or 30 km. In the case of the atmosphere, we are undoubtedly dealing with a process which is characteristic of the motion of the atmosphere, but when dealing with the ocean we may ask if the corresponding eddy-motion in the ocean represents an inherent characteristic of the ocean-currents or if it is a secondary phenomenon which is enforced upon the motion of the ocean by the atmosphere.

The latter alternative cannot be ruled out or accepted before we know more about the dimensions of eddies in the atmosphere and the ocean. If these are the same, much can be said in favor of the conception that the eddy-motion is enforced upon the ocean. It is evident that if eddies exist in the atmosphere convergences and divergences of the wind must occur, and these must lead to similar convergences and divergences of the wind-currents and to descending and ascending motion. Where we have convergence and descending motion we must obtain an accumulation of the light surface-water; where we have divergence and ascending motion the heavier sub-surface water must rise. The distribution of mass must therefore be altered down to the depth to which the convergences and divergences affect the conditions, and thus the eddies which are created in the surface-layers by the stress of the wind will be transmitted to much greater depths by the altered distribution of mass.

Further studies of the waters off the coast of Southern California are in progress, and it is hoped that these will throw light over the questions which have been raised.

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THE FORECASTING SIGNIFICANCE OF ANTICYCLONIC EDDIES ON THE ISENTROPIC CHART

Jerome Namias

The fundamentals of isentropic analysis of flow-patterns within the atmosphere were set forth last year in a paper presented before the meteorological section of the American Geophysical Union by C.-G. Rossby [see 1 of "References" at end of paper]. Since that time a considerable amount of research has been directed to the application of these charts for use in helping to explain certain weather-phenomena and to develop new tools for use in weather-forecasting. While the technique of constructing these analyses has been appreciably improved within the past year, the underlying procedure and suggestions made by Rossby have been found to be substantially most effective, and for this reason they are being carried out at the Massachusetts Institute of Technology.

One of the interesting phenomena which the isentropic chart has brought to light is the frequent formation of anticyclonic eddies. Indeed this peculiar velocity-distribution prevails so often, especially during the summer, that it shows up even in the mean monthly isentropic charts [2]. In the first series of charts analysed with the help of isentropic methods, Rossby [3] was able to show a "pronounced tendency of the current-systems to break up into large anticyclonic eddies." It so happens that this first weather-situation chosen for study was an exceptionally steady one, there being little change in the general upper-air flow for several days. Thus the flow-patterns were practically directed along stream-lines. More recent studies indicate that such steady conditions aloft are rare, yet is surprising to note that this fundamental flow-pattern is the most frequent one observed on the daily isentropic charts. The dimensions of the anticyclonic eddy of this situation were comparatively large. It is with the smaller eddies that this report is concerned.

The anticylonic eddies of the isentropic chart, at least in the warmer half of the year, are apparently always formed within the region of wind-shear between polar currents with a good northerly component and, to the west, returning southerly air which in its trajectory has been warmed and moistened through insolation and convective activity. The shear-zone shows up on the isentropic chart as a region of steep moisture-gradient and to a lesser extent, as one of rapid change of elevation of the isentropic surface.

The anticyclonic eddy in itself would probably not be of much meteorological significance were it not for the fact that it brings about a striking change in the distribution of moisture. For this reason it is generally possible to locate the eddies on the isentropic chart by means of the distribution of specific humidity. It frequently happens, however, that an eddy serves to create a new moisture-distribution and later on dies out as a circulatory motion in the wind-pattern on the isentropic surface. In this case the lines of constant specific humidity may suggest an eddy which is not borne out by the winds and which, therefore, is a result of previous history and is "fictitious" as an eddy. The important question then arises as to what are the determining factors in the life of an anticyclonic eddy.

In order to obtain a clue to this problem I have plotted in Figure 1 the wind-velocities observed around a well-marked anticyclonic eddy which appeared on the isentropic charts from June 23 to 27, 1937. The center of the eddy was easily fixed on the individual isentropic charts, and these centers are represented in Figure 1 as the centers of concentric circles, the distance between two successive circles being 100 miles.

On June 27 the circulation has broken up so that at this stage it hardly can be called an anticyclonic eddy. This disintegration can be observed setting in on June 26 where the symmetry of velocity is not as pronounced as in the first three days. Indeed, it appears from these individual eddy-profiles that the life of the eddy is closely tied up with the distribution of velocity about its center. Thus in the embryonic and developing stages of its history, from June 23 to 25, the velocities radially outward from the center show appreciable increase, while in the dying stages there is a pronounced weakening of the radial gradient of velocity. Even before these eddy-profiles were plotted, H. Wexler called my attention to the fact that such eddies should tend to remain stable as long as they rotate as solids. If the angular velocity increases radially outward, the eddy has a stronger tendency to persist than if the angular velocity decreases outward. In synoptic practice, this criterion has been found helpful.

The centers of anticyclonic eddies are not fixed in location but appear to migrate. There seems to be no association of the direction of migration with any of the features of the constant-level charts or surface-weather maps. There appears, however, to be a striking association of the movement with the velocity-distribution about the eddy itself. In Figure 1 arrows indicate the movement of the center of the eddy with respect to its own center in the succeeding 24 hours. From June 23 to 24, for example, the center has moved from O to O'. In all these cases, except that for June 24 to 25 where the movement is small and ill-defined, it appears that the direction of movement of the center of the anticyclonic eddy is along the region of lightest tangential winds or where the radial gradient of tangential velocity is a minimum. This rule has been applied to other well-defined eddies and has been found to be amazingly effective. Moreover, although there is less evidence to bear this out, it seems that the displacement is greater as this lack of symmetry in the velocity-distribution becomes more pronounced.

The conclusions of this preliminary report may be summed up as follows:

(1) The fundamental importance of the isentropic chart in synoptic meteorology is to offer a hydrodynamical picture of the flow-patterns of the free atmosphere, and to portray the dynamic interaction largely brought about through lateral shearing stresses.

(2) Rossby's earlier conclusion that current-systems tend to break up into large anticyclonic eddies is true. Moreover, there appear to be frequent cases of smaller anticyclonic eddies.

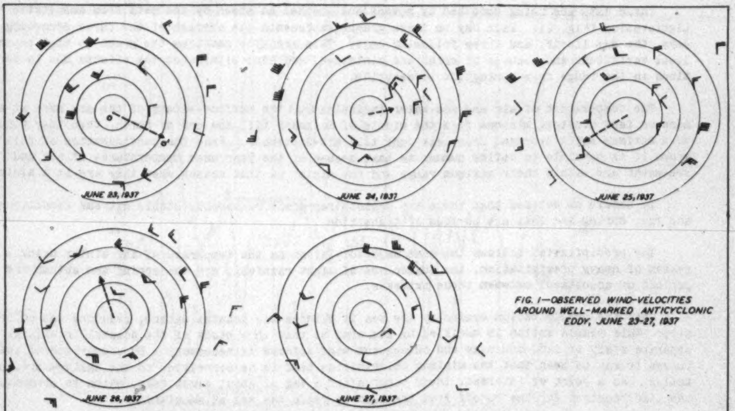
(3) These smaller anticyclonic eddies are formed within the region of wind-shear between a polar current with a strong northerly component, and the current on its right from the south.

(4) The wind-shift zone in which these eddies are formed tilts upward toward the west. Thus the axis of anticyclonic eddies, at least in their early stages, is tilted westward with elevation.

(5) The major importance of these eddies for the weather lies in their ability to bring about a redistribution of moisture in the free atmosphere.

(6) The life and stability of an anticyclonic eddy are associated with the distribution of angular velocity about its center. It is stable only when the angular velocity remains constant or increases outward from the center.

(7) The movement of the centers of these eddies appears to be directed along the region



JUNE 23, 1937 JUNE 24, 1937 JUNE 25, 1937

JUNE 26, 1937 JUNE 27, 1937

FIG. 1—OBSERVED WIND-VELOCITIES AROUND WELL-MARKED ANTICYCLONIC EDDY, JUNE 23-27, 1937

where the radial gradient of tangential velocity is a minimum, or, in other words, through the "weakest" quadrant.

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SOME RELATIONS BETWEEN METEOROLOGY AND COAST GRADIENT-CURRENTS OFF THE PACIFIC COAST OF NORTH AMERICA

John P. Tully

An extensive investigation is being made of the ocean-currents off the Pacific Coast of North America north of the Columbia River, in which seasonal observations of the gradient-currents in the sea and continuous observations of the meteorology of the coast are being made concurrently. The influence of the variations in the latter upon the former are significant, and their general effect will be briefly indicated by consideration of two of the seasonal conditions existing during 1936 off the west coast of Vancouver Island, Canada.

There are three factors affecting the movements of the ocean-waters, namely, the tides, wind-currents, and gradient-currents, each of which has a primary independent variation, and secondary interrelationships. In this progress-report it is proposed to limit the discussion to the simple relationships between meteorology of the coast and the vectors of the gradient-currents parallel to the coast of Vancouver Island.

Meteorological data

By arrangement with the Meteorological and Lighthouse services of Canada, daily observations of the atmospheric temperatures, precipitation, cloudiness, fog, direction and velocity of wind, and the temperature and chlorinity of sea-water at one-metre depth on the seaward coast are being made at twelve lightstations along the Pacific Coast of Canada.

These data are being smoothed by sevens and graphed as shown by the data from Amphitrite Lightstation (Fig. 2). Each day on these graphs represents the average of the three preceding days, the day itself, and three following days. This artifice smoothes the graph so that extreme local variations and chances of error are minimized, and also eliminates the effects due to variations in the range from springtide to neaptide.

The temperature of air and sea-water indicates that the surface-waters of the sea were at a more or less constant minimum from the middle of November till the end of March, then they rose to a maximum which extended from late June till mid-September. From the configuration of this graph it is possible to define summer as that season of the year when temperatures of air and sea-water are within their maximum range and the winter as that season when they are at a minimum.

It should be noticed that these two seasons represent reasonably stable extreme conditions, and that spring and fall are periods of transition.

The precipitation follows the same major divisions as the temperature, the winter being a season of heavy precipitation, the summer one of light rainfall, and the spring and autumn are periods of adjustment between these extremes.

When the precipitation drains to the sea it dilutes the coastal waters, reducing the chlorinity. This simple action is modified by the wind so that in a study of the coastal chlorinity a separate study of land-drainage and subsequent wind effects is necessary. By comparison of the curves it may be seen that the minimum chlorinities tend to be correlated to the maximum precipitation. As a point of interest, these occur after a lag of about eight days, which is presumably the time required for the runoff from the land to reach the sea at Amphitrite.

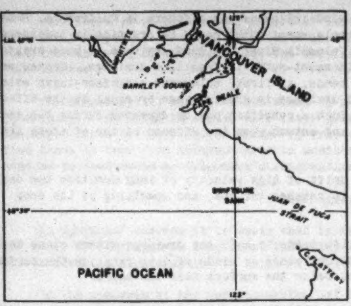
VANCOUVER ISLAND
BARKLEY SOUND
CAPE BEALE
SWIFTSURE BANK
JUAN DE FUCA STRAIT
C. FLATTERY
PACIFIC OCEAN
48°30'
125°
123°

FIG. 1—CHART TO SHOW LOCATION OF AREA DISCUSSED ►

In the winter the precipitation on the land remains as snow and does not affect coastal chlorinity until early summer when it melts and drains to the sea. This is clearly indicated in Figure 2 where the heavy precipitation in winter does not affect the chlorinity as much as its volume would indicate as probable, while in the early summer there is a greater lowering of chlorinity than could be attributed to rainfall.

From these summaries it is evident that the variations in temperature and chlorinity are seasonal, particularly the temperature. The coastal chlorinity varies inversely as the land-drainage, which varies directly as the precipitation in the coastal drainage-area and the atmospheric temperature.

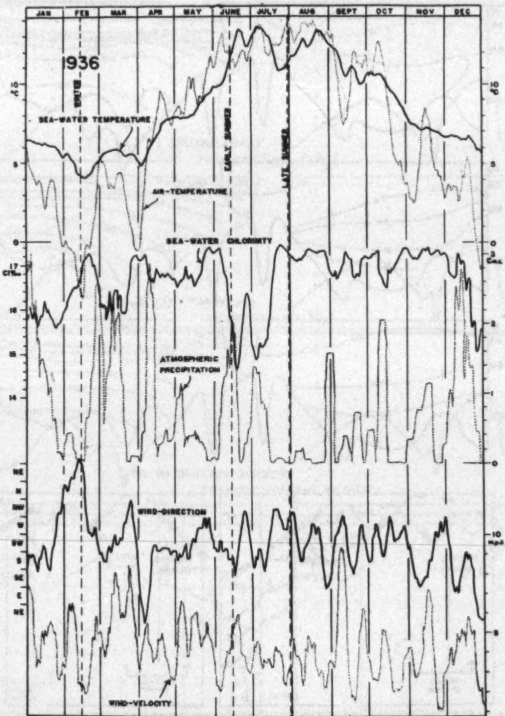

FIG. 2—METEOROLOGICAL DATA AT AMPHITRITE STATION, 1936, AS SEVEN-DAY RUNNING AVERAGES (VERTICAL BROKEN LINES SHOW THE TIME OF OBSERVATIONS)

When the transport of surface-water due to wind influence was offshore in California, McEwen [1912] evidently considered that the influx of replacement-water could be practically considered as coming wholly from the depths. North of the Columbia River the land-drainage is much greater than in the south, and at least part of the replacement-water is from land-drainage. Consequently a wind which develops an offshore drift-current tends, at first, to create a surface-layer which is laterally homogeneous. If the volume of land-drainage is greater than or equal to the offshore water-transport, the upwelling will not occur. Such a condition may be expected during the seasons of variable wind-direction, such as spring and autumn when the effects of the offshore transport are nullified by an onshore drift.

In the limiting condition with an offshore drift of high velocity or long duration the drainage-water is carried offshore more rapidly than it reaches the sea, and upwelling of the deep ocean-water develops as a secondary phenomenon.

A wind which develops an onshore surface-drift tends to hold the drainage-waters close to the coast and so preserves their identity. During strong winds or winds of long duration the surface ocean-waters approach the coast and are depressed below the surface near the shore.

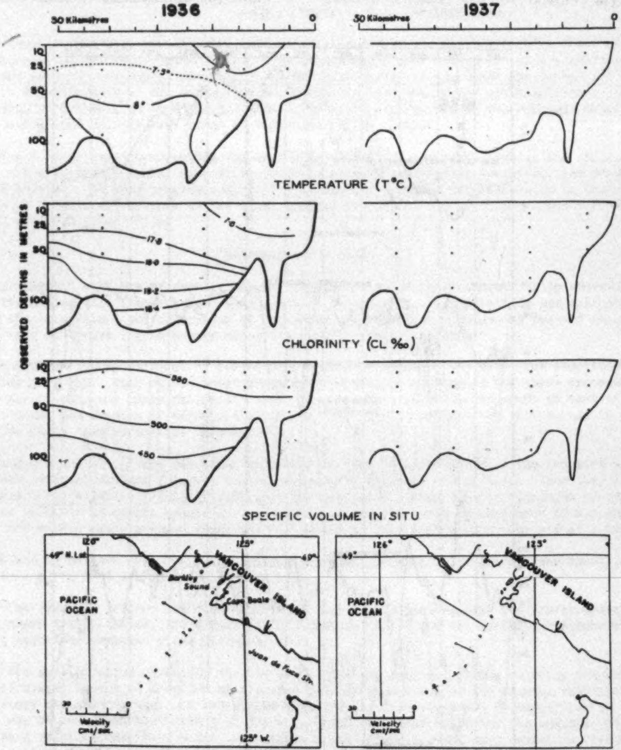

FIG. 3—DYNAMIC OBSERVATIONS SEAWARD FROM CAPE BEALE, DEMONSTRATING SPRING CONDITIONS

When there is an offshore transport of surface-waters the dilution-effects are reduced and when the transport is onshore the effects are increased. Consequently the coastal chlorinity-observations are a direct measure of the combined land-drainage and wind-transport of the surface-waters.

The annual cyclic nature of the winds is not as apparent as that of the precipitation and chlorinity. The winds in winter have their greatest velocity from the south and southeast while the major force of the winds in summer is from the northwest. During each season the wind-direction tends to veer from northwesterly to southeasterly, having the nature of a short-term rhythm similar to that shown by Skogsberg's observations of the nature of upwelling in Monterey Bay [1936].

Oceanographical data

In Bjerknes' theorem it is shown that in the open sea in the Northern Hemisphere, if the sea-water to the right is less dense than that to the left, then the current is flowing away from the observer in the direction in which he is facing.

In the analysis of the current-system off the west coast of Vancouver Island it is convenient to consider the primary density determining factors of temperature and salinity separately, since

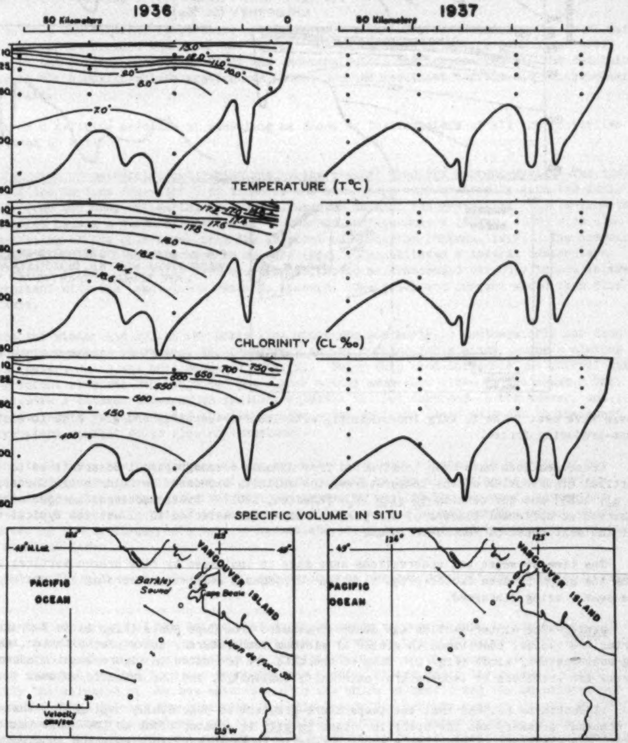

FIG. 4—DYNAMIC OBSERVATIONS SEAWARD FROM CAPE BEALE, DEMONSTRATING EARLY SUMMER CONDITIONS

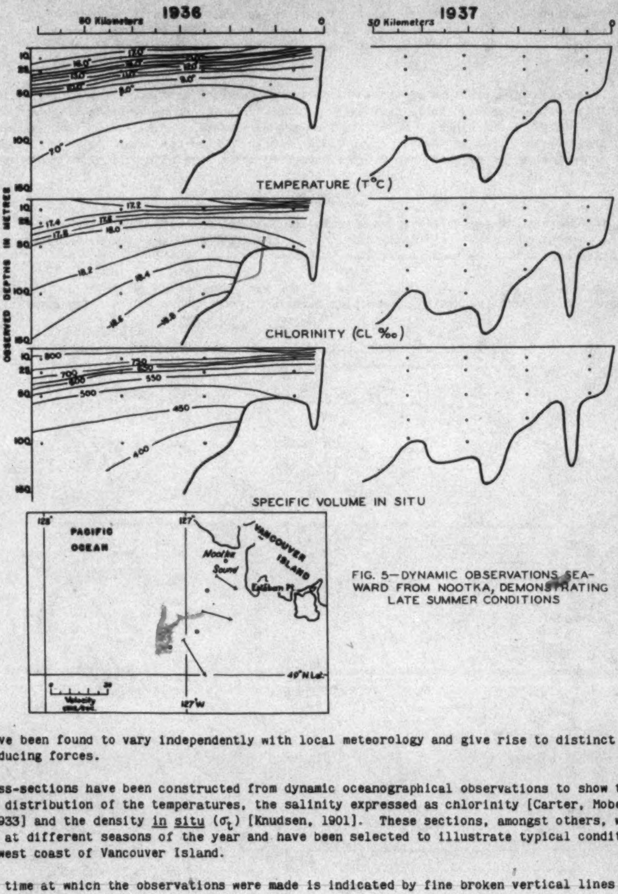

FIG. 5—DYNAMIC OBSERVATIONS SEAWARD FROM NOOTKA, DEMONSTRATING LATE SUMMER CONDITIONS

these have been found to vary independently with local meteorology and give rise to distinct current-producing forces.

Cross-sections have been constructed from dynamic oceanographical observations to show the vertical distribution of the temperatures, the salinity expressed as chlorinity [Carter, Moberg, *et al*, 1933] and the density *in situ* (σ_t) [Knudsen, 1901]. These sections, amongst others, were observed at different seasons of the year and have been selected to illustrate typical conditions off the west coast of Vancouver Island.

The time at which the observations were made is indicated by fine broken vertical lines cutting the graph of each factor (Fig. 2) which furnishes a complete meteorological description of the season being discussed.

Winter--The winter-section was observed seaward from Cape Beale (Fig. 1) on February 28, during the typical conditions in winter of minimum temperatures, heavy precipitation, and prevailing southeasterly winds (Fig. 2) Some of the data are presented in Figure 3 which shows respectively the isometers of temperature, chlorinity (salinity), and the specific volume.

It should be noticed that the temperature-gradient is practically negligible, that the water is somewhat fresher, and the specific volume *in situ* is greater close to the shore than to seaward. The resultant current has a small but definite northwestward flow which is greatest near the coast where the lateral density-gradient is a maximum.

There appears to be some evidence of a depression of the isometers of chlorinity and specific volume close to the shore which indicates a zone of sinking.

Early summer--The conditions prevalent in the early summer are demonstrated in Figure 4 from the data observed on June 20 from Cape Beale (Fig. 1) during a period of maximum air-temperatures, low precipitation, but heavy land-drainage from the melting snow.

The lateral temperature-gradient was small, but there is a large volume of fresh water close to the shore due to the influx of snow-drainage. The resultant distribution of specific volume is variable but the general shoreward depression of the isometers indicates that the waters are less dense near the coast. The resultant current is generally northwestward, but the small slope of the isometers of specific gravity and the form of the current-vectors indicate that the current is probably a series of eddies which may vary their direction of general movement with any small change in the physical characteristics.

Late summer--Off Cape Beale (Fig. 1) observations were made which demonstrated the conditions (Fig. 5) during the late summer, that is, maximum temperatures, minimum rainfall, maximum coastal chlorinity, and prevailing northwesterly winds (Fig. 2).

The lateral and vertical temperature-gradients were both fully developed and the water was distinctly warmer to seaward than near the shore. The vertical chlorinity-gradient was well developed but the lateral surface-gradient was indeterminate. The temperature was the determining factor in the specific-volume gradient as shown, and the resultant current was southeastward along the coast.

There is a definite evidence of upwelling as shown by the isometers of all the properties below a depth of 50 metres.

The relation of meteorological variations to the coastal gradient current-system--The temperatures of the surface sea-water like those of the atmosphere vary seasonally with the incident energy from the Sun, but during the summer, and the autumn, the prevailing wind is from the northwest which causes a seaward movement of the warmed surface-waters [Ekman, 1928] with consequent upwelling of the cold water from the adjacent ocean-depths [McEwen, 1912]. The presence of cold water along the coast and warm to seaward (Fig. 5) constitutes a lateral temperature-gradient, which, if the chlorinity were disregarded, could be transposed directly into a lateral density-gradient with the least-dense water to seaward. The resultant current would then flow southeastward.

During the winter and spring the prevailing winds are southerly to southwesterly and tend to force the surface-waters shoreward, and conversely to the northwesterly winds, cause a sinking of the coastal waters which has been observed (Fig. 3). Under this wind-influence the lateral temperature-gradient reverses its slope and the warmer waters are found close to the coast. This in turn constitutes a lateral temperature-gradient opposite to that observed in the summer, and if the chlorinity again be disregarded the least-dense water would be found adjacent to the coast and the resultant current would flow northwestward.

During the winter the temperature of the sea-water is almost constant to a depth of 100 meters and regardless of the direction of the drift-current or the distortion of the surface-waters, the lateral temperature-gradient is truly or nearly negligible. However, during the spring there is a distinct vertical temperature-gradient, and the warmer surface-waters under the influence of the drift-currents tend to accumulate along the coast, giving rise to a northwestward gradient-force.

The chlorinity also varies seasonally, and depends on coastal precipitation and land-drainage.

Due to the topography of the Island, the drainage tends to concentrate in the inlets, from which there is an almost continuous surface-outflow of fresh or brackish water [Tully 1936]. During the late summer there is a minimum of precipitation and as the season advances after the melting snow has drained from the mountains the coastal rivers reach their lowest levels. Consequently the dilution of the sea-water close to the shore is small, and the chlorinity attains a maximum (Fig. 2). At the same time the offshore drift-currents tend to remove what little brackish water there is present, and the upwelling of the deep water tends to increase the coast chlorinity. Disregarding the temperature, a gradient is formed, with the maximum chlorinity and consequently the maximum density adjacent to the coast, and the resultant gradient-current flows southeastward.

During the autumn, winter, and spring there is much more precipitation than in the summer, and a surface-layer of brackish water is developed close to the coast. Under the influence of the prevailing southwesterly winds it is held close to the shore and consequently a lateral chlorinity-gradient is developed, which if the temperature were neglected could be transposed into a density-gradient in which the less dense water would be on the shoreward side, and the resultant gradient-flow would be northwestward.

If the density as the resultant of these two factors--temperature and chlorinity--is considered, it may be shown that there is a distinct seasonal variation in the coastal current-forces resulting from the combination of the individual seasonal characters of each component.

In the spring and autumn the temperatures decrease and the chlorinity increases to seaward. Thus the density-gradient increases to seaward in both cases and the resultant gradient-current causes a definite northward flow.

In the winter the temperature-gradient is negligible and the chlorinity-gradient increases to seaward, which results in a northward gradient-flow (Fig. 3), but of less magnitude than that in the other seasons since only the one factor of land-drainage influences the density-gradient.

In the early part of the summer the temperature- and chlorinity-gradients have opposite effects on the density-gradient. The temperature generally increases to seaward, and the considerable land-drainage from the melting snows establishes a coastal zone of low chlorinity. The density-gradient is dependent on the relative magnitude of these two factor-gradients and the resultant current may be in either direction.

Off the southern end of Vancouver Island the effect of the land-drainage from the whole mainland coast-region is effective through Juan de Fuca strait (Fig. 1) with the result that the chlorinity is usually the determining factor in the density-gradient. Off the western side of the north end of the Island where only the Island drainage is effective the temperature-gradient is sufficient to offset the effects of the chlorinity in the density-gradient.

During the early summer the gradient-current is small and probably takes the form of a series of eddies along the coast whose force and direction are delicately balanced.

During the late summer the land-drainage is at a minimum and the prevailing winds carry the surface-waters offshore where a zone of low density is developed at the surface. Near the shore the upwelling phenomena give rise to a zone of cold water of relatively high chlorinity and consequently high density. The resultant density-gradient decreases offshore and induces a southeastward flowing current.

The considerations of the structure of the coastal gradients indicate that the current should have a maximum northwestward velocity at the time of lowest coast salinity, and least lateral temperature-gradient.

Summary

In the discussions of the data it has been shown that the variations in sea-water chlorinity and temperature are seasonal and can be directly related to local meteorology. The coastal chlorinity-gradient varies directly as the land-drainage, which in turn varies directly as the precipitation and atmospheric temperature in the coastal drainage-area. The magnitude of the temperature-gradient varies directly with the atmospheric temperature (incident energy), and increases onshore during southwesterly winds. If the prevailing wind is northwesterly, the temperature-gradient tends to increase offshore.

Since the drainage-characteristics of the land are constant and the warming effect of the Sun is approximately a regular seasonal function, the effective variables controlling the direction and velocity of the coastal current are precipitation and force and direction of wind.

It follows that the continuous observation of atmospheric meteorology and temperature and chlorinity of sea-water constitutes direct observations of the major forces controlling the coastal gradient-current system and serves as a record of its variations. Therefore it is essential, in any study of a similar coastal current-system, to establish the relationship between these factors, and the rate and direction of the resultant coastwise flow.

Acknowledgments

The dynamic observations discussed in this paper were made through the cooperation of the Department of National Defense with the assistance of the officers and men of H.M.C.S. Armentieres. The meteorological observations have been collected through the facilities of the Meteorological Bureau and the records of the temperatures and chlorinity of sea-water have been made possible through the Lighthouse Service and the close personal cooperation of the lightkeepers along the Canadian Pacific Coast. The chlorinity-determinations and mathematical treatment of the data were carried out by W. Anderson, R. N. Largue, and H. J. Hollister. It gives me great pleasure to acknowledge this assistance.

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AMERICAN GEOPHYSICAL UNION

Nineteenth Annual Meeting

SECTION OF TERRESTRIAL MAGNETISM AND ELECTRICITY

Washington, D. C.
April 27, 1938

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PAPERS

AN ELECTROMAGNETIC METHOD OF DETERMINING INDUCTION-COEFFICIENTS OF MAGNETOMETER-MAGNETS

J. H. Nelson

(Abstract)

If a piece of iron or steel is placed in a magnetic field, it is magnetized by induction. This is true whether the iron or steel is already permanently magnetized or not. Suppose that a permanent magnet is placed in a magnetic field parallel to the direction of the magnet, that is, so that no torque results from the action of the field on the magnet. The strength or magnetic moment of the magnet will be increased (or decreased, if the field of the magnet opposes the applied surrounding field) by induction. The ratio of the change dM, caused by a field of unit-strength, to the original moment M of the magnet is called the induction-coefficient h. The product hM is called the induction-factor of the magnet, designated by μ. It will be seen that the induction-coefficient of a magnet changes with a change of moment of the magnet, being inversely proportional to the moment within the range of field-strengths used in this investigation. The induction-factor μ, for all practical purposes in magnetometer work, may be considered a constant over fairly long periods of time.

This paper deals with a method proposed by the author of determining the induction-coefficient of magnetometer-magnets by using an electrically created magnetic field.

The complete paper is published in the Journal of Terrestrial Magnetism and Atmospheric Electricity [v. 43, pp. 159-165, 1938].

U. S. Coast and Geodetic Survey,
Washington, D. C.

TESTING THE ORIENTATION OF A DECLINATION-VARIOMETER BY ITS TEMPERATURE-COEFFICIENT

H. Herbert Howe

In July 1936, while assigning base-line values for the Eschenhagen declination-variometer at the Cheltenham Magnetic Observatory, P. H. Williamson noted considerable range in the observed values, and discovered that it was closely related to change of temperature. He submitted to me a graph showing this relation clearly. Figure 1 was made from a tracing of the essential features

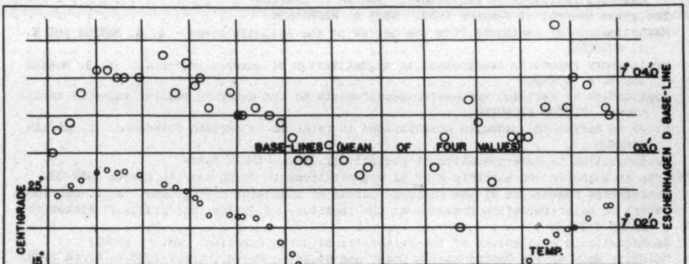

FIG. 1—TEMPERATURE AND D BASE-LINE VALUES, CHELTENHAM

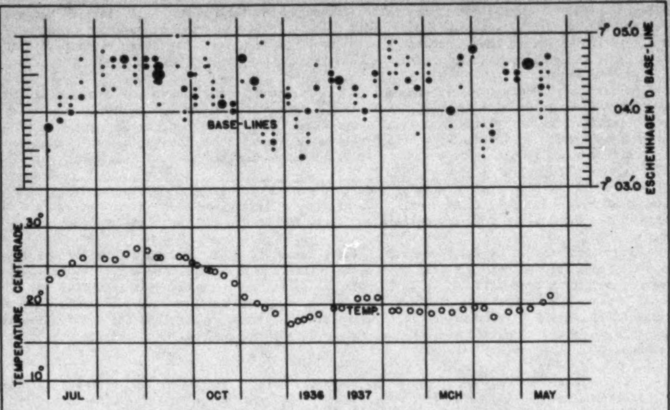

FIG. 2—TEMPERATURE AND D BASE-LINE VALUES, CHELTENHAM.

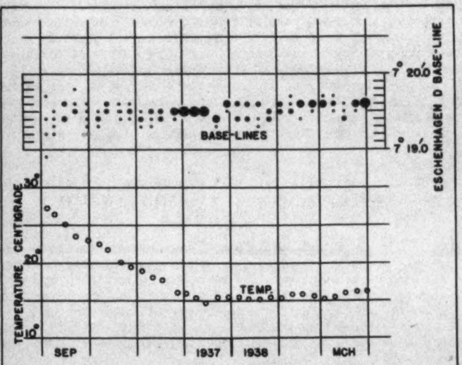

FIG. 3—TEMPERATURE AND D BASE-LINE VALUES, CHELTENHAM.

of that graph. It shows a change of about 1'.3 in the declination base-line value for a change of 18° C in temperature, or a temperature-coefficient of about 0'.077/°C. (The variation of temperature during that winter was unusually large because the room was not artificially heated. There was excessive variation of the observed values during March and April because a field-magnetometer was used while the standard instrument was being repaired.)

I had already worked out the general theory of the declination-variometer, including the fact that it has a small temperature-coefficient. There are two reasons for this: (1) The magnetic axis of the recording magnet is not as a rule parallel to the horizontal component of the Earth's field, so that the exact orientation depends upon its moment (the magnet cannot be always parallel to the Earth's field because of the torsion in the fiber); and (2) the recording magnet is affected somewhat by the other magnets of the magnetograph, the effect of which varies with temperature.

However, the observed temperature-coefficient was several times as large as the expected one. After careful consideration I noted, in my next weekly report to the Chief of the Division of Terrestrial Magnetism and Seismology, that this excessive temperature-coefficient apparently existed and that it could be accounted for in terms of known causes only by assuming that the axis of the recording magnet deviated from the magnetic meridian by an excessive amount, perhaps 1°.5.

Neglecting the effect of other magnets, the temperature-coefficient of a declination-variometer [see 1 of "References" at end of paper] is, in minutes of arc per degree, $q \tan v \cot 1'$, where v is the angle between the magnetic axis and the magnetic meridian and q

is the temperature-coefficient of the ratio of torsion-constant to magnetic moment. Since q was unknown, only an approximate value of v could be given, but the temperature-coefficient was large enough to indicate that the variometer was not properly adjusted. The above estimate of the error of orientation was actually intended as the least likely value.

In September 1936, upon my recommendation, the orientation was tested and an error was indicated, in the predicted direction, of 1° 54'. This test was made by deflecting the recording magnet with an auxiliary magnet placed magnetically south of it, with its axis along the magnetic meridian. (The magnetic meridian inside the variometer-room had already been located with a non-magnetic theodolite in connection with testing the orientation of the horizontal-intensity variometer.)

There are two principal effects of such an error in orientation. First, the base-line value changed with temperature. In this case, its effect was negligible over short periods, and the seasonal fluctuation could be approximately allowed for in the adopted base-line values.

Second, the base-line value changed 1' for a change of about 160γ in the horizontal intensity. One result of this was a spread of observed base-line values if they were taken at different times of day, or a systematic error in the mean value if they were taken at the same time of day. Figure 2 shows all observed values from July 1936 to May 1937, uncorrected for temperature or horizontal intensity. After the error of orientation was discovered, this fluctuation was taken into account in the base-line assignments, but to date no attempt has been made to correct the scalings.

A similar test in June 1937 (the error meanwhile having been increased about 12' by replacement of the horizontal-intensity variometer) showed the error to be 2° 14' by one method and 2° 42' by another.

In September 1937 the variometer was adjusted so that the magnetic axis is now approximately in the magnetic meridian. The method of adjustment consisted of removing the magnet, suspending a torsion-weight, and turning the torsion-head until the spot returned to its previous position, the angle through which the torsion-head was turned corresponding to an error of orientation of about 2°.5. Then the magnet was replaced and adjusted with respect to the mirror until the spot returned to the same place again. There is then no torque on the magnet about a vertical axis. The magnet is not exactly in the Earth's magnetic meridian because it is deflected by the other variometer-magnets. However, it appears that this error is only a few minutes, probably less than the uncertainty of the method of testing orientation by deflecting with an auxiliary magnet.

Since this adjustment, the observed base-line values have been very regular, indicating, in themselves, that the adjustment was quite satisfactory. Figure 3 shows all of the observed values reported to date.

It thus appears that when there is a large range of temperature, the orientation of a declination-variometer may easily be checked approximately by its temperature-coefficient. Unless the temperature-coefficient q has been measured, the method is rather uncertain quantitatively. It is somewhat less accurate than the other methods mentioned, which, as the above data show, do not give exact results. It may be quite useful for detecting errors of orientation, and frequently more useful than the change of base-line value with horizontal intensity. In this case, the seasonal change of base-line value was about 1'.3 while, as seen in Figure 2, the range of base-line values on a given day was usually not more than 0'.5.

Reference

[1] H. H. Howe, On the theory of the unifilar variometer, Terr. Mag., v. 42, pp. 29-42, 1937 (equation 38).

U. S. Coast and Geodetic Survey,
Washington, D. C.

ALTERNATING-CURRENT METHOD OF MEASURING MAGNETIC POLARIZATION OF ROCKS

E. A. Johnson

(Abstract)

A method of accurately measuring the residual magnetization of small cubical specimens by the use of an alternating-current detector is described. The specimen is rotated inside of a

multilayer coil and the voltage induced in the coil is measured by a high-gain amplifier. The limit of measurement is calculated from the noise-level computed for the amplifier and the voltage induced in the coil, on the assumption that the moment of the specimen can be represented as a dipole. A moment of 1×10^{-8} CGS unit per cc can be detected. The method eliminates the uncertainties of a magnetometric method and is considerably more sensitive than direct-current detectors. Measurements have been made on feebly polarized sediments of the oceanic bottom.

The complete paper will be published in the Journal of Terrestrial Magnetism and Atmospheric Electricity.

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Carnegie Institution of Washington,
Washington, D. C.

THE BRIGHTNESS OF THE TWILIGHT-SKY AND THE DENSITY AND TEMPERATURE OF THE ATMOSPHERE TO ABOUT 60 KM

E. O. Hulburt

(Abstract)

Direct observation of the composition, temperature, and density of the atmosphere has been made to an altitude of about 22 km above the surface of the Earth by means of manned balloon flights. Notably, in 1935 the National Geographic Society - United States Army Air Corps balloon _Explorer II_ reached an altitude of 22 km. A few ascensions in Germany of free balloons carrying instruments have attained nearly 30 km. The results have shown that the temperature of the atmosphere decreases with altitude from its sea-level average value of about 287° K to about 220° K at 10- to 14-km altitude and then remains approximately constant to about 30 km. The composition of the atmosphere is closely constant with altitude, being the same at 20 km as it is at sea-level, with the exception of water-vapor and ozone which, however, comprise only a very small fraction of the atmosphere.

Radio investigations show that there is ionization above 90 km. Meteor-trails are seen at altitudes from 50 to 100 km. These phenomena indicate that the atmosphere extends to great heights, of course very attenuated, but offer only indirect and vague information concerning its amount and nature. Last year a powerful searchlight was directed upward [E. O. Hulburt, J. Opt. Soc. Amer., November 1937]. Its beam was detected by photography to a vertical height of about 28 km but was too faint to yield exact values of the atmospheric density above 22 km.

In the present experiments sunlight was used. As the Sun sets the Earth's shadow above the observer moves upward and the region of the atmosphere illuminated by the direct rays of the Sun moves to high levels. A reversed sequence of events occurs before dawn. Measurements were made during clear weather of the brightness of the zenith-sky for about an hour after sunset and an hour before sunrise. From the measurements, combined with the known intensity of sunlight and the law of scattering of light by air, the density (or pressure) and the temperature of the atmosphere were determined. Corrections were made for secondary scattering which were obtained from measurements of the brightness of the sunset- or sunrise-glow of the twilight-sky near the west or east horizon.

The temperature came out to be between 205° and 235° K from 12 km to about 60 km. Thus, the temperature of the upper air was cold and was roughly constant with altitude. From the temperature the pressure was determined. No important changes were noted with the season from October to April. The measurements will be continued during the summer to complete a year's observations. For altitudes above about 60 km the method fails, since at greater heights the light of the twilight sky is very weak and the corrections due to secondary scattering are large.

It is to be remembered that the above results refer only to the atmosphere during conditions of twilight in a temperate latitude (Washington, D. C.). One would expect that the upper air grows warmer during the day and cooler at night. However, the change between day and night may not be very great, and might be supposed to increase with increasing altitude.

The complete paper appeared in the Journal of the Optical Society of America [v. 28, pp. 227-236, July 1938].

U. S. Naval Research Laboratory,
Washington, D. C.

CORRELATION OF COSMIC-RAY GEOMAGNETIC EFFECTS

Thomas H. Johnson

In the theory of the cosmic-ray orbits in the magnetic field of the Earth developed by Störmer and Lemaître and Vallarta the intensity of the cosmic radiation in space outside the atmosphere depends upon five parameters, all of which are subject to independent variation. Corresponding to these there are five geomagnetic cosmic-ray effects, namely, the latitude-effect, the zenith-angle effect at constant azimuth, the azimuthal effect at constant zenith-angle, the radius- or longitude-effect, and the magnetic moment or storm-effect. All of these effects have been reported from observational data and their interpretation given in terms of the theory. In most instances the theory has been used to determine the variation of the low-energy limit of the incident radiation and the energy-spectrum of primary cosmic radiation has appeared in the interpretation as an adjustable parameter. Since all of the effects depend upon this parameter and upon the relative intensities of positives and negatives in the primary beam, we may inquire to what extent the various effects are consistent with one another. Since atmospheric absorption is always present, it will be convenient to refer to the intensity as being proportional to the effects produced after reaching the point of observation by the particular component of primary radiation. Because of the greater wealth of observational data the effects observable in the vertical direction at sea-level will be of principal interest.

In view of Liouville's theorem the intensity, J, of the cosmic radiation on the Earth's surface can be expressed by sum of two integrals

$$J = \int_{V_c^+}^{\infty} + J_V^+ \, dV + \int_{V_c^-}^{\infty} - J_V^- \, dV + K \tag{1}$$

one of which represents the intensity due to positive primaries and the other that due to negatives. $J_V^{\pm}\,dV$ is the intensity at the point of observation produced by the positive or negative primaries in the energy-range dV, and K is a constant intensity inserted to take account of any possible uncharged radiation, not affected by the magnetic field. The orbital theory defines the low-energy limits $V_c^{\pm}$ of positives and negatives in terms of the magnetic moment M and the radius a of the Earth by a relation which for high energies converges to

$$V_c^{\pm} = 300\, M\, (r_c^{\pm})^2/a^2 \tag{2}$$

Here $r_c^{\pm}$ is a purely mathematical function of the latitude λ and direction (specified by zenith-angle and azimuth, ξ and φ, respectively).

From (1) and (2) the five geomagnetic effects in differential form may be expressed as follows.

(I) The latitude-effect--

$$\partial J/J\partial\lambda = -\,(600\, M/Ja^2)\,[J_V^+(V_c^+)r_c^+\,\partial r_c^+/\partial\lambda + J_V^-(V_c^-)r_c^-\,\partial r_c^-/\partial\lambda] \tag{3}$$

At all points in the meridian plane, including the zenith, $r_c^+ = r_c^-$, $\partial r_c^+/\partial\lambda = \partial r_c^-/\partial\lambda$ and

$$\partial J/J\partial\lambda = -\,(600\, M/Ja^2)\, J_V\,(V_c)\, r_c\,\partial r_c/\partial\lambda \tag{3a}$$

$$J_V \equiv J_V^+ + J_V^-;\ r_c \equiv r_c^+$$

(II) The azimuthal effect at constant zenith-angle--

$$\partial J/J\partial\varphi = -\,(600\, M/a^2J)\,[J_V^+(V_c^+)r_c^+\,\partial r_c^+/\partial\varphi + J_V^-(V_c^-)r_c^-\,\partial r_c^-/\partial\varphi] \tag{4}$$

In the zenith

$$\partial J/\partial\varphi = 0 \tag{4a}$$

In the meridian plane

$$\partial r_c^+/\partial\varphi = -\,\partial r_c^-/\partial\varphi \text{ and } \partial J/Jd\varphi = -\,(600\, M/a^2J)[J_V^+(V_c) - J_V^-(V_c)]\, r_c\,\partial r_c/\partial\varphi \tag{4b}$$

(III) The zenith-angle effect at constant azimuth--

$$\partial j/j\partial\xi = -\,(600\ M/a^2 j)\ [j_v^+(V_c^+)r_c^+\ \partial r_c^+/\partial\xi + j_v^-(V_c^-)r_c^-\ \partial r_c^-/\partial\xi] + (\partial j/j\partial x)(dx/d\xi) \qquad (5)$$

where x is the atmospheric-path length. In the east-west plane in the zenith (ξ increasing towards the east) $\partial r_c^+/\partial\xi = -\,\partial r_c^-/\partial\xi$ and $dx/d\xi = 0$. Hence

$$\partial j/j\partial\xi = -\,(600\ M/a^2 j)\ [j_v^+(V_c) - j_v^-(V_c)]r_c\ \partial r_c/\partial\xi \qquad (5a)$$

In the north-south plane at the zenith (ξ increasing towards the north) $\partial r_c^+/\partial\xi = \partial r_c^-/\partial\xi$ and

$$\partial j/j\partial\xi = -\,(600\ M/a^2 j)\ j_v(V_c)r_c\ \partial r_c/\partial\xi \qquad (5b)$$

(IV) The radius- or longitude-effect--

$$a\partial j/j\partial a = (600\ M/a^2 j)\ [j_v^+(V_c^+)(r_c^+)^2 + j_v^-(V_c^-)(r_c^-)^2] \qquad (6)$$

In the zenith

$$a\,dj/j\,da = (600\ M/a^2 j)\ j_v(V_c)\ r_c^2 \qquad (6a)$$

(V) The magnetic-moment effect--

$$M\partial j/j\partial M = -\,(300\ M/a^2 j)\ [j_v^+(V_c^+)(r_c^+)^2 + j_v^-(V_c^-)(r_c^-)^2] \qquad (7)$$

In the zenith

$$M\partial j/j\partial M = -\,300\ M\ j_v(V_c)\ r_c^2/ja^2 \qquad (7a)$$

The simplicity of the equations which refer to the zenith-intensities make them especially suitable for comparison with the experimental data. Of these (5a) alone contains the difference of the positive and negative intensities while the others contain their sum. If we define $\alpha = j_v/(j_v^+ - j_v^-)$ combination of (5a) with each of the other equations representing zenith-effects gives the following four determinations of α in terms of the experimentally determined geomagnetic effects:

(3a) with (5a) $$\alpha = (\partial j/j\partial\lambda)(\partial r_c/\partial\xi)_{EW}/(\partial j/j\partial\xi)_{EW}(\partial r_c/\partial\lambda) \qquad (8)$$

(5b) with (5a) $$\alpha = (\partial j/j\partial\xi)_{NS}(\partial r_c/\partial\xi)_{EW}/(\partial j/j\partial\xi)_{EW}(\partial r_c/\partial\xi)_{NS} \qquad (9)$$

(6a) with (5a) $$\alpha = -\,(a\partial j/j\partial a)(\partial r_c/\partial\xi)_{EW}/(\partial j/j\partial\xi)_{EW}r_c \qquad (10)$$

(7a) with (5a) $$\alpha = 2\ M\partial j/j\partial M(\partial r_c/\partial\xi)_{EW}/r_c(\partial j/j\partial\xi) \qquad (11)$$

All of these determinations of α are subject to the accuracy of the measurements of the zenith-angle effect in the east-west plane. This is probably the most accurately measurable geomagnetic effect and the agreement between the values obtained by the four methods may be regarded as a criterion of the consistency of the other observed geomagnetic effects with the theory. This criterion is now independent of the primary energy-spectrum or of the actual distribution between positives and negatives, assuming this to be the same at all energies.

Values of the various quantities involved in these equations are tabulated in Table 1. The parameter r_c has been computed by Lemaître and Vallarta [1] and its values for the vertical direction are given in the first row. The derivatives of r_c with respect to latitude and zenith-angle listed in the second, third, and fourth rows have also been taken from Lemaître and Vallarta's results and these also apply to the zenith-direction. Experimental values for the latitude-effect, listed in the fifth row, have been scaled from the average curve of Compton and Turner [2] corrected for external temperature and then reduced to the vertical intensity by the method of Gross. The details of this operation have been described elsewhere [3]. It is extremely difficult to judge the accuracy of the values thus found. The consistency of the original data after the corrections have been applied indicates that the tabulated values can be

Table 1--Results of computations

Row	Value and data used	Geomagnetic latitude			
		0°	10°	20°	30°
1	r_c (Lemaître and Vallarta)	0.500	0.490	0.470	0.419
2	$\partial r_c/\partial\lambda$ per degree	0	-0.0015	-0.0035	-0.0075
3	$(\partial r_c/\partial\xi)_{EW}$ per degree	0.0022	0.0021	0.0018	0.0013
4	$(\partial r_c/\partial\xi)_{NS}$ per degree	0	0.0003	0.0007	0.0007
5	$\partial j/j\partial\lambda$ (data of Compton and Turner reduced to vertical intensity)	0	0.0019	0.0028	0.0065
6	$\partial j/j\partial\lambda$ (vertical intensity, data of Johnson and Read)	-0.001	0	0.0037	0.0054
7	$a\partial j/j\partial a$ (Millikan and Neher, Clay, Compton, Hoerlin)	1.3			
8	$M\partial j/j\partial M$ (Forbush, Hess and Demmelmair)	-30			-30
9	$(\partial j/j\partial\xi)_{NS}$ (Johnson, 4300 meters)				0.0008
10	$(\partial j/j\partial\xi)_{EW}$ (Johnson, sea-level)	-0.0017	-0.0015	-0.0013	-0.0009
	(Johnson, 4300 meters)	-0.0026			-0.0022
11	α from latitude-effect	indeterm.	1.78	1.18	1.20
12	α from north-south effect				0.68
13	α from radius-effect	3.4			
14	α from magnetic-storm effect	130			138

relied upon to within 20 per cent. The directly measured vertical intensities of Johnson and Read [4], listed in the sixth row, have been scaled from their average curve and are perhaps somewhat less reliable than the values reduced from the results of Compton and Turner. Since the integrated latitude-effect found by the unidirectional measurements agrees approximately with that found by the analysis by Gross of the results of Compton and Turner, they lend weight to the values listed in the fifth row although the agreement is poor in the individual cases. These discrepancies have been attributed to the local magnetic anomalies in the vicinity of the Panama Canal Zone. In judging the values of the radius-effect listed in the seventh row of Table 1 it must be remembered that the data obtained by any one instrument are meager, and the time elapsed between observations at different latitudes may be long. Since fluctuations of an unexplained character are known to take place at a single station it is obvious that comparisons of intensities measured at different times based upon a few observations may be considerably distorted. The data upon which the figure 1.3 has been based have been compiled by Vallarta [5]. The minimum intensity, according to these data, occurs 90° in longitude west of the point of minimum radius and the amplitude of the longitude-effect is from two to three times greater than can be reconciled with the latitude-effect. It seems probable, however, that the uncertainties of the observational data may be of this order.

The magnetic-moment effect listed in the eighth row has been taken from the observations reported by Forbush [6] and by Hess and Demmelmair [7] during the storm of April 1937. Other observations [8] on fluctuations correlating with magnetic disturbances suggest that wide variations in the ratio of cosmic-ray to magnetic fluctuations may occur and that during a single storm at different phases of the disturbance there is no constant ratio of cosmic-ray to magnetic variation. Thus the figure -30 listed in Table 1 may be regarded as a very rough estimate of the order of magnitude. In actuality the ratio of the cosmic-ray disturbance to the horizontal-field variation is the quantity determined and this can be converted into the magnetic-moment coefficient only by making assumptions regarding the nature of the magnetic disturbance. Here it has been assumed, as suggested by Chapman [9], that the disturbance is due to a circular ring-current near the Earth.

The north-south effect, listed in the tenth row, was measured at 4300 meters in geomagnetic latitude 29° north by the writer [10]. The probable errors of the points and the consistency of the determinations at different angles indicate that the value given is correct to within 30 per cent.

The east-west effects, listed in the eleventh row, are probably correct to within ten or 15 per cent. The nature of the asymmetry observations allows one to make a much more accurate estimate of the probable errors than is possible with the measurements of other geomagnetic effects.

Within the allowable errors the values of α listed in rows 11 to 13 are consistent with one another. The magnetic-moment effect, however, leads to a value of α differing from the others in order of magnitude and indicates that this effect is not a true geomagnetic effect of the type considered in the deduction of equation (7).

In interpreting the values listed in Table 1 it should be noted that $\alpha = 1$ corresponds to 100 per cent positive intensity; $\alpha = 1.5$ represents a negative intensity equal to one-sixth of the total; the value α less than one listed in line 12 is impossible and must arise from experimental or theoretical errors.

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ON COSMIC-RAY EFFECTS ASSOCIATED WITH MAGNETIC STORMS

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(Abstract)

Evidence indicates that the storm-time field of some magnetic storms causes world-wide changes of several per cent in cosmic-ray intensity. That other magnetic storms of equal intensity at the Earth's surface occur with no appreciable cosmic-ray effects definitely indicates that the entire current-system for the storm-time field of both types of storms can not be located at the same distance above the Earth. In particular, the possibility that the current-systems responsible for the two types of storms both lie within the Earth's atmosphere appears remote. Assuming the current-system for the storm-time field of both types of storms to consist of a ring concentric with the Earth in the geomagnetic equatorial plane, magnetic data are analyzed to determine whether the radius of the assumed ring is, as would be expected, greater for magnetic storms which affect cosmic-ray intensity. Although the analysis is not conclusive on this point, the results satisfy a necessary condition for the existence of such a ring-current. The occurrence of aurora in temperate latitudes during most of the magnetic storms which affected cosmic-ray intensity is interpreted, after Størmer, to indicate the existence of such ring-currents. The percentage-changes in cosmic-ray intensity during magnetic storms are within the observational uncertainty the same at geomagnetic latitudes 50°.1 north and 0°.6 south. The significant correlation between changes in daily means of cosmic-ray intensity, for two stations separated 50° in latitude, probably results from the same mechanism responsible for the magnetic-storm effect.

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LATITUDE-EFFECT IN ELECTRICAL RESISTANCE OF A COLUMN OF ATMOSPHERE

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Introduction--The electrical resistance of a vertical column of atmosphere of unit cross-section is a convenient concept for use when considering some aspects of atmospheric electricity. For example, the Earth and the high atmosphere may be regarded, for some purposes, as two conductors of negligible resistance which constitute a spherical condenser, the outer element of which has a potential V relative to the inner element. Then if r is the columnar resistance of the atmosphere, the current-density (i) across this condenser at a given place is V/r. At a place situated on a high plateau r is doubtless generally less and i consequently is greater

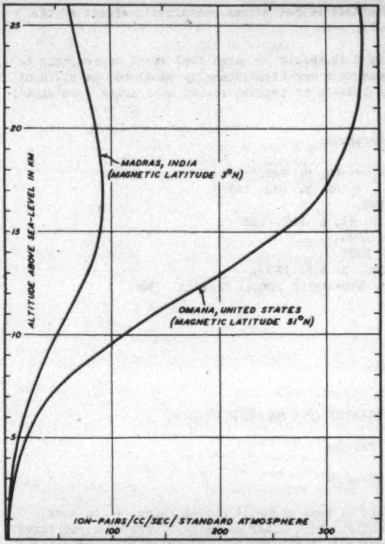

FIG. 1—RATE OF ION-FORMATION BY COSMIC-RADIATION FOR MIDDLE AND LOW LATITUDES (DERIVED FROM DATA OF BOWEN, MILLIKAN, AND NEHER)

than at sea-level. Also if r is dependent upon latitude, then i should vary with latitude. Furthermore, since at the surface the current is also equal to the produce of the potential-gradient (E) and the conductivity (Λ), it follows that $V/r = \Lambda E$, provided, as seems quite likely at a given time and place, i is approximately constant throughout the length of the column. Thus if V and Λ are constant while r varies, say with respect to latitude, then E will vary inversely as r. This is an analogue of what appears to be an actual set of circumstances in the atmosphere over the oceans. The evidence of those circumstances shall be considered later.

The cosmic radiation, which is doubtless the predominant ion-forming agency in the atmosphere--except in the lower stratum over land--up to considerable altitudes in the stratosphere, depends upon latitude in a striking manner. This is illustrated by the data of Bowen, Millikan, and Neher [see 1 of "References" at end of paper] exhibited in Figure 1 (which Dr. Millikan kindly made available for this calculation prior to their publication) and is borne out by other investigations [2]. At 50° geomagnetic latitude the intensity of cosmic radiation (I) at an altitude of 20 km is nearly four times the value for the same altitude at the equatorial station, although the values at the surface differ only slightly. Such a contrast in the intensity of the ionizing agent implies a corresponding contrast in the conductivity throughout the atmosphere and an inverse contrast in the columnar resistance. This contrast should also be reflected in other electrical aspects of the atmosphere, such as the air-earth current and the potential-gradient at the surface during fair weather, provided the contrast in the columnar resistance is great enough.

The considerations involved in the calculation of the resistance shall be reviewed first, after which the latitude-effect indicated by the calculated resistance shall be compared with what appear to be corresponding effects in potential-gradient and possibly also in the air-earth current in the atmosphere over the oceans. Conditions over land are generally too complicated to admit of such a comparison.

Dependence of columnar resistance upon cosmic radiation--The resistance of a vertical column of air of 1-cm^2 cross-section from the surface up to a height (h) shall be determined by evaluating the integral of the reciprocal of the conductivity for the altitude-range 0 to h. The distribution of conductivity with altitude is here calculated from the observed distribution of cosmic-ray intensity, shown in Figure 1. The justification of the method of calculating the conductivity rests in part upon the results of comparing values of conductivity registered throughout the altitude-range 0 to 22 km during the flight of Explorer II [3] with values calculated from the intensity of cosmic radiation (data of Regener and that of Fig. 1 for geomagnetic latitude 50° north) registered at corresponding levels. Further justification of the method is considered in the following paragraphs.

Factors which determine the conductivity--When small positive ions of concentration n_1 and mobility k_1 and small negative ions of concentration n_2 and mobility k_2 only are present in a gas, the total conductivity (Λ) is expressed by the relation $\Lambda = e(n_1k_1 + n_2k_2)$ where e is the electronic charge. In the atmosphere the contribution from ions of types other than the small ions is generally found to be negligible. It may be assumed that $n_1 = n_2 = n$ without incurring an appreciable error. Then writing $k = k_1 + k_2$, the conductivity is $\Lambda = ekn$. The calculation of conductivity then involves calculations of the concentration of small ions and of their mobility in the required range of altitudes.

The concentration of small ions depends upon the rate at which they are produced and that at which they disappear. The small ion disappears, or loses its identity as a small ion, when it recombines with another small ion having an opposite charge or when it attaches to larger charged or uncharged particles, which in most of the Earth's atmosphere consist largely of condensation-nuclei. The latter cannot be considered in these calculations because observations of the distribution of nuclei at sea are inadequate. At the surface the average concentration is apparently less than 1000 per cc but the vertical distribution has not been investigated. Indeed even if the latter were known, the effect on the concentration of small ions could be estimated only for pressures close to one atmosphere since it is not known how the coefficients for the attachment of small ions to nuclei vary with pressure. Near the surface at sea the average rate of destruction of small ions by attachment to nuclei is nearly three times that which results from the recombination of the small ions. When this effect of nuclei is neglected the calculated conductivity is nearly twice what it should be, but the calculated value of resistance is doubtless affected to a considerably less extent because the concentration of nuclei certainly decreases with altitude. Furthermore, since it is the purpose here to compare the total resistance at two different latitudes, proportional errors in the absolute values do not come into account. The errors from that source are doubtless nearly proportional to the absolute values of resistance if the distribution of nuclei, both horizontally and vertically, is not dependent upon latitude. Although direct observations of nuclei are not adequate to determine the character of their distribution, yet, since the numerous measurements of air-conductivity at sea show no dependence upon latitude, it is to be inferred that the content of nuclei near the surface is independent of latitude. The factors which affect the vertical distribution are thought to be nearly independent of latitude.

The assumption that the cosmic radiation is the predominant ionizing agent in the atmosphere above oceanic areas, from the surface up to considerable heights, must be admitted because measurements of the content of radioactive matter in the air at sea show that the ionization from that source is only a few per cent of that attributable to cosmic radiation. Such then is the justification for assuming that over the oceans up to the maximum heights to be considered small ions are produced by cosmic radiation alone and are destroyed only by recombination with other small ions.

For these conditions the number of pairs of small ions per cc (n) at a given altitude for equilibrium-conditions is given by the expression $n = \sqrt{q/\alpha}$ where q is the number of ion-pairs formed per cc per sec, and α is the coefficient of recombination at the appropriate temperature and pressure. Expressing the measure of cosmic-ray intensity by I--the number of ion-pairs formed each second in air of standard density (δ_0)--then q, at a corresponding point, is directly proportional to I and to the density (δ) of the air, that is, $q = \delta I/\delta_0 = pT_0 I/(p_0 T)$, where p is pressure and T is absolute temperature. The subscript "o" is used throughout to indicate a value at standard temperature and pressure.

The dependence of α upon pressure in the range with which these calculations are concerned, is not definitely known. According to the usual interpretation of Thomson's theory of recombination, α varies directly as the pressure. However, investigations in the laboratory so far yield diverse results, some indicating that $\alpha \propto p$, others that $\alpha \propto p^x$, where $0 \overset{=}{<} x < 1$. A comparison [3] of values of air-conductivity measured on the flight of the stratosphere balloon Explorer II, with values of cosmic-ray intensity, led to the conclusion that x is less than unity. That conclusion is supported by recent investigations by Gardner [8] which indicate that in oxygen α does not vary as the first power of the pressure in the pressure-range which has to be considered here, but that $\alpha \propto p^x$ is a close approximation for x considerably less than unity. Investigations in the laboratory indicate that α varies inversely as the 7/3 power of the temperature. It therefore seems admissible to write: $\alpha = \alpha_0(p/p_0)^x(T_0/T)^{7/3}$. The calculations reported here have been made for two values of x, namely, 1 and 0.3.

Thus far only those factors which affect the concentration of small ions have been considered. In order to calculate the conductivity the variation of mobility with altitude must also be taken into account. For the portion of the atmosphere which is to be considered, the mobility (k) of the positive as well as that of the negative ion may, with considerable confidence, be taken to vary inversely as the density of the air, that is, $k = k_0\delta_0/\delta = k_0 p_0 T/(pT_0)$. If it is assumed that positive and negative ions are equally abundant, it follows that the conductivity, $\Lambda = nek = (ek_0/\alpha_0^{1/2})(p_0/p)^{(x+1)/2}(T/T_0)^{5/3}I^{1/2}$, where k represents the sum of the mobilities of the positive and negative ions. The following values of the constants were used: $e = 4.77 \times 10^{-10}$, $\alpha_0 = 1.6 \times 10^{-6}$, and $k_0 = 900$ cm^2 Ves^{-1} sec^{-1}.

Method of calculation--Calculations of the conductivity for a range of altitudes have been made for a place near the equator and for one at about magnetic latitude 50° north. The required

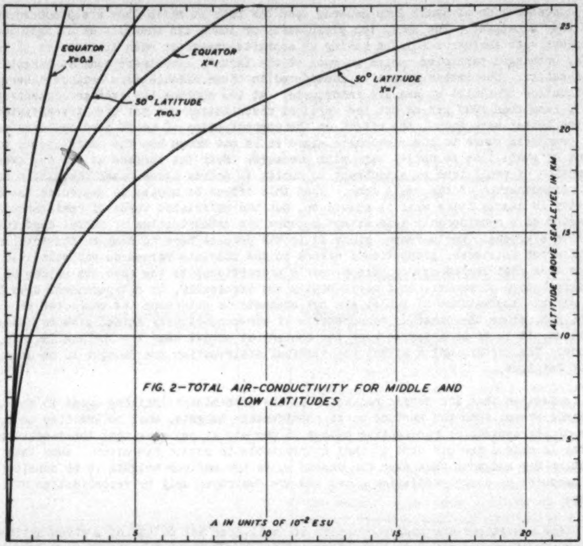

FIG. 2—TOTAL AIR-CONDUCTIVITY FOR MIDDLE AND LOW LATITUDES

values of temperature were taken from the Smithsonian Physical Tables [4]. Those for summer in the Northern Hemisphere were used because the observations, with which these calculations are to be compared, were generally made in temperate latitudes during the summer half of the year. The pressure at a given height was calculated from the relation $h = 19.413/(288T_m \log_{10}p_0/p)$, where h is the height in kilometers and T_m is the mean temperature of a column of air to the height considered [5]. Calculations have been made for the altitude-range 0 to 26 km above sea-level. In order to include greater heights for latitude 50° it would be necessary to extrapolate the cosmic-ray-data. But the region above 26 km probably contributes a negligible amount to the total resistance. This opinion is based on the following considerations, which are also of interest apart from this bearing.

The measurements obtained at Madras (India), Omaha (Nebraska), and Saskatoon (Canada), together with others obtained at Washington [6] show that at both high and low latitudes a decrease in I sets in at a certain height (18 km at Madras, 24 km at Omaha) and that at greater altitudes I varies nearly directly as the pressure. A recent analysis [7] indicates that near the top of the atmosphere I may be expressed by the relation: $I = I_e + Ap/p_0$ where I_e is the value of I extrapolated to zero-pressure and A is a constant. For such a variation of I the conductivity will continue to increase with height due to the pressure-term, for any value of x from zero to unity. In other words, in spite of the decrease of I with height above a certain point, the conductivity continues to increase.

It follows that a decrease of conductivity such as was observed above the 19-km level on the flight of Explorer II [3] must be attributed to other factors. Constituents in the atmosphere which form large, relatively immobile ions, through combination with small ions, would have such an effect. Such constituents, if generally distributed in sufficient quantity in the high atmosphere to affect the present calculations, would doubtless produce optical effects which would have led to their detection.

It is to be noted that if x is zero the increase of conductivity with height at these high levels would be small until the I_e- term becomes important after which the variation in conductivity would approach that of the inverse square root of the pressure. If x is taken equal to zero, the upper regions would contribute appreciably to the calculated resistance, provided the

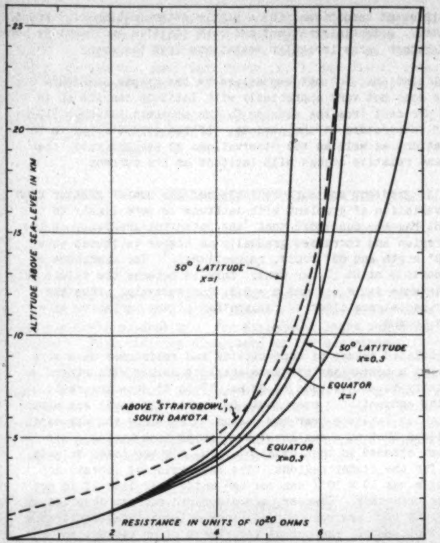

FIG. 3—RESISTANCE OF AIR-COLUMN ONE SQUARE CENTIMETER IN CROSS-SECTION BETWEEN EARTH AND POINTS IN ATMOSPHERE

integration is extended to considerable heights. However, such a contingency need scarcely be considered because there is considerable evidence that the conductivity does increase quite rapidly with height from say 60 km upward. It is also indicated in recent experiments with oxygen [8] that at pressures corresponding to such altitudes x is greater than at lower levels. Actually there are other considerations which limit the heights to which the integration need be extended. At the point where ionization due to ultraviolet light becomes important, or where the contribution of free electrons to the conductivity is pronounced, the conductivity is so great that no further contributions are made to the columnar resistance. It is therefore practically certain that most of the resistance is offered by the region below 26 km.

Values of conductivity--The results of the calculations of conductivity at the two latitudes are shown in Figure 2. Two graphs are shown for each latitude--one for the assumption x = 1, the other for x = 0.3. The latter was chosen because it lies between the value 0.28 previously reported [3] as required to reconcile the values of conductivity measured on the flight of Explorer II with Regener's cosmic-radiation data, and the value 0.33, which is required for reconciliation of those conductivity-data with cosmic-ray data obtained at magnetic latitude 50° north at Omaha, Nebraska. The curves in Figure 2 show that there should be considerable difference in conductivity between a high- and a low-latitude station even for low altitudes and that differences at a given latitude depend markedly on the value of x. This indicates the need for further determinations of the value of α under such conditions as are found in the Earth's atmosphere, either by suitable experiments in the laboratory or by further exploration in the atmosphere. Both approaches may be required to determine the influence on the ionization of all factors involved.

Values of columnar resistance--The results of integrating the reciprocal of the conductivity from the surface upward to obtain the total resistance between the Earth and a point in the atmosphere for a column of air of 1 cm^2 cross-section are shown in Figure 3. It may be seen that above a few km the resistance is greater at the equator than at the northern latitude. The difference is more pronounced for small values of x, amounting to 20 per cent for x = 0.3 and to 15 per cent for x = 1, at the 26-km height. For the purposes of the following discussion the actual value of x is not of great importance because at a given height the estimate of total resistance is much less dependent upon the value assumed for x than is the conductivity.

Comparison with observations--If there are real differences in the columnar resistance from place to place and if the air-earth current is constant with height, there should be a corresponding difference in the current since the potential of the atmosphere at great heights must be practically the same at all latitudes.

The data obtained on cruises IV and VI of the Carnegie [9] have been reexamined for evidence of a latitude-effect in the air-earth current. An increase in current of as much as 20 per cent from the equator northward is not definitely excluded, but the deviation of individual determinations from the mean is so great that one must agree with Mauchly who, in summarizing these data, concludes that there is "no evidence of a well-established difference between the current-densities as observed in different latitudes." These data are inadequate for this comparison for the following reasons: (a) Less than 500 determinations of current were made throughout the entire latitude-range and those under quite different conditions; (b) a single determination of current involves three observed quantities, namely, potential-gradient and both positive and negative conductivity, each of which is subject to rather large irregular deviations from the mean.

A feature brought out by these calculations, but not emphasized by the graphs in Figure 2, is that the conductivity at the surface does not vary appreciably with latitude despite an increase of cosmic radiation of about 12 per cent from the equator to the northern latitude [10]. This is the result of the difference in temperature at the surface. If the conductivity is independent of latitude, as these calculations as well as the observations at sea indicate, then the gradient would be subject to the same relative change with latitude as the current.

Since the individual observations of gradient are more reliable and the number greater than those of current, any indication of a variation of gradient with latitude is more likely to be significant. In respect to this element Mauchly concluded that "the potential-gradient, on the average, was lowest in the equatorial region and increased gradually as higher latitudes were reached, at least up to parallels of 60° north and 60° south, respectively." The magnitude of this increase from the equator to 50° north is about 17 per cent. This is between the values calculated for the change in columnar resistance for $x = 1$ and $x = 0.3$, respectively. Thus the results of the calculations of columnar resistance offer an explanation of the variation with latitude of potential-gradient over the oceans.

Although the variations in the calculated values of conductivity and resistance have more significance than the absolute values, yet a comparison of these absolute values with others deduced from observations of air-conductivity to an altitude of 22 km [3] is of some interest. The resistance found by extrapolating the conductivity over South Dakota to sea-level was about 1.7 times that calculated here for a similar latitude over the oceans. Therefore the air-earth current over the oceans should be 1.7 times that at a sea-level station on land having a distribution of conductivity similar to that assumed in that extrapolation, for the lower heights, and to that observed over South Dakota for the higher regions. The mean value of current actually observed on cruises of the Carnegie was 10×10^{-7} esu per cm^2 while over land it is not uncommon to find values 40 per cent less than that. However, the air-earth current observed at the ground-station in South Dakota was 14×10^{-7} esu per cm^2 [11] indicating a total resistance above that station less than that over the oceans. The actual resistance of an air-column over South Dakota would be less than that indicated in the previous calculation for a sea-level station, because (a) the conductivity at the surface was greater there than that used in calculating the resistance, and (b) that station was 1200 meters above sea-level so that the considerable resistance offered by a column extending from sea-level to that height should not be included. The results obtained by extrapolating the upper-air observations to the value found at the surface station are indicated in Figure 3. It will be seen that the total resistance calculated in this way is considerably reduced, the value being nearly equal to that obtained over the oceans. Part of the remaining discrepancy is doubtless attributable to the fact that the conductivity calculated from the cosmic-radiation data is higher than the actual values. For example, if the graph of those calculated values is extrapolated from two km to the value 2.6×10^{-4} esu--the mean of values observed over the oceans on cruises IV to VI of the Carnegie--the total resistance is increased by a factor of approximately 1.3. This brings the resistance calculated over South Dakota and that over the oceans in quite satisfactory agreement with the respective values of observed current. It would require about 1000 nuclei per cc at the surface of the oceans to make the value of q used in these calculations agree with the observed conductivity. The average measurements of concentration of nuclei at sea is close to that value. In addition to allowing for the presence of nuclei, there is the remaining possibility that the observed quantity I should have been reduced throughout these calculations by a constant multiplying factor which would have increased the value calculated for the total resistance. The observers responsible for these data apparently do not stress the accuracy of the absolute magnitude of I. A number of observers report lower values at the surface than those used here. This comparison of the columnar resistance calculated from cosmic-radiation data with that derived from observations of air-conductivity, up to an altitude of 22 km, therefore reveals no disturbing disparity.

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IONOSPHERIC DISTURBANCES COINCIDENT WITH MAGNETIC STORMS

L. V. Berkner

(Abstract)

During magnetic storms disturbances have been observed in the higher of the two main ionized regions of the Earth's upper atmosphere. The disturbance consists primarily of a reduction of the maximum electron-density. For a magnetic storm beginning at night the echoes are in addition blurred. This blurring disappears as the sun rises.

Figures 1 and 2 compare ionospheric records made near Washington, D. C., during the magnetic storm of December 23, 1937, with those made during the previous day which was magnetically undisturbed. Each individual record takes 15 minutes to make and is an automatic plot of the variation in delay-time of ionospheric echoes with the vibration-frequency of the exploring wave. The maximum electron-density in the ionosphere is proportional to the square of the maximum wave-frequency for which steady echoes are returned.

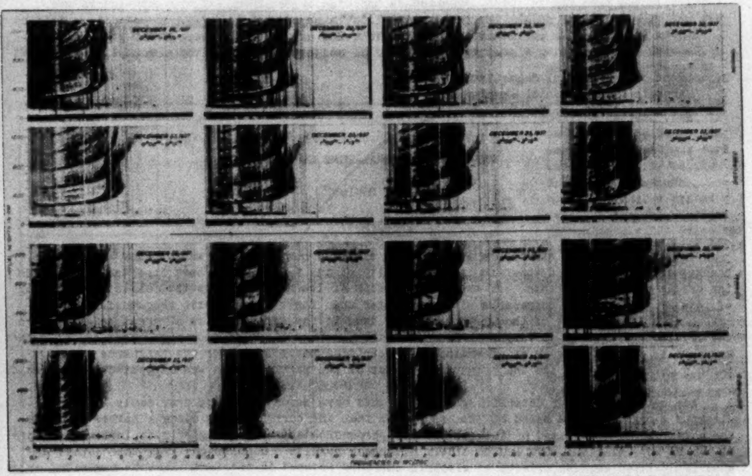

FIG. 1—IONOSPHERIC RECORDS, KENSINGTON EXPERIMENTAL STATION, DURING MAGNETICALLY UNDISTURBED AND MODERATELY DISTURBED CONDITIONS ON WINTER NIGHT
[SHOWING DEVELOPMENT AND EFFECT OF DISTURBANCE WHICH BEGAN ON DECEMBER 23 ABOUT 3^h, 75° WEST MERIDIAN TIME]

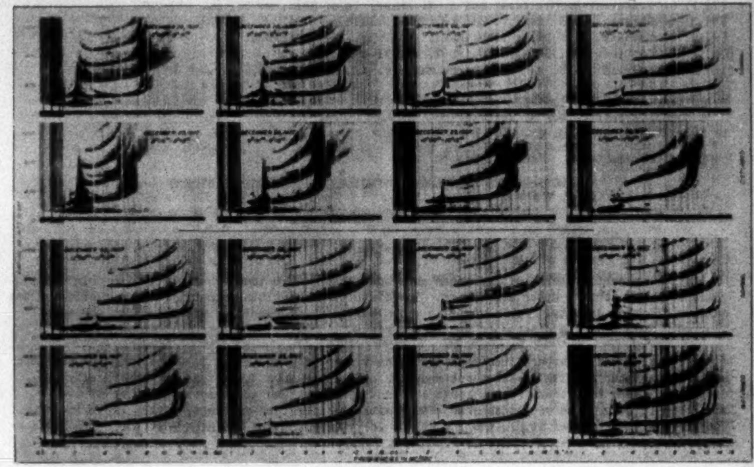

FIG. 2 — IONOSPHERIC RECORDS, KENSINGTON EXPERIMENTAL STATION, DURING MAGNETICALLY UNDISTURBED AND MODERATELY DISTURBED CONDITIONS ON WINTER DAY

(SHOWING DECREASING EFFECT OF DISTURBANCE AS DAYLIGHT CONDITIONS SET IN (DISTURBANCE BEGAN IN EARLY MORNING OF DECEMBER 23 ABOUT 3^h, 75° WEST MERIDIAN TIME))

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HEIGHTS OF AURORAL-ZONE CURRENTS

A. G. McNish

(Abstract)

The heights in the atmosphere at which the electric currents flow which cause the violent changes in magnetic field observed in the vicinity of the auroral zone have important bearing on the origin of those currents. If the currents flow at low heights where the collision-frequency of ions or electrons is comparable with or greater than their gyromagnetic frequencies dynamo-action is a likely cause of them. On the other hand if the currents flow at great heights where the collision-frequency is very much less than the gyromagnetic frequency some other cause of the currents is indicated, such as streaming of electrified particles under the influence of the Earth's permanent field and the original momentum of the particles, as was suggested by Birkeland.

Adequate methods for determining these heights have never been used previously. Several attempts along this line based on the assumption that the currents flow along a narrow path and consequently may be located by intersection of perpendiculars to the magnetic disturbance-vectors at two or more stations have led to widely divergent results. Conclusions from this method are open to question because effects of electric currents induced in the Earth must be neglected or their values assumed. No method for separating the internal and external effects has ever before been applied, the method of spherical harmonic analysis not being applicable because of the very restricted region where the magnetic effects are observed.

In this paper a method is described for separating the internal and external portions of the magnetic disturbance by use of solutions of Laplace's equation in Cartesian coordinates. By evaluating the current-function given by the external harmonics for various heights the upper limit of the currents is inferred. Heights where the harmonics of higher degree become comparable

in importance with those of lower degree are regarded as the upper limits. By comparison of computed distributions of current with hypothetical distributions, opinion is reached regarding the actual configuration.

The method has been applied to a magnetic "bay" observed at a number of stations at 21^h 45^m GMT, January 15, 1933. The upper limit of the current set for this case was slightly over 150 km while the most distribution and height was considered to be a broad flow of current, several hundred km wide, at a height not much greater than 100 km. A full account of this investigation may be found in the Journal of Terrestrial Magnetism and Atmospheric Electricity [v. 43, pp. 67-75, 1938].

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RECENT PROGRESS IN THE STUDY OF COSMIC CORRELATION WITH RADIO RECEPTION AT THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY

Harlan T. Stetson

Nightly measurements of field-intensity of WBBM Chicago received in the vicinity of Boston begun in 1926 by Pickard and later continued by Stetson and by Kenrick have been resumed in connection with the program of cosmic terrestrial research at the Massachusetts Institute of Technology. With but slight interruption, the series therefore comprises a full sunspot-cycle and so far as is known, is the longest systematic record of field-strengths on broadcast frequencies. The location of the receiving recorder is at present in Waban, Massachusetts, about three miles west of the original location of Mr. Pickard's laboratory in 1926.

The experience with the WBBM-series obtained at Delaware, Ohio, showed that before correspondence with the sunspot-cycle could be effectively studied, allowance should be made for the seasonal effect introduced by the longer hours of daylight of summer as compared with winter. From 8192 half-hourly periods of observation in the present series, a curve has been deduced showing the increase of field-intensity with the depression of the sun below the horizon (Fig. 1). In the early part of the series, observations were confined chiefly to the 21^h to 22^h, Eastern Meridian Time, a period at which the Sun averaged approximately 30° below the horizon at Chicago at the time of the equinox. This curve, therefore, has been used to correct field-intensities at other hours and at other dates to the mean value shown by the curve for a negative altitude of the Sun of 30°. This adopted standard is purely arbitrary but serves as a fiducial reference for eliminating the chief effect due to twilight, which is especially appreciable in the summer.

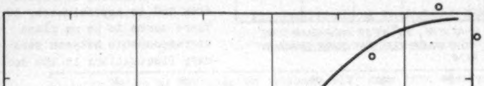

FIG. 1—VARIATION OF FIELD-STRENGTH WITH DEPRESSION OF SUN BELOW HORIZON, WBBM-RECORD, WABAN, MASSACHUSETTS [BASED ON 8192 OBSERVATIONS (HALF-HOUR PERIODS); EACH POINT IS MEAN FOR 10°]

All values of field-intensity, therefore, made prior to the time that the Sun has attained a negative altitude of 30° are corrected by the addition of a per-

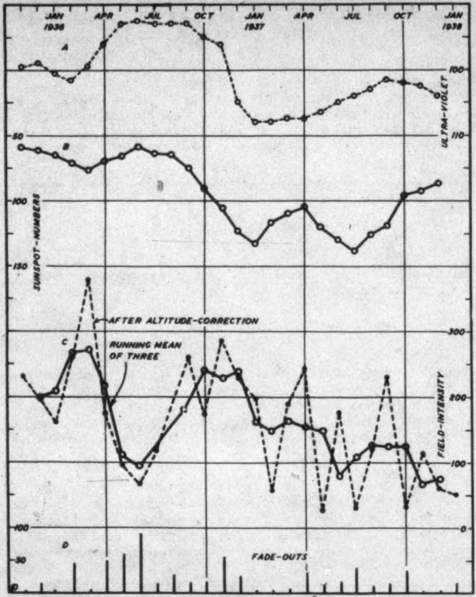

FIG. 2—RADIO FIELD-STRENGTH, 9-10 P.M., 75° WEST MERIDIAN TIME WBBM, WABAN, MASSACHUSETTS, NOVEMBER 1935 THROUGH JANUARY 1938

centage-increment that would bring the mean value of the evening to the adopted level. Field-intensity measures at greater depressions of solar altitude than -30° are similarly reduced by a percentage-correction to bring the average back to the adopted level.

In curve C (Fig. 2) there is represented the relative field-intensity measurements from November 1935 to December 1937, after these corrections have been made. The broken-line curve represents the monthly values of the hour 21 to 22 where the vertical scale marks microvolts at the receiver. The full-line curve represents the running mean of three successive months. Curve B is the inverted curve of sunspot-numbers during the same interval. It will be observed that the trend of the field-intensity graph and that of sunspots shows in general an inverse relationship. While sunspot-numbers were increasing from around 60 to 125, field-intensities decreased from 250 to approximately 50. There seems to be no close correspondence between secondary fluctuations in the sunspot-curve and those shown in field-intensities.

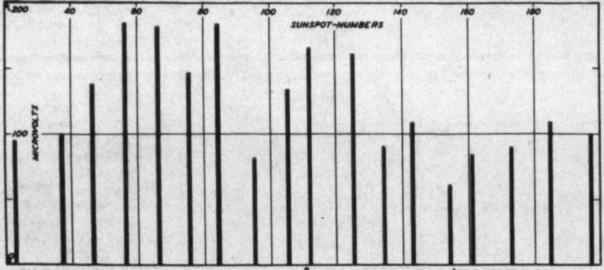

FIG. 3—RADIO FIELD-INTENSITY, 9-10 P.M., 75° WEST MERIDIAN TIME, (CORRECTED FOR ALTITUDE), WBBM, WABAN, MASSACHUSETTS, NOVEMBER 1935 THROUGH JANUARY 1938

In curve A there are represented the values of ultra-violet light as determined at the Mount Wilson Observatory. This curve is again inverted to show a correspondence with the sunspot-curve. It is observed that a drop in sunspot-numbers from March 1936 to June 1936 was accompanied by a marked fall in the intensity of ultra-violet. This period was followed by a marked increase in sunspot-numbers and in the intensity of the ultra-violet lasting until January 1937. During most of this interval, field-intensities showed a direct response both to the sunspot-curve and to that of the ultra-violet. It is perhaps worth remarking that the low values of radio reception during May, June, and July 1936 coincided with the conspicuous dip to low values in the ultra-violet-light curve. This was also the period of marked fade-outs recorded by Dellinger at the Bureau of Standards as shown in Graph D. As has often been emphasized, however, it appears that these high-frequency fade-outs are produced by sudden bursts of ultra-violet-emanation from the Sun quite different from the mean distribution of ultra-violet obtained from the monthly values.

In Figure 3 an attempt has been made to depict such relation as may exist between sunspot-numbers and field-intensities. This graph is based on 543 hours of observation confined to measurements during the hour 21-22, extending from November 1935 through January 1938. Sunspot-numbers are represented as abscissae and relative field-strengths as ordinates. The graph suggests that there is a tendency for an increase of field-intensity with sunspot-numbers until the latter attain an optimum value of 80-100. Thereafter increased solar activity as represented by larger sunspot-numbers appears to favor decreased field-strengths in the receiver. This suggests that the absorption in the E-layer may depend upon more than one critical value of electron-density. It appears that this may be one reason for the complexity of the problem of day-to-day relationships between solar activity and field-strengths. When yearly values of solar activity, magnetic phenomena, and radio reception are considered, graphs between these apparently related phenomena appear much more obvious.

It must be borne in mind that sunspot-numbers are at best probably but a crude indication of the active solar phenomena which are responsible for ionospheric disturbances. Some progress has been made in developing a solar index which involves the average area per group within a single penumbral disturbance.

Observers of terrestrial magnetic phenomena are often aware of disturbances accompanied by an unusually large sunspot which, standing by itself, produces by the conventional formula a relatively small sunspot-number. A large number of apparently insignificant spots, on the other hand, would give us a large sunspot-number even without the existence of any single large disturbance which might produce measurable terrestrial effects.

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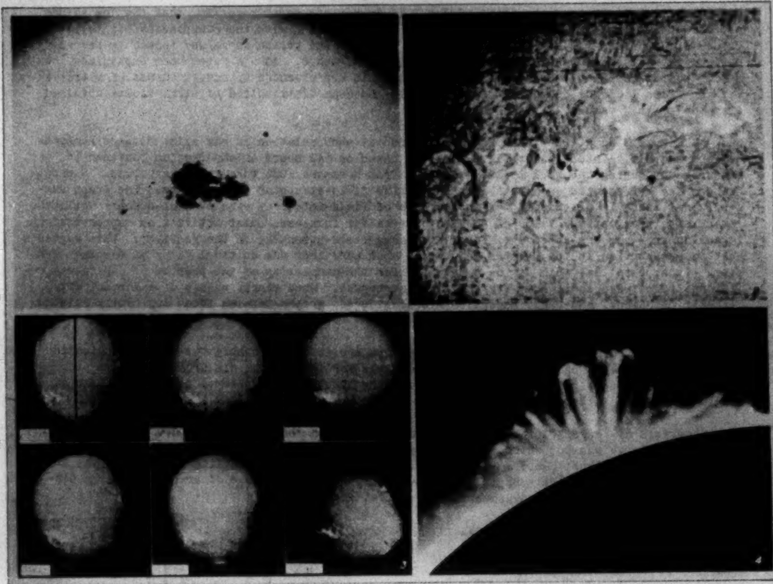
THE GREAT SUNSPOT OF JANUARY 1938

Seth B. Nicholson

A large group of sunspots in latitude +17° came into view at the east edge of the Sun on January 12, 1938. It was very complicated in structure (Fig. 1), consisting of a very irregular penumbra within which were several umbrae as well as areas of approximately photospheric brightness. The regions of opposite magnetic polarities were not separated into distinct spots as they are in a typical bipolar group. The regions of positive polarity were, however, in the north-preceding part of the group and those of negative magnetic polarity in the south-following part.

Three very great magnetic storms occurred between January 12 and 26. The first, January 16-18, began when the center of the group was 21° east of the central meridian; the second, January 21-22, began when the group was 42° west of the central meridian; and the third, January 25-26, began one day after the group had passed around the west limb of the Sun.

Cloudy weather seriously interfered with the observations of this group at Mount Wilson and our record of its activity is very incomplete. On January 16, at the time of the first magnetic storm, the hydrogen flocculi around the group were only of ordinary brightness (Fig. 2). A very large bright chromospheric eruption was photographed on January 20 at 18^h GMT, (Fig. 3), coincident with a major frequency radio fade-out. This chromospheric eruption and fade-out preceded the second magnetic storm by about 30 hours, although the most intense phase of the storm occurred about ten hours later. In 1931, Dr. Hale called attention to several instances when major magnetic storms followed large bright chromospheric eruptions by intervals ranging from 18 to 36 hours. The radio fade-outs reported on January 13, 14, 15, 18, and 19 were also probably caused by eruptions in this group although this is not certain since the Sun was not under observation here at the time of any of them. The fade-out of 18^h 38^m, January 15, which was one of the most pronounced of those mentioned, preceded the first storm by 29.5 hours. The fade-out of 15^h 18^m, January 13, which was also a pronounced one, was not followed within 36 hours by a magnetic disturbance.



FIGS. 1-4—PHOTOGRAPHS GREAT SUNSPOT OF JANUARY 1938, MOUNT WILSON OBSERVATORY: (1) SUNSPOT, JANUARY 18, 1938; (2) Hα GROUP, JANUARY 16, 1938; (3) CHROMOSPHERIC ERUPTION OF JANUARY 20, 1938; (4) K PROMINENCES, JANUARY 24, 1938

As the large spot-group passed around the west limb of the Sun on January 24, large fountain-like prominences such as often occur over active spots were photographed over it (Fig. 4). The third magnetic storm began about 20 hours after this photograph was taken, by which time the spot-group was completely out of sight. If this magnetic storm was also due to activity originating in the large group, as seems probable, it began at least 18 or 20 hours after the exciting energy left the Sun.

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MAGNETIZATION OF SEDIMENTS FROM THE BOTTOM OF THE ATLANTIC OCEAN

A. G. McNish and E. A. Johnson

(Abstract)

The direction of magnetization of sediments obtained from the bottom of the North Atlantic Ocean by the core-sampling apparatus developed by Dr. C. S. Piggot of the Geophysical Laboratory of the Carnegie Institution of Washington has been measured by the method described in the preceding abstract by E. A. Johnson. Distinct differences in declination and inclination were found at various depths in one core--the only one extensively examined. The results of these measurements are plotted in Figure 1.

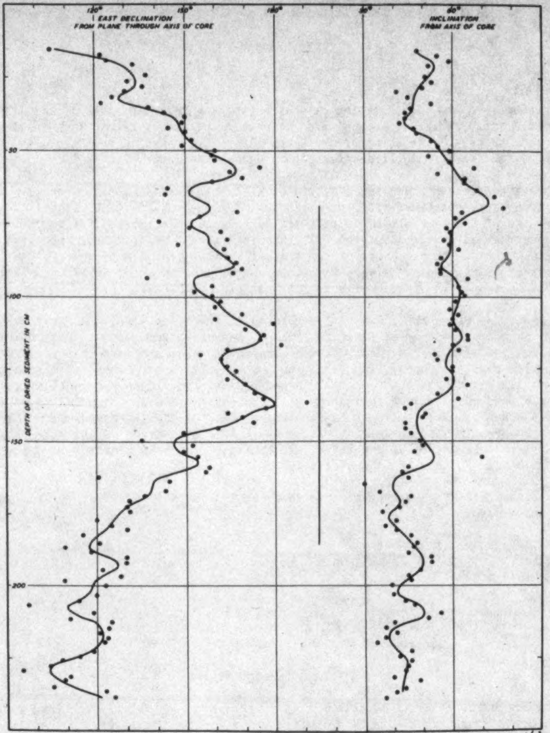

FIG. 1—DIRECTION OF MAGNETIZATION OF SEDIMENTS IN CORE NO. 3 [CURVE SMOOTHED BY FORMULA $(a+2b+3c+2d+e)/9$]

This core was taken at a depth of 2,575 fathoms in latitude 46° 03' north, longitude 43° 23' west. It consists of a material known as "blue mud" with some volcanic ash through its lower portion. Compared with most sedimentary materials, this mud proved to be highly magnetic.

While the conclusion that this magnetization represents the fossil magnetization of the sediments, acquired by action of the Earth's field in the process of deposition, is not warranted at the present stage of the research, there is considerable evidence in its favor. If subsequent investigation supports this belief, this method will enable a study of the history of the Earth's magnetic field in past geological ages--the only method whereby such studies can be made. Information of importance to other geological investigations may also be derived from the study which should furnish a means of dating sediments by their magnetic orientation.

A full account of these researches will appear in a future issue of the Journal of Terrestrial Magnetism and Atmospheric Electricity.

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Carnegie Institution of Washington,
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PRELIMINARY REPORT ON MEASUREMENT OF MAGNETIZATION OF OCEANIC SEDIMENTS

A. G. McNish and E. A. Johnson

(Abstract)

Measurements on the magnetization of sediments taken from the floor of the Atlantic Ocean are reported. These sediments are believed to retain the magnetization they acquired in formation and hence to reveal the state of the Earth's magnetic field in past geologic ages very much as fossils reveal the types of life existing on the Earth in the past.

Extensive measurements made on a sample of sediments extending nine feet into the floor of the North Atlantic Ocean taken with the apparatus developed by Dr. C. S. Piggot of the Geophysical Laboratory of the Carnegie Institution of Washington show marked differences in the direction of magnetization at various depths. If the interpretation which has been given these measurements is correct, the direction of the compass in that region was about 60° east of true north when some of the sediments were deposited several thousands of years ago. Today the direction of the compass in the same region is 30° west of true north.

Tiny particles of magnetic material deposited in the ocean tend to align themselves with the Earth's magnetic field as they settle through the water, very much as the ordinary mariner's compass aligns itself with the Earth's field. Coming to rest on the bottom they retain their positions and are covered up by later deposits. Thus they form a record of magnetism in past ages such as man might have obtained had he been making magnetic measurements at the time. Magnetic observations made many years ago show that considerable changes in the direction of the compass have taken place in the past few centuries, but the changes revealed by the oceanic sediments are much greater than those recorded by man. This new method of studying terrestrial magnetism in the past by measuring the fossil magnetization of rocks promises to shed light on the origin and history of the Earth's magnetic field.

Full account of this investigation is to be published in the Journal of Terrestrial Magnetism and Atmospheric Electricity.

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APPLICATION OF VERTICAL VARIOMETER-MEASUREMENTS TO THE STUDY OF SECULAR MAGNETIC VARIATIONS

Victor Vacquier

The advance of terrestrial magnetism as a science has been largely confined to a better understanding of the magnetic fluctuations generated by processes external to the lithosphere. Aside from the obvious fact that the Earth's magnetic field is locally disturbed by magnetic inhomogeneities of the crustal rocks, our knowledge of the Earth's permanent field is purely descriptive. The magnetic charts now available--useful as they are to the navigator and to the surveyor--have not provided magneticians with data adequate for the formulation of even a tentative hypothesis of the origin of the permanent field and of its secular variations. As the matter stands now, it is unlikely that the mere continuation of the present observational activity will yield facts of a new kind within the next 50 years. The study of secular magnetic variations in other ways than by repeating from time to time absolute magnetic measurements at widely scattered stations offers a good chance for achieving further progress.

Several investigators have indicated the possibility of tracing the attitude of the Earth's magnetic field in the past by ascertaining the direction of the remnant magnetization in rocks and baked clays [see 1 of "References" at end of paper]. Unfortunately their researches have aroused very little interest, so that at present the number of such data is too small to warrant far-reaching conclusions.

The purpose of this paper is to direct attention to another method of securing novel information in regard to secular variation. It consists of investigating the behavior of this phenomenon in selected regions of small extent in greater detail than is feasible in the case of world-wide surveys. The latter have given us the bold outlines of the secular magnetic changes all over the Earth. The maps available at present show large foci of abnormal secular variation

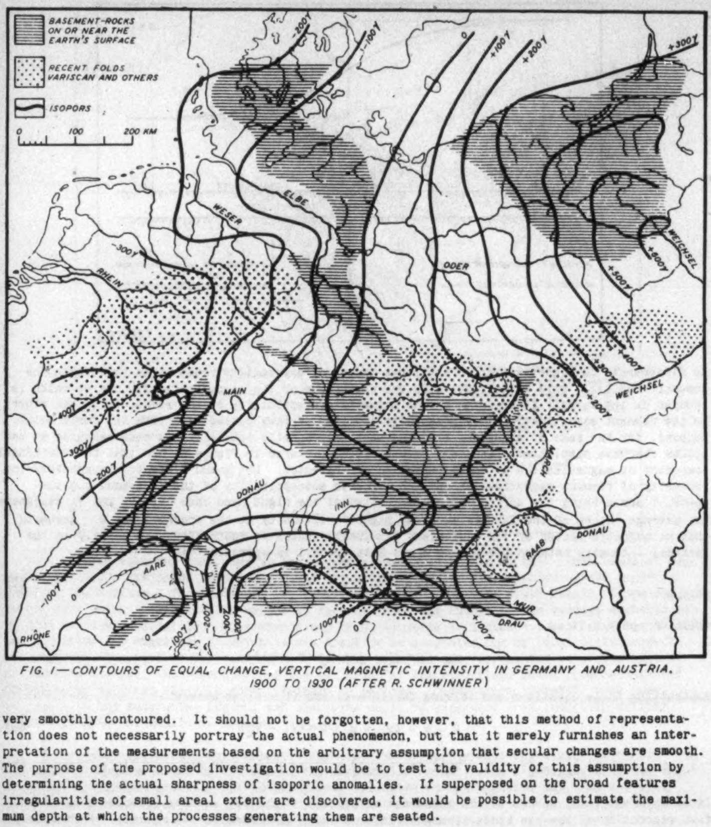
BASEMENT-ROCKS ON OR NEAR THE EARTH'S SURFACE
RECENT FOLDS VARISCAN AND OTHERS
ISOPORS
0
100
200 KM
-200γ
-100γ
0
+100γ
+200γ
+300γ
WESER
ELBE
ODER
WEICHSEL
+600γ
+500γ
-300γ
RHEIN
MAIN
-400γ
-300γ
-200γ
+400γ
+300γ
+200γ
WEICHSEL
MARCH
DONAU
INN
DONAU
RAAB
AARE
-100γ
0
RHÔNE
0
-100γ
-200γ
-300γ
-200γ
-100γ
0
+100γ
MUR
DRAU

FIG. 1—CONTOURS OF EQUAL CHANGE, VERTICAL MAGNETIC INTENSITY IN GERMANY AND AUSTRIA. 1900 TO 1930 (AFTER R. SCHWINNER)

very smoothly contoured. It should not be forgotten, however, that this method of representation does not necessarily portray the actual phenomenon, but that it merely furnishes an interpretation of the measurements based on the arbitrary assumption that secular changes are smooth. The purpose of the proposed investigation would be to test the validity of this assumption by determining the actual sharpness of isoporic anomalies. If superposed on the broad features irregularities of small areal extent are discovered, it would be possible to estimate the maximum depth at which the processes generating them are seated.

The existence of local anomalies in secular magnetic variations has been pointed out by H. Reich [2] and R. Schwinner [3]. A map showing the change of the vertical intensity in central Europe between the epochs of 1900 and 1930 compiled by R. Schwinner is reproduced in Figure 1. One cannot help being impressed by the sharpness of the anomalies appearing on this map, especially when one compares it with conventional isoporic charts. Unfortunately both Reich and Schwinner have attempted to interpret them in terms of geologic structure. Such vague speculations, many of which find their way into geophysical journals, serve only to incite prejudice against what significant material their authors happen to present.

Granting that closely-spaced data on secular magnetic variations would in all probability give us some insight into the nature of the phenomenon itself, it will now be pointed out that they could also be applied to geological investigations, provided that the measurements be interpreted on a physical basis. Instead of trying to correlate local features of secular variation directly with geologic structure, it seems more logical to see first whether they bear a quantitative relationship to those anomalies of the Earth's magnetic intensity which are ascribed

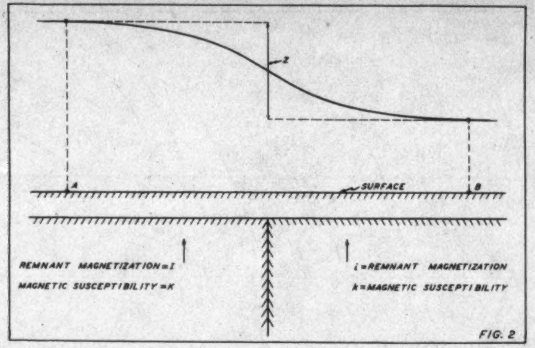

FIG. 2

to differences in magnetization of the large masses of crystalline rocks which constitute the basement-complex. This magnetization is the vector sum of two component parts, one of which is induced by the Earth's field owing to the magnetic susceptibility of the rocks, while the other is the remnant magnetization they presumably acquired as they cooled down from the Curie point. Suppose, for the sake of argument, that the vertical magnetic intensity has been measured at two epochs over the simple two-dimensional distribution pictured in Figure 2, and that the horizontal component of magnetization is small enough to be neglected. Let I and K denote respectively the intensity of remnant magnetization and the magnetic susceptibility of the left-hand quarter-space, i and k being the corresponding properties of the right-hand one. Let V_1 and V_2 designate the average values of the Earth's vertical magnetic intensity at the epochs 1 and 2. Assuming that no magnetic matter or its equivalent was locally added or subtracted, the anomaly in the vertical intensity between the points A and B at epoch 1 is approximately

$$z_1 = 2\pi[I - i + V_1 (K - k)] \tag{1}$$

while at epoch 2 it is

$$z_2 = 2\pi[I - i + V_2 (K - k)]$$

Subtracting these equations and solving for (K - k) and (I - i) we obtain

$$K - k = (z_1 - z_2)/2\pi(V_1 - V_2) \tag{2}$$

$$I - i = (1/2\pi)\left\{z_1 - [V_1/(V_1 - V_2)]\ (z_1 - z_2)\right\} = (1/2\pi)\left\{z_2 - [V_2/(V_1 - V_2)]\ (z_1 - z_2)\right\} \tag{3}$$

It is apparent that, thanks to the existence of secular variation, we have effected a quantitative separation of the two kinds of magnetizations which give rise to the anomaly. Fortunately the actual values of (K - k) and (I - i) are not any more significant than their ratio

$$R = (I - i)/(K - k) = z_1/(z_1 - z_2) - V_1/(V_1 - V_2) \tag{4}$$

Assuming $(z_1 - z_2)$ and $V_1 - V_2)$ are of the same sign, then when R is large, the anomaly changes little with time indicating that most of the magnetization contrast is remnant. When $R = 0$, the remnant-magnetization contrast is zero. When $R < 0$, the observed anomaly is the sum of the positive contribution proportional to V(K - k) and a negative one proportional to (I - i). Furthermore, when $V(K - k) > (I - i)$, $(z_1 - z_2)$ is of the same sign as $(V_1 - V_2)$; while when $V(K - k) < (I - i)$ the opposite is true, and we have the situation of an anomaly increasing in magnitude while the general field is decreasing. It may be noted that this does not necessarily mean that one of the rock-masses should possess a remnant magnetization in opposition to the present direction of the Earth's magnetic vector, but merely that the rock of higher susceptibility possesses a smaller remnant magnetization.

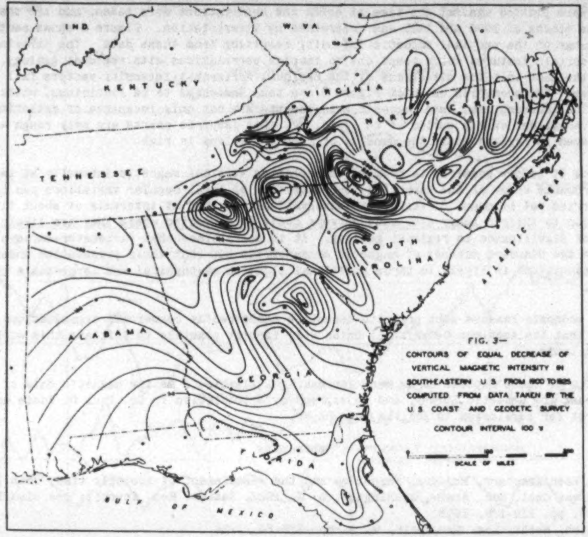

FIG. 3— CONTOURS OF EQUAL DECREASE OF VERTICAL MAGNETIC INTENSITY IN SOUTHEASTERN U.S. FROM 1900 TO 1925 COMPUTED FROM DATA TAKEN BY THE U.S. COAST AND GEODETIC SURVEY CONTOUR INTERVAL 100 γ

It is thus apparent that, due to the magnetic induction in the Earth's crust, secular magnetic variations are bound to produce minor disturbances in the distribution of the magnetic field at the surface. These effects are indistinguishable from what would result if new magnetic matter were being formed locally except in regions where the change in the average magnetic intensity had been negligible. Schwinner's map (Fig. 1) seems to indicate local features in secular variation the magnitude of which is too great to be accounted for by local differences in the magnetic induction of basement-rocks. If they are real and representative of the rest of the Earth's surface, the separation of the two kinds of magnetizations which produce magnetic anomalies as outlined above is more difficult to perform. Chances are, however, that they originate in the subcrustal regions and therefore could be separated from induction-effects which take place at shallower depths.

It is generally agreed that magnetic anomalies arise mainly from the distribution and from the state of magnetization of the underlying crystalline rocks. When a geologic trend is characterized by igneous activity, it is definitely reflected on the magnetic map. On the other hand, magnetic trends do not necessarily correspond to known geologic trends, since some of them may arise from rock-distributions antedating the era of sedimentation. One of the difficulties in the geologic interpretation of many magnetic surveys lies in the fact that these meaningless trends complicate the picture to such an extent that the useful ones are difficult to follow. Although in interpreting the observed quantity R we have neglected the contribution of the horizontal components of rock-magnetization, this does not necessarily invalidate its possible diagnostic value for segregating magnetic anomalies into groups, thus bringing order into an apparent chaos. In this connection it should be noted that J. G. Koenigsberger [1] has observed that in general the magnitude of the residual magnetization of igneous rocks is inversely proportional to their age.

To carry out detailed surveys of secular magnetic variation within a reasonable time and with accuracy sufficient to measure the quantity R discussed above, variometer-measurements have to be used. Although it was almost certain that the available magnetic data would be inadequate, an attempt was made to corroborate the results of Reich and Schwinner by studying in detail the magnetic observations of the United States Coast and Geodetic Survey. The north, the east, and the vertical components of the magnetic intensity were computed for every observation at repeat-stations located in the southeastern part of the United States. The values pertaining to each station were plotted against the time at which the observations were taken, and the change between the epochs of 1900 and 1925 was determined by interpolation. Figure 3 shows contours of equal change of the vertical magnetic intensity resulting from these data. The striking alignment of certain features would tempt one to imagine correlations with regional geology, were it not for the fact that the directions of the residual horizontal-intensity vectors fail to substantiate them. The anomalies shown on Figure 3 are thus indicated to be ficticious, which demonstrates that conventional land-magnetic measurements are not only incapable of detecting local secular variations over a period of 25 years, but that isoporic charts are only rough approximations even in regions where the density of repeat-stations is high.

Since it is now possible to compare the relative vertical magnetic intensity at two points with a probable error of one gamma [4], the detection of local secular variations can be economically carried out by means of variometer-observations repeated at intervals of about five years. In addition to their primary aim, such surveys could be conducted where they are likely to yield results of significance to regional geology. It is not claimed that variometer-surveys should supersede the standard methods of magnetic measurements but that their prosecution under controlled conditions is likely to throw additional light on geophysical and large-scale geological problems.

For economic reasons such projects cannot be sponsored by commercial organizations, and it is hoped that the American Geophysical Union will find it possible to initiate this work on a practical basis.

I wish to thank Captain N. H. Heck for making available to me the magnetic data of the United States Coast and Geodetic Survey, and to express my appreciation to Dr. Paul D. Foote and Dr. E. A. Eckhardt for permission to publish this paper.

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[2] H. Reich, Beitr. ang. Geophysik, v. 4, pp. 373-84, 1934.
[3] R. Schwinner, Beitr. Geophysik, v. 48, pp. 388-416, 1936.
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NOTES ON HORIZONTAL-PENDULUM OBSERVATIONS IN RELATION TO CERTAIN PHENOMENA

F. Napier Denison

The writer has been interested in the above subject for a number of years, and recently has published some of his results up to 1936 in the January Bulletin of the Seismological Society, and further notes were read at the Annual Meeting of the above Society which was held early in April, 1938, in California.

It is hoped the following information in connection with this subject may prove of some value to those attending the meeting of the American Geophysical Union.

As temperature-changes frequently affect recorded pendulum-movements, it is well to state that both the north-south and east-west horizontal pendulums from which the following observations have been obtained are mounted on the same concrete pier in the basement of the Gonzales Observatory which is well situated on an ideal rock-foundation. The temperature of the pendulum-room is kept remarkably steady by means of special ventilation and massive concrete walls.

In Figure 1 the lowest curve represents the mean temperature of the outside air at the Gonzales Observatory from 1927 to 1937. Above this is shown the mean temperature of the pendulum-room (1928-37), which is derived from daily observations taken at 10^h. This curve shows an annual range of about 10° F, and to a large extent parallels the air-temperature curve in form, but one-half the amount in amplitude.

Above these curves are shown the monthly mean values of both the east-west and north-south horizontal pendulums from 1927 to 1937. As pointed out in the January Seismological Bulletin,

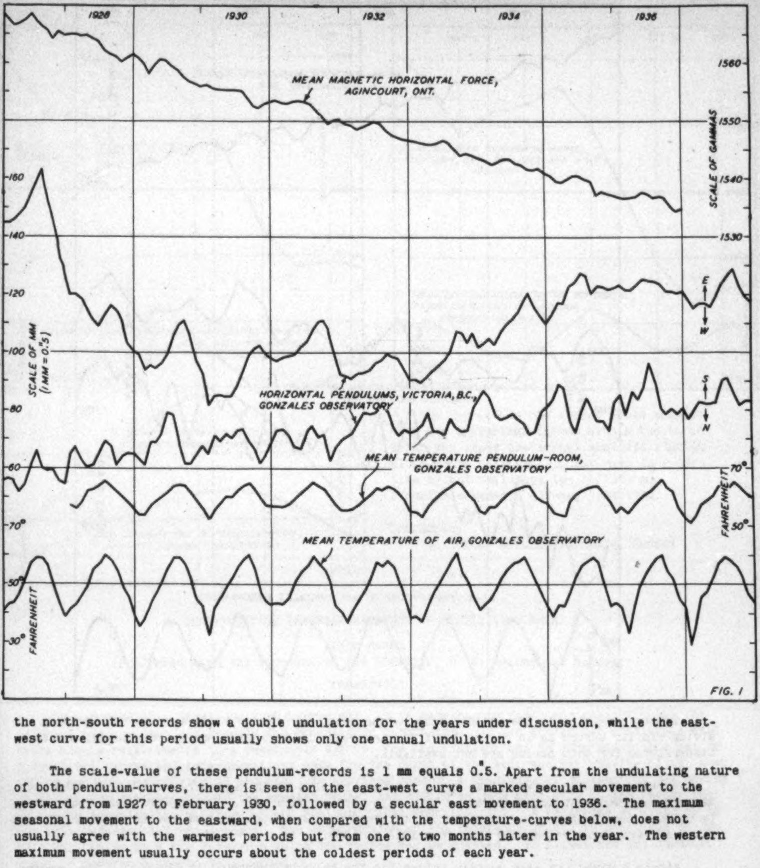

FIG. 1

the north-south records show a double undulation for the years under discussion, while the east-west curve for this period usually shows only one annual undulation.

The scale-value of these pendulum-records is 1 mm equals 0".5. Apart from the undulating nature of both pendulum-curves, there is seen on the east-west curve a marked secular movement to the westward from 1927 to February 1930, followed by a secular east movement to 1936. The maximum seasonal movement to the eastward, when compared with the temperature-curves below, does not usually agree with the warmest periods but from one to two months later in the year. The western maximum movement usually occurs about the coldest periods of each year.

In the case of the north-south pendulum-curve we find a marked secular movement to the southward from 1927 to 1936 of over ten seconds of arc. The double annual undulations marked on this curve are of considerable interest, for though one maximum southerly movement occurs about the time of the summer solstice, the northerly maximum movement does not agree with the coldest period of each year, but usually falls much earlier, and frequently in September or October.

These and the other undulations are shown in Figure 3 as monthly mean values for that period.

Thinking these north-south pendulum-undulations might in some way be related to terrestrial magnetic changes the upper curve of Figure 1 is shown giving the monthly mean magnetic horizontal force results for Agincourt, Ontario, from 1927 to 1936. This curve clearly shows an annual double undulation, and comparing it with the north-south curve below, one finds some correspondence between these curves.

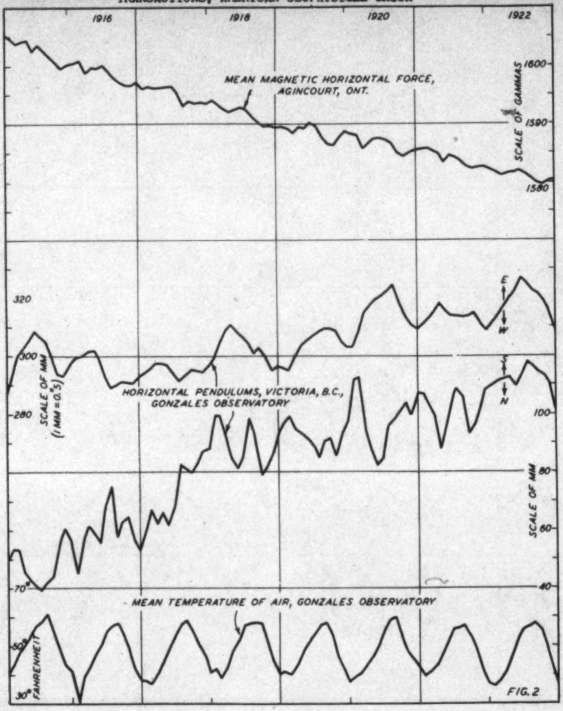

FIG. 2

Before referring to the monthly mean values shown on Figures 3 and 4, Figure 2 is shown giving similar curves as in Figure 1 but for the years 1915 to 1922, except the pendulum-room temperatures for this period are not available.

A study of these curves gives about the same results as in Figure 1, except that both the maximum east and west movements occur as a rule earlier each year. Throughout these years there is noted a resultant secular movement or "tilting" to the southeast, and the north-south fluctuations are even more pronounced than from 1927 to 1937.

Figure 3 gives the mean monthly values for the records discussed in Figure 1. The temperature-curves both for the pendulum-room and the outside air parallel one another and represent the normal seasonal change. The north-south curve clearly indicates one northerly maximum movement in April, followed by a marked southerly movement to July of about three seconds of arc, then a second northerly movement sets in reaching a maximum of about four seconds of arc in September and October; then the movement is again towards the eastward.

The Agincourt mean magnetic horizontal-force curve, for this period clearly indicates the secular magnetic decrease and a marked double annual fluctuation. The times of the maximum magnetic increase appear to occur about the times of the solar and winter solstices, and the periods of decreasing magnetic intensity are greatest about the vernal and autumnal equinoxes.

In Figure 4 the mean monthly results are given for the 18 years under discussion. Here the magnetic horizontal force curve and that of the north-south pendulum show, as in Figure 3, sufficient correspondence to warrant a further study of this engrossing subject.

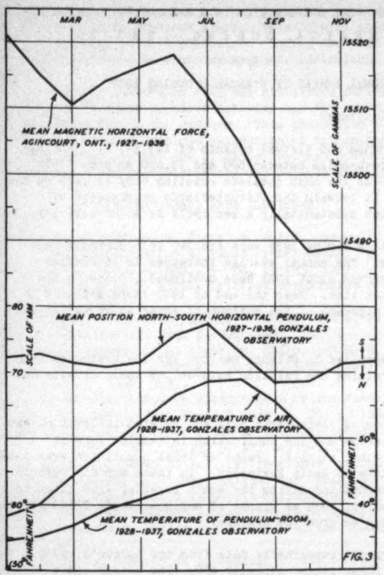

FIG. 3

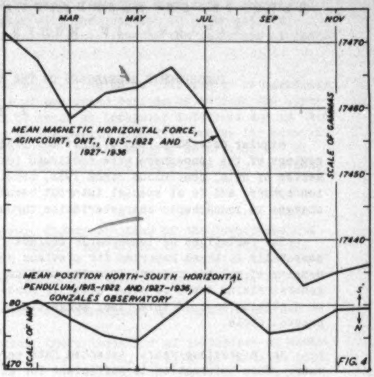

FIG. 4

In conclusion the writer would suggest that the mysterious forces within the Earth which may cause the annual magnetic fluctuations above referred to, may also to a certain extent influence the horizontal-pendulum movements already described.

Craigmyle,
Victoria, British Columbia, Canada

SUPERPOSITION IN INTERPRETATION OF RESISTIVITY-DATA

Irwin Roman

(Published with the approval of the Director, U. S. Geological Survey)

(Abstract)

In the case of a single uniform overburden, it is possible to calculate theoretically the value of the apparent resistivity for preassigned conditions. If the geological conditions meet these simple requirements to a reasonable degree, the observations may be superposed on a set of theoretical curves and interpretations made for the depth of the bed, the resistivity of the overburden and the resistivity of the bed. In previous papers, a knowledge of mathematics was needed to follow the discussion. In the present paper, the method is made available to the reader who is not mathematically inclined, or who has but little such training. A knowledge of graphing is the only mathematical requirement. The construction of the reference-charts is explained in detail and tables give the data from which the curves may be plotted. To illustrate the results, a set of reference-charts is included.

In the application of the method, the field-observations are reduced to graphs by means of a plotting table. These values are plotted in the same scales as used in the reference-charts. The observation-curve is superposed on the reference-chart until the best fit is obtained. The depth-index of the reference-chart determines a value on the depth-axis of the observation-graph and this value is converted to the depth by means of an interpretation-table. The resistivity-index of the chart determines a value on the resistivity-axis of the observation-graph and this value is converted to the value of the overburden resistivity by the same interpretation-table. The curve of best fit determines a value to be added to that determined by the resistivity-index and this sum is converted to the resistivity of the bed by the interpretation-table.

After the reference-chart has been prepared and the observation-curves drawn, the interpretation proceeds simply and directly. The method is rapid and requires no calculations.

U. S. Geological Survey,
Baltimore, Maryland

REPORTS ON MAGNETIC AND ELECTRIC WORK OF ORGANIZATIONS IN NORTH AMERICA DURING 1937-38

IONOSPHERIC RESEARCHES OF THE NATIONAL BUREAU OF STANDARDS DURING 1937

J. H. Dellinger

Regular determinations of critical frequencies and virtual heights of the E-, F_1-, and F_2-regions of the ionosphere were continued for frequencies between 600 and 16,000 kc/sec. This series of data, continuous since 1933, constitutes the most complete existing body of data on the ionosphere, and is of special interest because it reveals the characteristic progression of changes in ionospheric characteristics throughout substantially a semicycle of solar activity.

The variations of ionospheric characteristics during 1937 were similar both diurnally and seasonally to those reported for previous years. The annual average increases in ionization-density of all the regions from the minimum observed about 1933 have continued, following the general rising trend of solar activity since that time. Near the end of 1937 there appeared to be a slowing down of this rise, suggesting the approach of the maximum of solar activity for the present cycle.

As in previous years, selected data were supplied to Science Service for the Ursigrams each week. This information is sufficient for determining the relation between the ionic density and height in the ionosphere.

During the year the program of dissemination of ionospheric data and related information was expanded considerably. In addition to the Ursigrams and the publication in tabular form of monthly averages of critical frequencies and virtual heights, graphs of these quantities were published monthly in the Proceedings of the Institute of Radio Engineers. In these monthly reports was also included a discussion of ionospheric conditions during the month together with other information of interest in radio-transmission studies, such as graphs of maximum usable frequencies and tables showing variations of critical frequencies from the average.

In addition, weekly radio broadcasts of current ionospheric data from the Bureau's radio station WWV were inaugurated [see Nation. Bur. Stan. letter circular 499, "The weekly radio broadcasts of the National Bureau of Standards on the ionosphere and radio transmission conditions," May 15, 1937]. The critical frequencies, virtual heights, and maximum usable frequencies for various distances are given for both day and night. Announcements are also made of sudden ionospheric disturbances, effects associated with magnetic storms, and other irregularities.

Progress was made in the correlation of ionospheric data with radio-transmission conditions for various distances and frequencies. Both theoretical and experimental work was done on this problem. Regular continuous automatic recording of the field-intensities of medium-frequency and high-frequency stations and aural observations of radio-transmission conditions furnished the experimental material.

Evidence of the existence of a layer below the E-layer which is effective in broadcast-frequency transmission was discovered. Ionospheric data indicated that certain observed changes in the morning and evening of layer could not be between E- and F-layer transmission but must be between the E-layer and a still lower layer of much less ionization-density. This change-of-layer effect was similar to that observed for changes between E- and F-layer transmission at higher frequencies.

Studies of propagation of medium-frequency waves across the North Atlantic path in the winter indicated clearly that transmission over that path was at decidedly lower intensity than transmission over other paths. Results showed, for example, for given conditions of power and distance, the received intensities over the North Atlantic path were only about one-twentieth of those over the path between North and South America. A further study of the ionosphere at high magnetic latitudes is necessary before a clear explanation of this anomaly may be expected.

The previous work of the Bureau in the study of ionospheric storms was extended. A larger number of severe storms was observed, accompanying the increased solar activity. Besides the previously noted diffusion in the F_2-region, slight disturbances were noted in the F_1-region. The effects noted have indicated that a general turbulence of the F-region is associated with severe magnetic storms. There was some evidence of an increase in low-layer absorption during periods of extreme magnetic activity.

Sudden disturbances of the ionosphere manifested by radio fade-outs were more numerous and intense than in previous years. This is also associated with the increase in solar activity. Further study of this phenomenon during the year has thrown much light on the mechanism of radio transmission and terrestrial-magnetic variations.

Another phenomenon that was discovered during the year and which is believed to be associated with the increase in solar activity was the occurrence of prolonged periods of extreme low-layer absorption during the daytime. This phenomenon tends to occur at irregular intervals and is manifested by almost complete absorption of transmissions at the medium high frequencies for several hours during the day and often for several consecutive days. This low-layer absorption is different in some characteristics and related effects from the absorption associated with fade-outs.

The following papers were published:

T. R. Gilliland, S. S. Kirby, N. Smith, and S. E. Reymer, Characteristics of the ionosphere and their application to radio transmission. J. Res. Nation. Bur. Stan., v. 18, p. 645, 1937; Proc. Inst. R. Eng., v. 25, p. 823, 1937.

N. Smith and S. S. Kirby, Critical frequencies of low ionosphere layers, Phys. Rev., v. 51, p. 890, 1937.

T. R. Gilliland, S. S. Kirby, N. Smith, and S. E. Reymer, The ionosphere and magnetic storms, Phys. Rev., v. 51, p. 992, 1937.

T. R. Gilliland, S. S. Kirby, N. Smith, and S. E. Reymer, Characteristics of ionosphere at Washington, D. C., January to July, 1937, Proc. Inst. R. Eng., v. 25, p. 1174, p. 1354, p. 1493, and p. 1648, 1937.

J. H. Dellinger, Sudden disturbances of the ionosphere. J. Res. Nation. Bur. Stan., v. 19, p. 111, 1937; Proc. Inst. R. Eng., v. 25, p. 1253, 1937.

N. Smith, Rapid determination of angle of secant of angle of incidence for skip-distance calculation, QST, v. 21, p. 47, May 1937.

N. Smith, Extension of vertical-incidence ionosphere measurements to oblique-incidence radio transmission, J. Res. Nation. Bur. Stan. v. 19, p. 89, 1937.

J. H. Dellinger, Radio fade-outs through 1937. QST, v. 21, p. 35, February 1937.

J. H. Dellinger, Sudden ionosphere disturbances. Terr. Mag., v. 42, p. 49, 1937.

J. H. Dellinger, Sudden disturbances of the ionosphere. J. Applied Phys., v. 8, p. 732, November 1937.

S. S. Kirby, Remarks on S. Chapman's note on radio fade-outs and the associated magnetic disturbances, Terr. Mag., v. 42, p. 420, 1937.

Averages of critical frequencies and virtual heights of the ionosphere observed by the National Bureau of Standards, Washington, D. C., Terr. Mag., v. 42, March, June, September, and December, 1937.

National Bureau of Standards,
Washington, D. C.

REPORT ON SOLAR-RADIATION RESEARCH AT THE INSTITUTE OF OPTICS, UNIVERSITY OF ROCHESTER

Brian O'Brien

The development of equipment for the study of fluctuations in the ultra-violet output from the Sun by measurements from high-altitude sounding-balloons has been continued. As before we have been aided in this by the Smithsonian Institution. The current through a photocell, receiving sunlight through a specially designed optical system and filters, actuates, after suitable amplification, a small radio transmitter carried by the balloon. The transmitter is caused to emit a series of short dashes, the number per unit-time, suitably recorded on a chronograph at a ground receiving-station, being the measure of the light-intensity. The first successful flight with this equipment was made on March 30, 1937, and a number of flights have been made in the year following.

Since the expected fluctuations in solar radiation are small, it is necessary that a high order of accuracy and stability be achieved in apparatus for its measurement, and the major portion of our work has been devoted to this end. Progress has been good, but the overall accuracy in flight still falls a little short of the desired precision of 1 part in 200 in radiation-measurement.

Development work has been in progress upon a method and equipment for automatically recording the solar ultra-violet spectrum at ground-level, and the mechanical reduction of the observations to quantity of ozone in a zenith-atmosphere above the station. The major part of this has been completed, and is being reported elsewhere. Equipment is now in operation, and yields an hourly record of ozone which is of good accuracy and nearly independent of cloud-conditions. The aim has been to design the field-station instrument so that it is fully automatic, and produces a photographic record which is self checking for instrument adjustments.

University of Rochester,
Rochester, New York

GEOMAGNETIC INVESTIGATIONS OF THE UNIVERSITY OF NORTH CAROLINA

Wm. F. Prouty

The work that has been done by the University of North Carolina group may be divided into seven parts:

Part 1--Work in the North Carolina Piedmont

(A) Durham Triassic Basin--A highway survey of the northern two thirds of the Durham Basin has been completed. Stations were taken along the highways at half-mile intervals and in all, nine traverses were made across the Basin. The purpose of this work was to determine, if possible, whether the boundaries of such a basin could be detected by magnetic methods alone.

Variations within the basin are not large (the maximum being about 400 gammas, the average about 100) and the maximum anomalies may be traced to diabase-dikes and sills. The amplitude of the oscillations outside the Basin is about four times that within it. This change in amplitude permitted the location of boundaries of the Basin with a satisfactory degree of accuracy.

(B) A pegmatite-zone in the eastern granite-belt--An irregular zone of pegmatitic material extending for about four miles in a direction roughly parallel with the trend of Appalachian structure has been investigated. The enclosing schists and gneiss have anomalies considerably higher than the pegmatitic material. The anomalies over the granites are intermediate between those over the metamorphic rocks and those over the dikes and those over the quartz-veins are even lower than those over the dike-material. It has been found possible to locate the boundaries of these pegmatitic masses with a very satisfactory accuracy, in fact, it would be possible to map pegmatite-dikes under the given conditions with sufficient accuracy for commercial purposes.

Earth-resistivity methods have been applied to the area but as yet the work is not sufficiently complete for us to be able to advance any definite conclusions. Indications are that electrical resistivity is lower over the pegmatitic material than over the surrounding rocks.

Part II--Northern North Carolina Coastal Plain

(A) From the Virginia Line southward to Beauford, W. R. Johnson, Jr., has made several traverses across this area and apparently has located a trough trending subparallel with the coastline. This trough has been mentioned as possibly a river-valley such as was hypothecated by Grabau to have existed during Tertiary time. He suggested that this hypothetical river left the present land-surface north of Wilmington, North Carolina, and returned to the present coast-line somewhere about Charleston, across the State of Georgia and emptied into the Gulf of Mexico. Little evidence has been found for this hypothesis south of the area in which Johnson is working.

(B) Beaufort south to Pendor County--No work has been done in this area, but applications have been made for a grant-in-aid to complete a reconnaissance-survey during 1938. It has also been suggested that the area might serve as the field material for a Ph. D. dissertation.

It is necessary to connect the work done by Johnson, Straley, etc., in North Carolina and Virginia with the work done by Alexander, MacCarthy, Prouty, and Straley south of Pender County. This connection must be made across the area now under discussion.

Part III--Pender County southward to Myrtle Beach, South Carolina

(A) MacCarthy has reported four linear regional highs extending from about the latitude of Myrtle Beach northward to Wilmington, North Carolina, Near Wilmington, he and Straley were unable to find four highs. but have found some unusual high anomalies and have interpreted them as representing these same linear highs offset and disturbed by the Wilmington arch. The four trends mentioned are as follows: Little River to Leeland (nearest to the coast-line); Conway to Acme-Delco; Loris to Bolton; and Cool Springs-Bayboro to west of Lake Waccamaw.

(B) In the area around Wilmington, these highs have been contoured and the "structures" appear to be dipping toward southeast. Evidence has been obtained both for and against the concept of a Wilmington arch. However, the probability is that confirmation of this arch will be secured upon the completion of the survey in this area. Unfortunately, the area immediately overlying the axis of the so-called Wilmington arch is extremely swampy and will necessitate the use of a boat. Funds as yet have not been secured for the completion of this area.

(C) A buried Triassic basin has been found by G. R. MacCarthy, W. F. Prouty, and A. E. Alexander, in the neighborhood of Florence, South Carolina. This Basin has been traced into the borders of North Carolina.

(D) There appears to be an abrupt change in the slope of the basement-rocks about 25 miles northwest of the coast-line between Myrtle Beach and Wilmington. This abrupt change in slope appears to correlate with a similar change in slope discovered by Ewing, etc., in the area near Norfolk, Virginia. It also corresponds to increased slope of basement-rock between Wilmington and Southport, North Carolina, as shown by deep wells.

Part IV--From Myrtle Beach southward

(A) Myrtle Beach to the Santee River--This section of the coastal plain is to be surveyed 1938 by J. C. McCampbell, as the field-work for a Ph. D. dissertation. It is hoped to trace the highs mentioned above into and across this area and to detail such structures as will be encountered.

Recently, reports of drilling activities near Hemingway, Williamson County, South Carolina, indicate the possibilities of petroleum-production. Such being the case, the area is of extreme importance and efforts will be made to rush it through to completion.

(B) Santee River southward--A large amount of detail work has been done in this region as far south as Florida. However, most of it has been done by commercial concerns for commercial purposes and these firms are as yet not willing to release their data.

Drilling for petroleum has begun in Coffey County, Georgia, and it is hoped that upon the completion of this venture, much, if not all, of the data now in possession of the oil-companies may be released for publication.

The area immediately south of the Santee River is of particular interest because the Charleston earthquake had its epicentrum near the town of Summerville, South Carolina, and it is believed that detailed magnetic work in this area might supply information useful in the complete elucidation of the problem of the Charleston earthquake.

Part V--Carolina bays

The work previously done by W. F. Prouty, G. R. MacCarthy, H. W. Straley, and J. A. Alexander on the elliptical shallow, sand-rimmed depressions (Carolina bays) found chiefly in North Carolina and South Carolina has been much extended through a grant from the Geological Society of America. This work was carried out by W. F. Prouty, H. W. Straley, and six advanced students of geology. The investigation can be classified under two heads: (a) geological; (b) geophysical. (b) consisted of a grid-survey of about 25 square miles chiefly in the neighborhood of Syracuse, South Carolina, where one large bay is well separated from a number of smaller bays. The problem here was found to be somewhat complicated by the presence of a linear high with general east-west trend. In the finished isogamic map of the area the influence of the linear high has been eliminated as far as possible. A spot-high is associated with each of the bays. The large bay has the best developed spot-high. There is, however, on the map one very widespread or rambling, poorly defined high area which has no apparent connection with any bay. This may be related to the linear high which has otherwise been largely eliminated in the finished map of the region.

Part VI--Appalachian Mountain Area

(A) Nine traverses from the Appalachian Plateau across the folded belt have been made by H. W. Straley and one by L. L. Nettleton. The tentative conclusions that they draw from these traverses are: (1) Probably the basement-rocks are not involved in the folds northwest of the Appalachian structural front as defined by Price; and (2) that not all of the thrust faults in the folded Appalachians extend into the basement complex. Nettleton has made one gravimetric profile which apparently substantiates the conclusions drawn from magnetic data. Straley has conducted laboratory-experiments which also bear out the above interpretation.

(B) Stratigraphic Interpretation--Detailed work in the Giles County (Virginia) area indicates the possibility that magnetic anomalies may be due to local changes in stratigraphy or to structures having brought iron-bearing formations, such as the Clinton, near to the surface or above it.

Part VII--Research into new applications of electrical resonance-methods

The geophysical work of Professor L. D. Burling, first prosecuted as merely another method of discovering oil-field structures, has gradually proven that the particular type of electrical resonance-method used has far wider application as an instrument of pure research. The instrument includes a tuning device, vacuum-tube amplifiers, and measuring devices for the qualitative and quantitative study of radiations which seem to characterize all minerals and elements. Part of the observed facts almost necessitate the explanation of these radiations in terms of charged particles, other of the results point as indubitably to wave-length. But, whatever their character, they have been rigorously subjected for several years to the statistical method. The present objective of the research is in the direction of improving and refining both the instrument and the technique of its operation.

University of North Carolina,
Chapel Hill, North Carolina

MAGNETIC WORK OF THE UNITED STATES COAST AND GEODETIC SURVEY, APRIL 1937 TO MARCH 1938

E. W. Eickelberg

Magnetic surveys--Observations for magnetic declination were made at 141 stations in Alaska, California, Connecticut, Florida, Massachusetts, Montana, New York, Oregon, Texas, Vermont, Virginia, and Washington by officers of the Bureau. Massachusetts Geodetic Survey, working under the Works Progress Administration, occupied 19 stations in Massachusetts and Rhode Island. Complete observations for declination, horizontal intensity, and dip were made by Bureau personnel at 12 stations in California, Kansas, Louisiana, Missouri, Nevada, North Carolina, Oklahoma, and Texas. Dip, only, was observed at one station in California. A large number of observations for declination and two ship-surveys were made offshore around San Clemente Island, California. Four determinations of declination were made in a non-magnetic whaleboat in the Gulf of Mexico, using a standard ship's compass and azimuth circle.

Magnetic observatories and cooperative work--The five magnetic observatories of the Bureau have been maintained in continuous operation. Through cooperation with the Department of Terrestrial Magnetism of the Carnegie Institution of Washington, the following projects have been continued or started during the year:

(a) Use of the Carnegie Institution of Washington sine-galvanometer No. 1 as the standard horizontal-intensity instrument at Cheltenham. The standard cells were compared at Cheltenham by personnel of the National Bureau of Standards, Department of Terrestrial Magnetism of the Carnegie Institution of Washington, and Coast and Geodetic Survey on May 21, 1937. The results were in satisfactory agreement with values previous determined on November 20, 1935.

(b) Use of the Carnegie Institution of Washington standard earth-inductor No. 48 at Cheltenham.

(c) Routine operation of the cosmic-ray meter at Cheltenham for the Committee on Coordination of Cosmic Ray Investigations of the Carnegie Institution of Washington.

(d) Standardization of all magnetic instruments of the Carnegie Institution of Washington and of the Survey at Cheltenham Observatory, in cooperation with the staff of the Institution's Department of Terrestrial Magnetism.

(e) Continuation of operation of the Carnegie Institution of Washington perminvar vertical-intensity variometer at Cheltenham on an experimental basis with the cooperation of A. G. McNish of the Institution's Department of Terrestrial Magnetism.

(f) Continuation of atmospheric-electric and earth-current programs at the Tucson Magnetic Observatory.

(g) A series of comparison observations between Carnegie Institution of Washington magnetometer-inductors No. 13 and 28 and the Coast and Geodetic Survey magnetic instruments at the Honolulu Magnetic Observatory were made in July, 1937. An auxiliary absolute observatory was constructed at the site of the auxiliary magnetic station just prior to the time of beginning of the comparisons, and this new observatory was used in the observing program.

Through cooperation with Science Service, magnetic character-figures for Greenwich half-days for all American observatories (five Coast and Geodetic Survey, two Carnegie Institution of Washington) are broadcast weekly from Station W1XAL of the Wide World Broadcasting Foundation of Boston, Massachusetts. The magnetic character of the day as determined every morning from the magnetograms recorded at the Cheltenham Observatory is broadcast daily from the same station. The above information is also published weekly in mimeographed form by Science Service in "Science Service Research Aid Announcement," issued to Research Workers.

Special investigations--The Danish Meteorological Institute loaned their QHM 23 to this Bureau temporarily for the purpose of making comparisons between Cheltenham and Sitka and between Cheltenham and the Danish stations. A long series (20 sets) of comparisons was made at Cheltenham between CIW sine-galvanometer No. 1 and QHM 23; the latter was then shipped to Sitka Magnetic Observatory where 64 sets of comparisons were made between it and magnetometer 36, after which it was returned to Cheltenham and compared again (20 sets) with the sine-galvanometer. The results were quite satisfactory and demonstrate the feasibility, convenience, and accuracy of the method.

Special diurnal-variation observations for magnetic declination were made at Fort Bragg, North Carolina, at the request of the War Department.

Several United States Navy compasses and azimuth-circles have been tested by special methods at the Cheltenham Observatory for use in determination of magnetic declination at sea.

Special investigations of orientation of variometers were continued in the Washington office and at the Cheltenham Magnetic Observatory. Both Eschenhagen and Adie magnetographs at that Observatory have been oriented and the scale-value of the Adie vertical-intensity variometer has been increased.

Special studies have been made of field-surveys made with Askania vertical-intensity variometers.

A new method of determination of induction-coefficients of magnetometer-magnets has been devised and found to be quite satisfactory as to accuracy and convenience.

Publications--"Results of observations at the Honolulu Magnetic Observatory, 1929-1930" was completed.

Miscellaneous--Magnetic character-numbers have been assigned to records obtained at the Los Angeles and San Antonio observatories operated by this Bureau during the interval 1882-95. Progress has been made on the preparation of the College-Fairbanks Polar Year results for publication.

U. S. Coast and Geodetic Survey,
Washington, D. C.

PROGRESS-REPORT ON RESEARCHES IN TERRESTRIAL MAGNETISM AND ELECTRICITY AT DEPARTMENT OF TERRESTRIAL MAGNETISM, CARNEGIE INSTITUTION OF WASHINGTON, FOR YEAR APRIL 1937 TO MARCH 1938

J. A. Fleming

The year has been marked by energetic prosecution of experimental investigation and continued coordination and integration of various researches paving the way for more general formulation of geophysical facts. Good progress has been made in two major projects which open new fields of investigation and promise important advances.

Atomic-Physics Observatory--One of these is the completion of the Atomic-Physics Observatory and the considerable progress in the installation of the electrical equipment. The electrostatic generator and tube are designed to operate at potentials exceeding five million volts and offer unique opportunity for further studies in the expanding field of nuclear physics--a field to which we must look for additional understanding of magnetic phenomena.

Automatic multifrequency equipment--The second project realized is the installation of the automatic multifrequency equipments for ionospheric measurements at both the Huancayo and Watheroo magnetic observatories. This equipment, developed at the Department, has the following characteristics: Ability to record successfully without interference from existing radio services; relatively uniform vertical radiation throughout the frequency-range; automatic interlocking of transmitting and receiving tuning; mechanical simplicity and uniform limits of precision and resolution. The stations at Huancayo and Watheroo operating continuously in conjunction with a somewhat similar equipment at the National Bureau of Standards station at Meadows, Maryland, should provide a much more complete survey of the upper atmosphere than has been previously possible. Results at the Kensington Experiment Station of the Department in Maryland, where the apparatus was tested, appear to have gone a long way towards settling the vexed question of the ionosphere and its refractive index for radio waves--an uncertainty heretofore restricting our ability to interpret ionospheric observations.

Magnetic investigations--Heights and character of the electric currents which occasionally flow in regions around the magnetic poles of the Earth were determined by a new method of mathematical analysis. The results indicate that these currents flow in a belt several hundred miles and at heights between 60 and 90 miles wide above the Earth's surface.

Measurements made on a sample of sediment extending nine feet into the floor of the North Atlantic Ocean show marked differences in the magnetization at various depths. If correctly interpreted, these measurements indicate that the direction of the compass in that region was about 60° east of true north at the time the sediment was deposited many thousands of years ago, whereas at the present time in the same locality it is 30° west of true north. In further investigations of this nature, varved Pleistocene clay from beds of old glacial lakes near New Haven, Connecticut, are to be collected for like examination. This method of studying geomagnetism through the measurement of fossil magnetization holds the hope of possible determination of secular changes in geologic ages. A method of measuring the moment of small magnetic dipoles was devised in this connection. The theoretical sensitivity and practical application are such that moments of 3×10^{-7} CGS unit can be detected.

The weekly American magnetic character-figures for the seven American-operated observatories at Watheroo, Huancayo, Cheltenham, Honolulu, San Juan, Sitka, and Tucson were compiled and published regularly each week through Science Service. Statistical examination of the data for the first year of this character-figure shows it to be a precise measure and to represent world-wide conditions with relatively high fidelity. The first six months of 1937 reveal a close relation to the international magnetic character-figure adopted from the records of over 40 observatories. It appears that, while the international figure is superior for selecting extremely quiet days, the American figure is superior for selecting extremely disturbed days and for the study of radio-communication conditions.

The investigation of a positive relationship of cosmic radiation with magnetic and other phenomena has been continued. World-wide decreases of three to five per cent in daily means of cosmic-ray intensity are found to be associated with changes in the Earth's magnetic field during two major magnetic storms. Other magnetic storms of equal intensity occur with no appreciable cosmic-ray effects. Thus it appears that the entire current-system for the storm-time field of both types of storms can not be located at the same distance above the Earth. A significant correlation between changes in daily means of cosmic-ray intensity for two stations separated 50° in latitude probably results from the mechanism responsible for the magnetic-storm effect. Statistical analyses of the cosmic-ray records obtained at Cheltenham and at Huancayo proved inadequate to establish a sidereal diurnal variation in cosmic-ray intensity.

Instruments--The coil of the new primary standard for measuring the Earth's vector magnetic field has been completed. It is constructed with an accuracy so that the magnetic field is calculated to about one part in a million. It is hoped that it will be ready for use by the end of 1938.

Geoelectric investigations--The ionospheric investigations demonstrated that the radio fade-out--the sudden failure of high-frequency radio-wave transmission--in the sunlit hemisphere and which coincides with certain bright chromospheric eruptions must be due to the absorption of the radio waves in the lower ionosphere.

An examination of the earth-current flow at Huancayo and Tucson for 1932, near the sunspot-minimum, reveals a definite lunar diurnal variation of markedly double period in both components of potential-gradient at both stations. Its amplitude is not more than one magnitude smaller than that of its solar diurnal variation. Changes with the phase of the Moon are in general similar to those found in the Earth's magnetic field and are most pronounced at low latitudes.

The observation that the electrical conductivity of air at sea is relatively independent of latitude was accounted for quantitatively. The quantitative independence of air-conductivity upon the intensity of cosmic radiation up to considerable altitudes, especially over the oceans and the polar regions, was more securely established. The electrical resistance of a vertical column of atmosphere as calculated from the observed intensity of cosmic radiation was found to be about 20 per cent greater at the equator than at 50° latitude, thus providing an acceptable explanation for the dependence of electrical field-strength upon latitude shown by observations made on the cruises of the Carnegie.

Experiments are in progress to determine whether small molecular ions increase in size to become intermediate ions and, after further growth, large ions. Through such a process a production of large ions by ultra-violet light from a mercury-arc lamp might be explained. Tests of small ions produced by radioactive material indicated no increase in size, and tests are now being made to determine whether small molecular ions produced by ultra-violet light grow into intermediate or large ions.

Observatory-work--The observatories at Huancayo, Peru, and at Watheroo, Western Australia, continued the extensive geophysical program, obtaining continuous records of magnetic elements. Magnetic activity was very marked as the peak of the sunspot-cycle was reached during 1937. The remarkable range of 1350 gammas occurred in horizontal intensity during the exceptionally violent magnetic storm of April 16, 1938, at Huancayo.

Cooperative work was continued with the following observatories: Atmospheric-electric program at Apia in Western Samoa; atmospheric-electric and earth-current programs at Tucson, Arizona; maintenance of international magnetic standards at the Cheltenham Magnetic Observatory of the United States Coast and Geodetic Survey. Some cooperation was extended to the Magnetic Observatory at Cape Town through the loan of instruments and information regarding methods. As a result of cooperation with the MacGregor Arctic Expedition, a temporary magnetic observatory was established in September 1937 at Reindeer Lake near Etah, Greenland.

Land-work--Field-work by W. C. Parkinson in Australasia and the Pacific Islands was continued. Comparisons of observatory-standards were made at the Apia Observatory, at the Honolulu Magnetic Observatory, at Blacktown in Australia (with the instruments of the Aerial, Geological, and Geophysical Survey of Northern Australia), and at the Watheroo Magnetic Observatory. Plans were made for later comparison of standards at the Batavia Observatory. During September to November 1937, Mr. Parkinson occupied ten stations in New Caledonia, New Hebrides, and Tahiti. In January 1938, after the comparisons at Watheroo, he occupied two repeat-stations in Western Australia, and through April 1938 had occupied 16 stations in Malaya (six) Siam (five), and French Indo-China (five). Seven stations in Northern Australia were occupied by the Aerial, Geological, and Geophysical Survey of Northern Australia.

In cooperation with the Gulf Research and Development Company, results at six stations in Arabia were obtained by Messrs. Murrell and Wendenburg.

Publications--The activities of the Department are given in more detail in the report of the Director in Year Book 36 of the Carnegie Institution of Washington (pp. 231-285). A few of the papers published during the past year are listed below.

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M. A. Tuve and L. R. Hafstad, Structural forces within the atomic nucleus, (Abstract) J. Wash. Acad. Sci., v. 28, pp. 29-31, 1938.

Department of Terrestrial Magnetism,
Carnegie Institution of Washington,
Washington, D. C.

AMERICAN GEOPHYSICAL UNION

Nineteenth Annual Meeting

SECTION OF OCEANOGRAPHY

Washington, D. C.
April 29, 1938

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OCEANOGRAPHIC ACTIVITIES OF THE HYDROGRAPHIC OFFICE AND THE UNITED STATES NAVY DURING 1937 AND PLANS FOR 1938

Lamar R. Leahy

The Hydrographic Office, in carrying out its mission to the maritime world, has contributed during the past year much to the science of oceanography in widely separated oceanic areas of the Earth's surface.

Hydrographic surveys have been continued in the Caribbean area and along the northeast coast of South America, together with a continuance of dynamic sounding work.

The ever-increasing use of the sonic depth-finder by navigators of naval and merchant vessels, a work actively fostered by the Hydrographic Office, is resulting in an accession of information relating to those areas of the ocean-floor, heretofore unsounded, that will be of the greatest value to the scientific world in its studies of the configuration of the ocean-bottom.

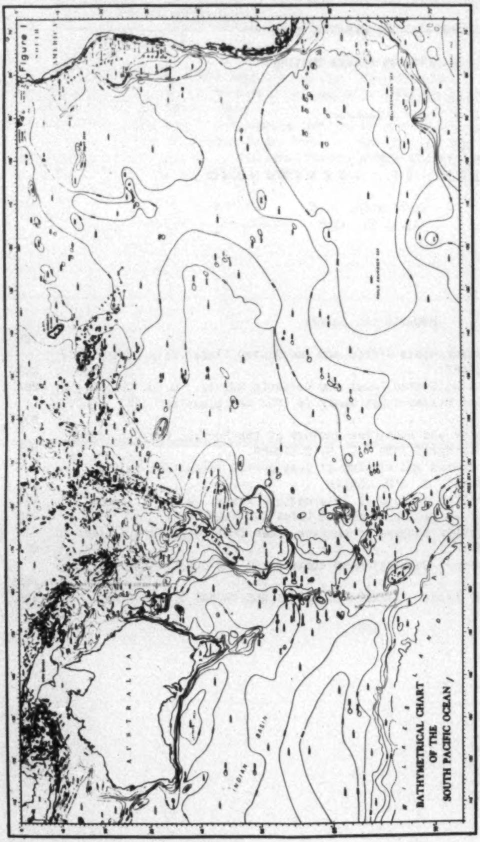


Figure 1

BATHYMETRICAL CHART OF THE SOUTH PACIFIC OCEAN

During the year the U.S.S. Ramapo, in the course of her regular service across the North Pacific, in three passages, obtained 4,430 sonic soundings, which, added to the soundings previously obtained by that vessel in 44 passages, makes a total of 29,172 sonic soundings in the Pacific area.

In the Caribbean area the U.S.S. Gridley, Bagley, and Nokomis obtained numerous sonic soundings, most of which were in unsounded areas. This information, combined with that obtained by the U.S.S. Barracuda, in connection with the Gravimetric Expedition of 1936, previously reported, together with sonic soundings obtained by the U.S.S. Philadelphia, Fanning, Erie, and Brooklyn, during January and February, 1938, have produced much valuable data for the portrayal of the bottom-contours of this area.

Based on the recent information received in this Office, there were issued during the year on the back of the Pilot Chart of the respective oceans, graphic presentations of the areas now sounded.

Also compiled during the year and published on the back of the South Pacific Pilot Chart for June, July, and August, 1938, was a bathymetrical chart of the South Pacific Ocean. This chart is in nine colors and clearly delineates the outstanding characteristics of this extensive ocean-area. Incorporated on this chart are the Carnegie and Merriam Ridges discovered by the non-magnetic vessel *Carnegie* in latitude 1° 32' south, longitude 82° 16' west, and latitude 24° 57' south, longitude 82° 15' west, respectively, a deep found by the Byrd Antarctic Expedition 1928-30, in latitude 61° 06' south, longitude 179° 00' east, and the results of many other scientific expeditions (Fig. 1).

Echo (sonic) soundings obtained by U.S. Naval vessels--The following is a tabulated list of echo (sonic) soundings obtained by U.S. Naval vessels from January 1, 1937, to December 31, 1937:

U.S.S. *Barracuda* - 75 sonic soundings obtained on two trips through Anegado Passage where she discovered a shoal of 23 fathoms extending in an east-northeasterly direction from 18° 19' 00" north in 64° 13' 15" west to 18° 20' 42" north in 64° 08' 20" west.

U.S.S. *Ramapo* - 1029 sonic soundings in the North Pacific en route Wilmington, California, to Manila, Philippine Islands, crossing 33° 16' north in 120° 43' 30" west, 27° 09' north in 179° 03' west, and 12° 52' north in 124° 40' east.

U.S.S. *Nokomis* - 200 sonic soundings obtained off Navassa Island while investigating shoal of 75 fathoms reported in 18° 25' north and 75° 56' west.

U.S.S. *Erie* - 37 sonic soundings obtained near Flores Island, Azores, in 39° 22' 30" north and 31° 54' west.

U.S.S. *Cushing* - 80 sonic soundings in the North Pacific from 29° 25' north in 131° 19' 30" west to 23° 18' north in 149° 21' west.

U.S.S. *Perkins* - 163 sonic soundings taken en route Hilo, Territory Hawaii, to Cape San Lucas, Mexico, from 19° 49' 40" north in 154° 43' 15" west to 22° 50' 05" north in 110° 17' 10" west.

U.S.S. *Mississippi* - 160 sonic soundings obtained off San Pedro, California, in 33° 42' north and 119° 44' 30" west.

U.S.S. *Preston* - 32 sonic soundings obtained near Great Barrier Island, Auckland Harbor, New Zealand, from 35° 26' south in 175° 46' east to 36° 01' 45" south in 175° 19' 30" east.

U.S.S. *Nokomis* - 1200 sonic soundings taken en route Norfolk, Virginia, to Coco Solo, Canal Zone, via Miami, Florida, and Guantanamo Bay, Cuba, crossing 34° 41' 15" north in 75° 05' 00" west, 13° 23' 00" north in 77° 55' 30" west, and 9° 50' 10" north in 79° 53' 10" west.

U.S.S. *Barracuda* - 1000 sonic soundings taken in connection with gravity-expeditions in the Caribbean Sea mostly around Virgin Islands area, crossing 18° 34' north in 63° 36' west, 15° 27' north in 66° 41' west, 20° 08' north in 60° 50' west, and 10° 00' north in 79° 32' west.

U.S.S. *Nokomis* - 310 sonic soundings taken en route Coco Solo, Canal Zone, to Cartagena, via Caledonia Harbor, Republic of Panama, and Barbacoas Bay, Colombia, crossing 9° 46' 30" north in 79° 41' 00" west, 8° 55' 23" north in 77° 38' 20" west, and 9° 07' 10" north in 75° 42' 23" west.

U.S.S. *Charleston* and U.S.S. *McDougal* - 1200 sonic soundings taken en route Norfolk, Virginia, to Gibraltar, crossing 36° 54' north in 71° 06' west, 37° 42' north in 29° 54' west, and 40° 15' north in 18° 30' west.

U.S.S. *Hannibal* - 1875 sonic soundings taken en route Norfolk, Virginia, to Coco Solo, Canal Zone, via Guantanamo Bay, Cuba, and Port Limon, Costa Rica, crossing 36° 54' 30" north in 75° 52' 00" west, 18° 31' 30" north in 75° 31' 30" west, 14° 41' 40" north in 78° 54' 15" west, and 9° 23' 20" north in 80° 00' 00" west.

U.S.S. *Hannibal* - 164 sonic soundings obtained around Panama Canal area in 9° 25' 00" north and 79° 55' 30" west.

U.S.S. *Nokomis* - 150 sonic soundings taken en route Barbacoas Bay, Colombia, to Coco Solo, Canal Zone, crossing 10° 06' 55" north in 75° 45' 32" west and 9° 40' 52" north in 79° 34' 20" west.

U.S.S. *McDougal* and U.S.S. *Charleston* - 1589 sonic soundings in North Atlantic, crossing 35° 57' north in 5° 35' west, 30° 24' north in 39° 14' west, and 18° 33' north in 66° 04' west.

U.S.S. *Vega* - 77 sonic soundings taken off south coast of Panama crossing 7° 49' north in 89° 32' 18" west and 6° 31' 42" north in 83° 05' west.

U.S.S. *Hannibal* - 71 sonic soundings off northern coast of Panama in 9° 34' 15" north and 80° 18' 13" west.

U.S.S. *Preston* - 219 sonic soundings taken en route Bremerton, Washington, to Pearl Harbor, Territory of Hawaii, crossing 45° 40' 30" north in 129° 10' west, 28° 09' north in 151° 01' west, 22° 13' north in 156° 28' west, and 21° 24' north in 157° 13' west.

U.S.S. *Pennsylvania* - 17 sonic soundings in western area of Hawaiian Islands, crossing 25° 47' north in 175° 01' west and 24° 33' north in 171° 21' west.

U.S.S. *Hannibal* - 96 sonic soundings in the Caribbean Sea, crossing 9° 26' 00" north in 79° 55' 30" west and 10° 00' 00" north in 82° 57' 30" west.

U.S.S. *Tarpon* - 30 sonic soundings in the Pacific in 4° 37' 15" north and 80° 21' 45" west, over an uncharted bank.

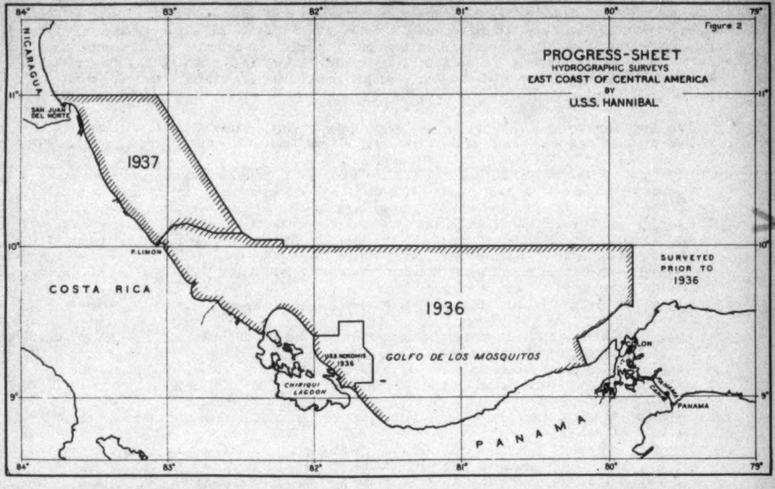


Figure 2

PROGRESS-SHEET
HYDROGRAPHIC SURVEYS
EAST COAST OF CENTRAL AMERICA
BY
U.S.S. HANNIBAL

84°
83°
82°
81°
80°
79°
11°
10°
9°
NICARAGUA
SAN JUAN DEL NORTE
1937
P. LIMON
COSTA RICA
1936
SURVEYED PRIOR TO 1936
U.S.S. NOKOMIS 1936
GOLFO DE LOS MOSQUITOS
CHIRIQUI LAGOON
COLON
PANAMA
PANAMA

U.S.S. Reid - 207 sonic soundings in North Atlantic, crossing 36° 51' 20" north in 75° 48' 00" west, 33° 42' north in 60° 58' 30" west, 32° 44' 15" north in 28° 21' 40" west, and 32° 48' 07" north in 17° 15' 47" west.

U.S.S. Holland - 124 sonic soundings in western part of Hawaiian Islands, crossing 22° 08' north in 162° 40' west and 26° 50' north in 178° 24' west.

U.S.S. Hannibal - 98 sonic soundings in the Caribbean Sea, crossing 9° 58' 45" north in 82° 57' 30" west and 9° 27' 30" north in 79° 55' 00" west.

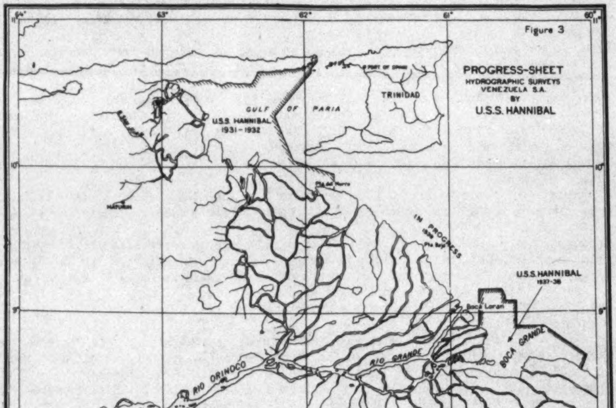


Figure 3

PROGRESS-SHEET
HYDROGRAPHIC SURVEYS
VENEZUELA S.A.
BY
U.S.S. HANNIBAL

64°
63°
62°
61°
60°
11°
10°
9°
8°
PORT OF SPAIN
TRINIDAD
GULF OF PARIA
U.S.S. HANNIBAL 1931-1932
MATURIN
IN PROGRESS 1938
Pta Baja
U.S.S. HANNIBAL 1937-38
Boca Loran
BOCA GRANDE
RIO GRANDE
RIO ORINOCO
CIUDAD BOLIVAR

U.S.S. Shaw - 20 sonic soundings in North Atlantic crossing 37° 05' 30" north in 31° 24' 00" west and 37° 36' north in 45° 28' west.

U.S.S. Drayton, U.S.S. Flusser, U.S.S. Reid, and U.S.S. Lamson - 512 sonic soundings taken over "Guardian Bank" in 9° 25' north and 86° 45' west.

U.S.S. Hannibal - 750 sonic soundings taken while en route Costa Rica to Norfolk, Virginia, crossing 11° 08' 00" north in 82° 49' west, 12° 31' 48" north in 81° 19' 30" west, 20° 54' 45" north in 84° 32' 30" west, and 35° 20' 00" north in 75° 14' 42" west.

U.S.S. Pollack - 78 sonic soundings in the Caribbean Sea, crossing 21° 10' 06" north in 84° 39' 06" west and 18° 30' 00" north in 82° 53' 30" west.

U.S.S. Drayton - 143 sonic soundings in the Pacific, near Howland Island, crossing 5° 40' north in 173° 02' west, 1° 20' south in 179° 00' west, 2° 30' 00" north in 179° 02' west, and 4° 58' 00" north in 176° 02' 00" east.

U.S.S. Lexington - 486 sonic soundings in the Pacific, in vicinity of Howland Island, crossing 2° 00' south in 163° 00' west and 11° 00' north in 179° 00' east.

U.S.S. Cushing - 167 sonic soundings in the Pacific taken while engaged in operations in connection with the search for Amelia Earhart Putnam, crossing 10° 37' 42" north in 167° 59' 00" west, 0° 22' 00" south in 177° 55' 30" west, 0° 48' 00" north in 175° 41' 00" east, and 17° 32' 00" north in 163° 32' 00" west.

U.S.S. Ranger - 85 sonic soundings taken during cruise from San Diego, California, to Lima, Peru, and return, crossing 15° 23' north in 102° 19' west, 4° 01' north in 91° 24' west, 1° 46' south in 87° 20' west, and 9° 45' 30" south in 79° 25' 30" west.

U.S.S. Hannibal - 966 sonic soundings taken en route Hampton Roads, Virginia, to La Guaira, Venezuela, crossing 35° 36' 36" north in 75° 14' 48" west, 26° 42' 12" north in 70° 34' 00" west, 16° 35' 30" north in 67° 21' 30" west, and 10° 38' 30" north in 61° 35' 48" west.

U.S.S. Shoshone - 70 sonic soundings taken around Howland Island in 0° 50' north and 176° 42' west.

U.S.S. Gridley - 1077 sonic soundings in the Caribbean Sea, crossing 17° 33' 45" north in 67° 09' 30" west, 10° 52' 45" north in 67° 03' 00" west, 14° 31' 00" north in 67° 30' 30" west, and 18° 02' 00" north in 74° 55' 00" west.

U.S.S. Bagley - 579 sonic soundings in the Caribbean Sea, crossing 17° 53' 30" north in 75° 00' 00" west, 10° 35' 54" north in 79° 00' 00" west, 15° 45' 30" north in 80° 55' 42" west, and 20° 21' 24" north in 83° 51' 36" west.

U.S.S. Hannibal - 104 sonic soundings taken en route Port of Spain, Trinidad, British West Indies, to Boca Grande Bar, Venezuela, crossing 10° 49' 00" north in 61° 32' 00" west and 8° 56' 12" north in 60° 21' 30" west.

U.S.S. Hannibal - 125 sonic soundings taken en route Boca Grande Bar, Venezuela, to Port of Spain, Trinidad, British West Indies, crossing 8° 56' 30" north in 60° 22' 00" west and 10° 38' 00" north in 61° 35' 18" west.

U.S.S. Ramapo - 2500 sonic soundings obtained en route San Pedro, California, to Pearl Harbor, Territory of Hawaii, to Guam, M.I., to Chefoo, China; Guam, M.I., to Pearl Harbor, Territory of Hawaii, to Manila, Philippine Islands, crossing 33° 25' north in 120° 31' west and 21° 42' north in 156° 28' west, crossing 21° 19' 30" north in 160° 27' 30" west and 13° 52' 18" north in 145° 39' east, crossing 15° 31' 12" north in 143° 03' east and 29° 53' north in 130° 39' 30" east, crossing 13° 47'

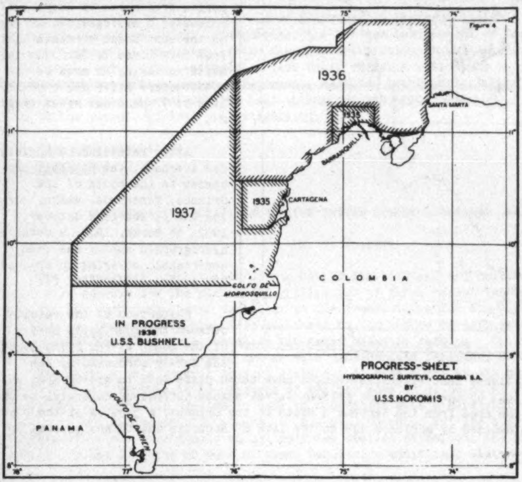


Figure 4

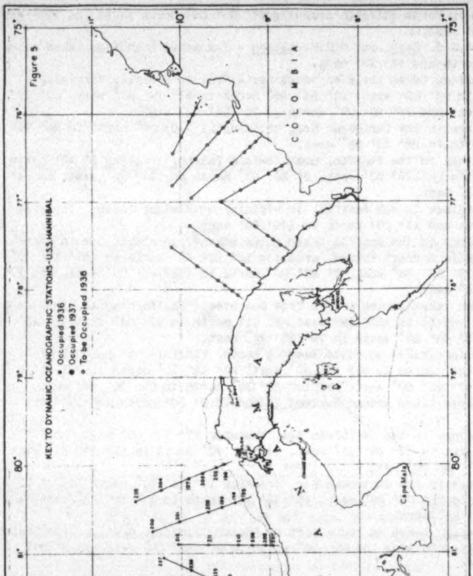


Figure 5
KEY TO DYNAMIC OCEANOGRAPHIC STATIONS-U.S.S. HANNIBAL
• Occupied 1936
● Occupied 1937
○ To be Occupied 1938
COLOMBIA
PANAMA
COSTA RICA
Cape Mala
Gulf of Dulce
Gulf of Nicoya
San Juan del Norte
75°
76°
77°
78°
79°
80°
81°
82°
83°
84°
85°
11°
10°
9°
8°
7°

north in 144° 59' east and 21° 23' 30" north in 159° 30' 48" west, crossing 21° 36' north in 161° 13' west and 13° 16' 30" north in 125° 45' east.

U.S.S. Ramapo - 911 sonic soundings obtained en route Shanghai, China, to San Diego, California, crossing 30° 10' north in 130° 13' 30" east, 34° 27' north in 151° 54' east, 39° 56' north in 165° 18' west, and 33° 13' north in 120° 00' 30" west.

U.S.S. Hannibal - 141 sonic soundings taken en route Port of Spain, Trinidad, British West Indies, to Boca Grande Bar, Orinoco River, Venezuela, crossing 10° 38' 24" north in 61° 36' 36" west and 8° 53' north in 60° 23' 54" west.

U.S.S. Hannibal - 85 sonic soundings taken en route Boca Grande Bar, Orinoco River, Venezuela, to Port of Spain, Trinidad, British West Indies, crossing 8° 56' north in 60° 21' 30" west and 10° 38' 42" north in 61° 39' 12" west.

U. S. Naval surveys--The U.S.S. Hannibal, during the period from April to July, 1937, conducted a hydrographic survey on the east coast of Costa Rica from Port Limon to San Juan del Norte, covering an area of 2,370 square miles and a coastline of 77 nautical miles (Fig. 2).

After returning to Norfolk for overhaul, the Hannibal proceeded to the mouth of the Orinoco, Venezuela, where, during the period from October, 1937, to March, 1938, a detailed hydrographic survey has been undertaken, covering an area of 753 square miles (Fig. 3).

Comparison of the results between the Hannibal's survey of the mouth of the Orinoco and the survey conducted by the U.S.S. Dolphin in 1900 discloses that extensive changes have taken place both in hydrography and topography. It is hoped that by the end of the present survey season sufficient data will be on hand from which to chart the area from the Serpent's Mouth to the Orinoco, a portion of the coast which heretofore has been avoided by mariners due to the lack of accurate charts and general information.

The U.S.S. Nokomis continued the survey of Colombia southwestward from Punta Baru covering during the season an area of 6,700 square miles. At the end of the season the Nokomis was decommissioned, having been found unfit for further duty, and the U.S.S. Bushnell was assigned to the Hydrographic Office in her place. This vessel will continue the survey of the north coast of Colombia which it is hoped will be extended this season to cover the area from Golfo de Morrosquillo to the Panama boundary including the Gulf of Darien (Fig. 4).

Oceanic depths--The eighth of the series of annual publications listing oceanic depths, Hydrographic Office Publication No. 210G, "List of oceanic depths in the Pacific Ocean, 1935-1937, inclusive," is now in preparation and will be issued shortly.

The seven previously published annuals, 210 to 210F, inclusive, contain soundings previously obtained by U. S. Naval vessels in the Caribbean Sea, the North and South Pacific oceans, and the North and South Atlantic oceans.

Dynamic oceanographic surveys--In the furtherance of the dynamic sounding work, inaugurated by the Hydrographic Office in 1933, the U.S.S. Hannibal occupied during the past year 18 stations on the east coast of Costa Rica. The total number of stations that have been occupied in Central American Waters by the Hannibal is now 266. It had been planned that a series of stations contiguous to the coasts of Panama and Colombia would be occupied by the Hannibal during the past season, but a change in the surveying schedule of that vessel precluded this operation. It is expected that the Bushnell will occupy during the present season those stations originally laid down for occupation by the Hannibal (Fig. 5).

Current- and temperature-charts--Since the beginning of the twentieth century, a vast accumulation of data relating to oceanic currents and sea-surface temperatures, contributed by cooperating observers of all nationalities, has been deposited in the files of the Office. Due to the limited personnel available for the analysis of these data, no general program for the evaluation of the material has been possible in the past.

Recently, through the aid of governmental projects, the Hydrographic Office has been able to undertake a program, involving the correlation of more than 3,000,000 observations of ocean-currents, sea-temperatures, and other cognate natural phenomena, covering the various oceans of the world. This will result in a reservoir of information for the study of oceanic circulation, the refreshing of existing charts and publications of the Office, and the compilation of ocean current-charts and sea temperature-charts that will be of inestimable value both to the mariner and the scientist.

In conclusion, I wish to express my appreciation to the Woods Hole Oceanographic Institution, the Bingham Oceanographical Laboratories, the Scripps Institution of Oceanography, the Department of Terrestrial Magnetism of the Carnegie Institution of Washington, and to the other scientific institutions which have continued to lend their valuable assistance to the Hydrographic Office. The Hydrographic Office, which I represent, welcomes the opportunity to make available to members of the Union any information that it may possess which might be of interest or service to them in the pursuit of their investigations.

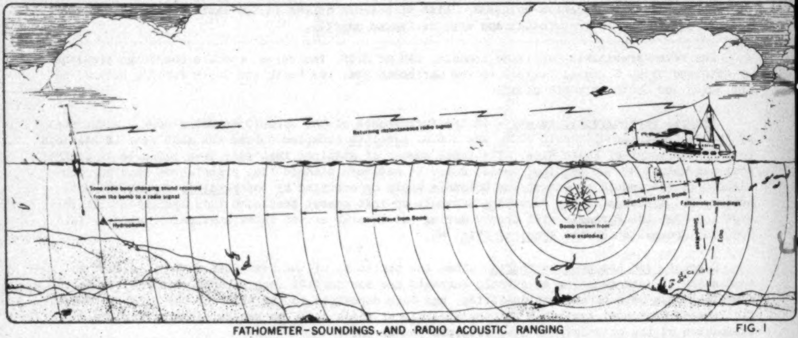
U. S. Hydrographic Office,
Washington, D. C.

OCEANOGRAPHIC ACTIVITIES OF THE UNITED STATES COAST AND GEODETIC SURVEY

L. O. Colbert

The oceanographic activities of the United States Coast and Geodetic Survey, during the past year, were notable for the successful utilization of three recent innovations developed in the Bureau. The sono-radio buoy, described in the Transactions for the Eighteenth Annual Meeting of the American Geophysical Union, was developed to the degree of efficiency that it is now no longer necessary to use station-ships for radio acoustic ranging. This results in a substantial saving of funds because a vessel manned with considerable personnel is supplanted by a mechanical buoy costing only several hundred dollars. It also obviates the need of subjecting the necessarily small vessels to the hazards of weather some 40 or 50 miles at sea. Figure 1 shows the method of using the sono-radio buoy in connection with a surveying vessel. The operation is similar to the radio-acoustic apparatus in a station-ship, but the buoy is entirely automatic. Heretofore, it was practicable to use only two station-ships, but the low cost of these buoys permits the use of three or more of them, furnishing additional distance-arcs for checking a position. The more powerful radio transmitters on the station-ships reach greater distances, but the buoys can be anchored farther offshore and in deeper water than it is practicable to anchor vessels; accordingly, experience has proved that surveys controlled by sono-radio buoys can be extended as far, if not farther, than with the use of station-ships.

Figure 2 shows the manner in which the ship Hydrographer controlled a survey of an area in excess of 15,000 square miles, off the Texas coast, during the past year. This control consisted, first, of a series of closed traverses using taut-wire measurements and Sun azimuths, with visual fix ties to signals at the inshore ends; and later, in the offshore area, of bomb-distances to sono-radio buoys. These traverses all closed with exceedingly gratifying accuracy. The sono-radio buoys gave strong control to the hydrography beyond the limits of visual fixes on the survey-buoys.


Returning instantaneous radio signal
Sono radio buoy changing sound received from the bomb to a radio signal
Hydrophone
Sound-Wave from Bomb
Bomb thrown from ship exploding
Sound-Wave from Bomb
Fathometer Soundings
Sound-Wave
Echo

FATHOMETER-SOUNDINGS, AND RADIO ACOUSTIC RANGING FIG. 1

The second innovation, shown in Figure 3, was the measurement of tides at sea by the use of the Dorsey Fathometer. The vessel was anchored with a short scope of chain over even and level bottom. The observations consisted of measuring the depth with the Dorsey Fathometer, every half-hour, over a period of hours. These readings were plotted and a smooth curve drawn through

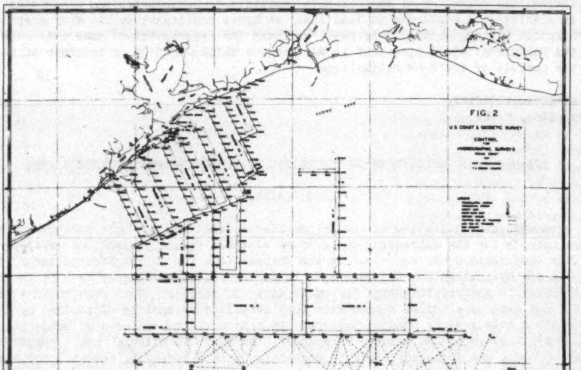
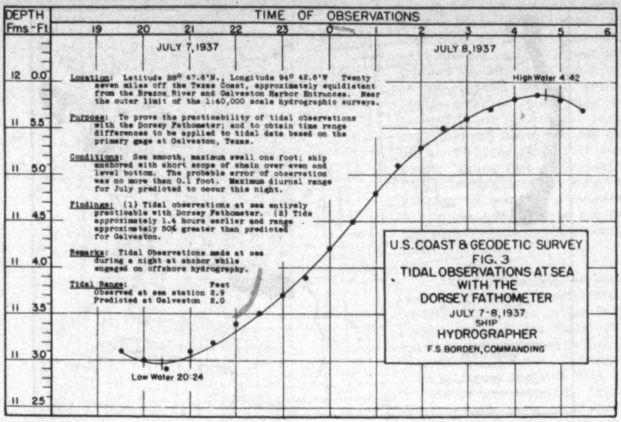

U.S. COAST & GEODETIC SURVEY
CONTROL
HYDROGRAPHIC SURVEYS

FIG. 2



Location: Latitude 28° 47.6'N., Longitude 94° 42.6'W Twenty seven miles off the Texas Coast, approximately equidistant from the Brazos River and Galveston Harbor Entrances. Near the outer limit of the 1:40,000 scale hydrographic surveys.

Purpose: To prove the practicability of tidal observations with the Dorsey Fathometer; and to obtain time range differences to be applied to tidal data based on the primary gage at Galveston, Texas.

Conditions: Sea smooth, maximum swell one foot; ship anchored with short scope of chain over even and level bottom. The probable error of observation was no more than 0.1 foot. Maximum diurnal range for July predicted to occur this night.

Findings: (1) Tidal observations at sea entirely practicable with Dorsey Fathometer. (2) Tide approximately 1.4 hours earlier and range approximately 50% greater than predicted for Galveston.

Remarks: Tidal Observations made at sea during a night at anchor while engaged on offshore hydrography.

Tidal Range:	Feet
Observed at sea station	2.9
Predicted at Galveston	2.0

U.S. COAST & GEODETIC SURVEY
FIG. 3
TIDAL OBSERVATIONS AT SEA
WITH THE
DORSEY FATHOMETER
JULY 7-8, 1937
SHIP
HYDROGRAPHER
F.S. BORDEN, COMMANDING

the points. The commanding officer stated that the probable error of the resulting typical tidal curve was not in excess of one-tenth of a foot. These observations were made 27 miles off the Texas coast. By comparison with observations at Galveston, times of high and low water were found to be about 1 hour and 24 minutes earlier than at Galveston and the range approximately 50 per cent greater.

Other vessels are being instructed to make similar observations during the coming year. It is expected that the results of these observations will furnish data needed to reduce soundings in offshore areas to the datum-plane, and information of considerable value for the determination of cotidal lines. Because tidal observations have been confined to the immediate vicinity of the coasts, the cotidal lines have heretofore been drawn only by making certain assumptions.

The third feature of the 1937 season was observation for magnetic variation at sea, using a non-magnetic whaleboat (Fig. 4). The four circles "B" in the illustration represent sea-anchors, suspended eight fathoms below the surface of the water and secured to outriggers from the whaleboat. These were used to reduce the motion of the boat by damping the effect of wave-action. A spherical type of compass "C" was used, mounted on a stand amidships. The boat steadied on 24 headings by use of a steering oar. The observations were made in the Gulf of Mexico, some 70 to 100 miles offshore. The results have furnished the best data for declination in this area, since the observations by the Carnegie. Obviously, favorable conditions of weather are needed, and this method will serve only as a temporary measure, pending the replacement of the Carnegie. Modern steel vessels are unsuited for magnetic observations, and it has been necessary, since the loss of the above-named vessel in 1929, to extrapolate from observations prior to that date in order to draw isogonic lines for chart purposes. This has given rise to many uncertainties for the navigator. New oceanic areas, where large disturbances occur, are constantly being found. One such area is off the coast of California in depths ranging from 50 to 700 fathoms where the declination varies from 11° east to 18° east within an area of 200 square miles.

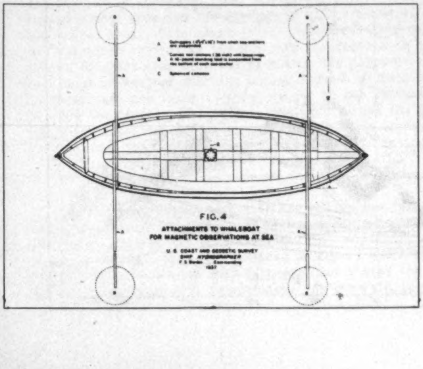


FIG. 4
ATTACHMENTS TO WHALEBOAT
FOR MAGNETIC OBSERVATIONS AT SEA
U.S. COAST AND GEODETIC SURVEY
SHIP HYDROGRAPHER
F.S. Borden Commanding
1937

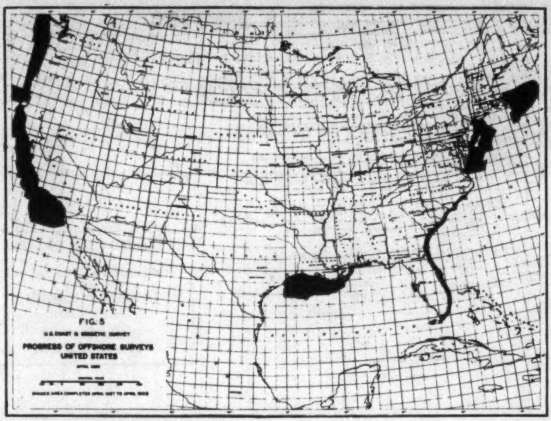


FIG. 5

U.S. COAST & GEODETIC SURVEY

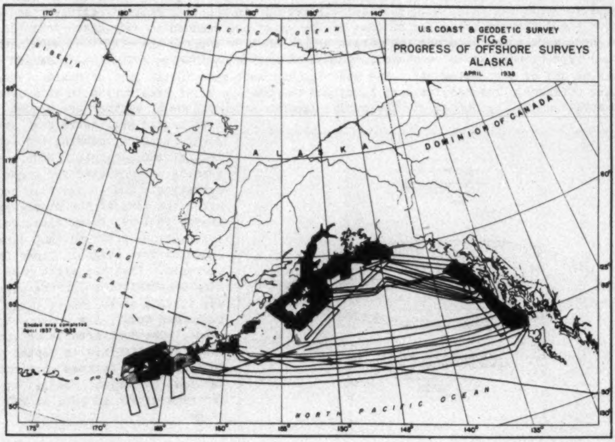
PROGRESS OF OFFSHORE SURVEYS
UNITED STATES

SHADED AREA COMPLETED APRIL 1937 TO APRIL 1938

The Bureau is vitally interested in the acquisition of a vessel for this work, either by new construction or by alteration of a suitable sailing vessel. It will be recalled that in 1905, at the request of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington, three officers were assigned from the Bureau for duty aboard the Galilee to act as commanding officer and observers during the first magnetic survey of the Pacific Ocean.

The foregoing features are simply details of the extensive oceanographic work of the Bureau during the past year, which are represented largely in offshore surveys on the Atlantic, Gulf, and Pacific coasts, and in Alaska.

Figure 5 shows the progress of the surveys along the United States coasts. On the Atlantic Coast the Oceanographer, Lydonia, and Gilbert accomplished the survey of a large area (shown by



U.S. COAST & GEODETIC SURVEY
FIG. 6
PROGRESS OF OFFSHORE SURVEYS
ALASKA
APRIL 1938

ARCTIC OCEAN
SIBERIA
ALASKA
DOMINION OF CANADA
BERING SEA
NORTH PACIFIC OCEAN
Shaded area completed April 1937 to 1938
170°
180°
170°
160°
150°
140°
175°
165°
155°
145°
135°
65°
60°
55°
50°

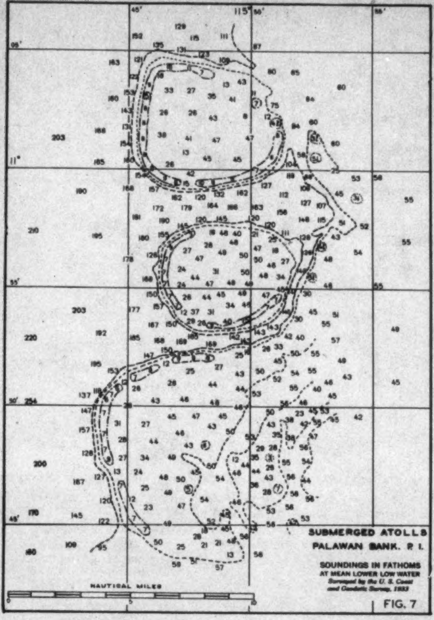


FIG. 7

the shaded portions) off the coasts of New Jersey and Delaware. The configuration of the ocean-bottom so well sounded in this survey has attracted the attention and interest of geologists. The data are so vastly superior to the old, both in intensity and accuracy of location, that they may originate new concepts of the geologic history of the region. During 1938 the vessels will continue this offshore work, completing the small area between the shaded portion and then commencing the work between the two northerly black sections.

The Hydrographer, supported by the tender Faris, accomplished the program of surveys in the Gulf of Mexico shown by the shaded portion, an area, as mentioned before, of over 15,000 square miles. During the coming season the work will be continued to the westward. Important fishing banks were found and preliminary inspection of the results, prior to a more thorough study, indicate that this area will be of interest to geophysicists and oceanographers.

The Pioneer completed the program of surveys off the coast of southern California.

The Guide continued the hydrography and wire drag work along the coast of northern California and during the coming season will resume this work.

The Explorer began new basic surveys of Sumner Strait, southeast Alaska, and will resume this working during the summer. In addition to the past season's work, revision surveys were made of Port Townsend, Wasnington, and Wrangell Harbor, Alaska. A resurvey of Sitka Harbor will be made in connection with the present year's program.

The work of the Surveyor and Discoverer is shown on Figure 6. In addition to the surveyed area (shaded), several sounding lines were run across the Gulf of Alaska en route to and from the Aleutian Islands. In 1938, the Surveyor will continue surveys to the westward of the westernmost shaded area and the Discoverer will proceed with surveys to the eastward of the easternmost shaded area. This season these vessels will be joined in this region by the Pioneer which will establish control to the westward in addition to making hydrographic surveys. As these surveys progress away from the mainland, the distance from fuel- and supply-bases and the severe conditions of weather make the accomplishment of this work a difficult task. Landing conditions are hazardous, and there is great danger to the personnel in landings through the surf. In order to avoid the dangers of frequent landings, during the past year the Commanding Officer of the Discoverer established camp-parties and arranged transportation by horses for the surveyors working along the south coast of Unimak Island. The horses were taken north from Seattle.

No additional work was done in the Hawaiian Islands.

The original basic surveys in the Philippine Islands are nearing completion. A junction was made in northeast Luzon with previous surveys, thus completing the program of surveys in this region, one of the most hazardous in the Philippines because it is directly in the general path of typhoons. Work was extended southward along the west coast of Palawan. This work was done by the commonwealth ship Fathomer, but during the current year the United States Coast Survey ship Pathfinder will be operated. As the Pathfinder is nearly twice the size of the Fathomer,

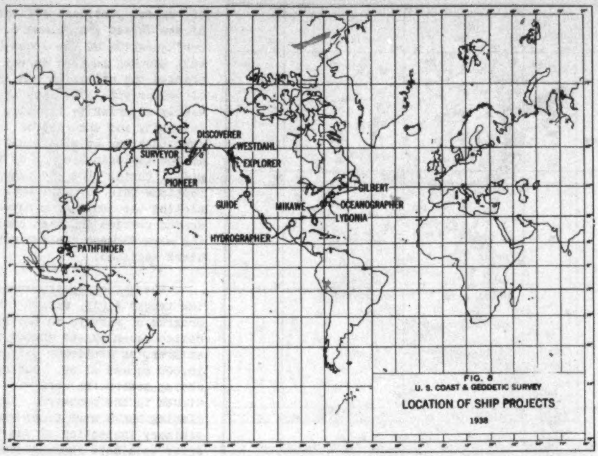


FIG. 8
U. S. COAST & GEODETIC SURVEY
LOCATION OF SHIP PROJECTS
1938

with much more fuel capacity, the work, which will be confined chiefly to extending the surveys southward along the west coast of Palawan, will progress much more rapidly. The west coast of Palawan, embracing Palawan Shelf which extends over 30 miles offshore, is one of the most interesting regions in the world in that the coastal shelf is covered with many coral knolls and submerged atolls. Some of the interesting formations of these submerged atolls are illustrated in Figure 7. The seaward limit of the Palawan Shelf is bordered by the deep water of Palawan Passage.

The field-work of the United States Coast and Geodetic Survey in tides, during the past year, was a continuation of its program of maintaining a number of primary tidal stations in continuous operation and securing short series of observations in connection with its hydrographic work and with special surveys.

At the present time the Survey has in operation 36 primary tidal stations, namely, 21 on the Atlantic Coast, three on the Gulf Coast, nine on the Pacific Coast, two in Alaska, and one in the Hawaiian Islands. It has been generally necessary to locate these stations in important harbors. There are, therefore, portions of the coast, especially on the Gulf and Pacific coasts, where primary tidal stations are needed, and it is hoped that such stations may be put into operation in the not too distant future.

It may be of advantage here to summarize briefly the important functions the primary tidal stations serve. Initially, these stations were established to furnish basic sea-level datums for the precise level-net of the country and to furnish data for predictions of tides and for tidal research. They also serve, however, to correct to mean values the results of short series of observations taken for the purpose of reduction of soundings and determination of chart-datums. In addition, they furnish a complete record of the vertical movement of the surface of the sea at their respective stations, including storm-effects. Finally, they furnish the data for the determination of changes in relative elevations of land and sea. This latter purpose needs emphasizing, for it is only through tide observations at primary tidal stations that the slow changes in relative elevations of land and sea can be evaluated.

For all the purposes enumerated above, it is essential that the observations at the primary tidal stations be as continuous as possible and that the records furnish precise data. This latter feature necessitates periodic leveling between bench-marks and tide-staff preferably not less than once a year.

At many of the tidal stations daily observations are made of the temperatures and densities of sea-water, and compiled data on temperature, density, and salinity of the sea-water are available for a number of localities. These observations, which are taken incidentally to the main purpose of the tide station, are secured at very little extra expense.

It may be of interest to note here that in order to obtain as large a number of tidal observations as possible with the limited funds available, many of the tidal stations are maintained in cooperation with other agencies. Stations maintained in cooperation with the United States Navy are located at Newport, Rhode Island; Annapolis, Maryland; Naval Operating Base and Norfolk Navy Yard, Virginia; Guantanamo Bay, Cuba; San Diego, California; and Bremerton, Washington. Stations maintained in cooperation with the United States Army are located at Old Point Comfort, Virginia; Southport and Wilmington, North Carolina; Savannah River Entrance, Georgia; Mayport and Miami Beach, Florida; and Stockton, California. The Lighthouse Service of the Department of Commerce is cooperating in maintaining stations at Washington, D. C.; Astoria, Oregon; and Ketchikan, Alaska.

The following oceanographic institutions are cooperating in maintaining tidal stations in their respective localities: Woods Hole Oceanographic Institution at Woods Hole, Massachusetts; Scripps Institution of Oceanography at La Jolla, California; Oceanographic Laboratories of the University of Washington at Friday Harbor, Washington; The Chesapeake Biological Laboratories at Solomons Island, Maryland, and the Biological Station of the Texas Fish, Game, and Oyster Commission at Rockport, Texas.

In addition to the primary tidal stations, short series of tidal observations have been taken at more than one hundred places along the coasts of the United States during the past year to provide data for use in connection with various surveys. The tidal datums determined from such observations are referenced to local bench-marks for the future use of engineers.

As tidal currents are intimately connected with the rise and fall of the tide, their study constitutes an essential part of the work of this Bureau. Their effect on oceanic life is of even greater importance than the rise and fall of the tide. To the navigator who may be carried off his course by an unsuspected current and to the engineer who has witnessed costly damages to marine structures and immense losses incurred in shore-erosion, the subject is of vital interest. As the securing of current-observations involves a relatively large expense, the work has been somewhat curtailed during the past year. However, a number of short series of observations have been obtained in connection with hydrographic surveys, in addition to observations taken in York River, Virginia, in cooperation with the Bureau of Fisheries; in Galveston Bay, Texas, in cooperation with the United States Army Engineers, and in Puget Sound, Washington, in cooperation with the State Health Department.

Current-observations can be taken from lightships while on their regular station at very little extra cost, and through the cooperation of the Bureau of Lighthouses many valuable series of observations have been thus obtained. In the near future it is planned to resume this work.

During the year tide- and current-observations were harmonically analyzed for a number of stations. The results are used in making tide- and current-predictions and also provide basic data for a student engaged in tidal research. The tabulation of the high and low waters and the hourly heights of the tide from the records of the primary tide-stations is a part of the routine work of the Office. These tabulations furnish a complete record of the stage of the tide at each of these stations, together with monthly and yearly variations. It is of interest to note that 1937 was a year of relatively high sea-level on all three coasts of the United States.

As far as practicable, the United States Coast and Geodetic Survey cooperates with scientific expeditions and explorations in the securing of tide- and current-data by loaning instrumental equipment for the purpose. Equipment was recently loaned to the Woods Hole Oceanographic Institution for obtaining tidal observations in the Bahama Islands opposite our primary tidal station at Miami Beach. These observations are to be used in connection with studies of the Gulf Stream. Instrumental equipment has also been loaned to the Louise A. Boyd Arctic Expedition, under the auspices of the American Geographic Society, for surveys in the vicinity of Spitzbergen.

In addition to the tide- and current-tables published annually by the United States Coast and Geodetic Survey, there was issued this year Special Publication No. 211, "Currents in St. Johns River, Savannah River, and intervening waterways." The latter is one of a series of publications containing a discussion of the observational data for a number of the principal waterways of the United States. It includes charts depicting the hourly current-movement in these waterways.

The publication of the descriptions and elevations of tidal bench-marks along the coasts of the United States has been continued. During the past year mimeographed pamphlets were issued covering the tidal bench-marks along the coasts of New Jersey, Florida, Louisiana, and the harbors of Los Angeles and Long Beach. There now remain but two States on the Gulf Coast and four States on the Atlantic Coast not covered by such publications and it is hoped to issue these in the near future.

Figure 8 is shown to furnish a visual picture of the scope of oceanographic work of the Bureau during the present year. The distribution of vessels, all in United States waters, extends for half the distance around the world. This wide distribution results in the accumulation of data, on the configuration of the sea-bottom, over an area which is no inconsiderable portion of the surface of the globe. The variety in geologic history of the regions embraced by the modern detailed surveys carried out by these vessels supplies a wealth of data which the Bureau is glad to make available to oceanographers and others interested in the Earth sciences.

U. S. Coast and Geodetic Survey,
Washington, D. C.

THE OCEANOGRAPHIC WORK OF THE UNITED STATES COAST GUARD IN 1937 AND PLANS FOR 1938

Floyd M. Soule

For the 1937 season the Coast Guard assigned the cruising cutters Champlain and Mendota to the duties of the International Service of Ice Observation and Ice Patrol. The 125-foot patrol boat General Greene was again designated as the oceanographic vessel of the Ice Patrol Force. Because of the unusually early date upon which it was necessary to begin the service of ice-observation, the cutter Cayuga made one ice-observation patrol, February 5 to 21. The ice-observation officer, who has charge of the scientific work on the patrol cutters, was Lieutenant G. Van A. Graves, and the oceanographer in charge of the scientific work on the oceanographic vessel was Floyd M. Soule. From several thousand temperature-reports received by the patrol cutters from passing steamers the ice-observation officer was enabled to maintain an up-to-date surface isothermal chart of the usually ice-infested area in the vicinity of the Grand Banks and to prepare nine fortnightly sea-surface isothermal charts which will appear in Coast Guard Bulletin No. 27.

The General Greene spent the months of April, May, and early June, supplying dynamic topographic charts to the patrol cutters Champlain and Mendota. During this period three such charts, based on as many surveys, were constructed with reference to the 1000-decibar surface although the temperature- and salinity-measurements usually extended to a depth of 1400 meters where the depth of water permitted. In this work 201 oceanographic stations were occupied. After the discontinuation of the Ice Patrol for the season, the General Greene made a post-season cruise on which 75 additional oceanographic stations were occupied between June 21 and July 12. This post-season cruise involved six sections approximately normal to the Labrador and Atlantic currents and designed to cross their common boundary at intervals from the Tail of the Grand Banks to northeast of Flemish Cap. Serial observations of temperature and salinity extended to a depth of 2000 meters, and in the region southwestward of Flemish Cap many of the stations were occupied to as near bottom as was practicable. Although the cruise was laid out primarily in the form of individual sections, rather than a close network of stations, the sections were so disposed as to permit the construction of a dynamic topographic chart which revealed at least the major features of the circulation-pattern. Also on the post-season cruise 849 fathometer soundings were taken. These soundings have been corrected, tabulated, and supplied to the United States Hydrographic Office and the International Hydrographic Bureau.

Since last year's meeting of the Union, Coast Guard Bulletin No. 19, part 2, by Ed. H. Smith, Floyd M. Soule, and Olav Mosby, and Coast Guard Bulletin No. 26 by Floyd M. Soule and G. Van A. Graves, have appeared in print. The former bulletin deals with the physical oceanography of the Labrador Sea and is based chiefly on the results of the northern cruises of the Marion in 1928 and of the General Greene in 1931, 1933, and 1934; while the latter bulletin gives the results of the 1936 ice-patrol season and the 1935 and 1936 post-season cruises of the General Greene. Coast Guard Bulletin No. 27, also by Soule and Graves, is now in press and contains a discussion of the 1937 season and post-season cruise results. The oceanographic part of this bulletin may be summarized as follows:

(1) The three current-surveys of the region immediately eastward of the Grand Banks made during the 1937 ice-season and the additional survey extending farther to the south, east, and

and north made on the post-season cruise have been shown in chart-form and the current-patterns found have been described.

(2) The seasonal variation in the volume of flow of the Labrador Current during 1937 has been quantitatively determined and compared with conditions existing during the previous three ice-seasons.

(3) The volume of water contributed eastward across the Grand Banks by the western branch of the Labrador Current which flows southward along the Avalon Peninsula of Newfoundland was found to have been greater than usual during 1937 and has been discussed in relation to data on icebergs and the 1935 flood of the West Greenland Current past Cape Farewell.

(4) The boundary between Atlantic Current water and Labrador Current water, on the basis of the horizontal projection of the course of the zero-salinity anomaly maximum, has been shown with relation to the position of the four-year average 1934-37.

(5) An abnormally large quantity of mixed water was found east of the Grand Banks during the 1937 post-season cruise and indicated the great variability in current-pattern to which this area is subject. This has been discussed with respect to extra-southerly berg-drifts.

(6) Temperature-salinity curves representing conditions found on the post-season cruise have been compared with those from other observations north of the Azores and in the Sargasso Sea, and indicate that the sections made on the post-season cruise did not completely cross the Atlantic Current.

(7) The post-season cruise presented additional evidence that a most nearly motionless level existed, in the region surveyed, at a depth of about 2000 or 2500 meters; and that below this level the deep water was in motion in a direction counter to the overlying Atlantic Current.

The program of oceanographic work for 1938 is already under way. At the present time the General Greene is engaged in supplying topographic dynamic charts of the Grand Banks Region to the patrol cutters of the International Ice Patrol as in previous years. This will continue until the end of the ice season whereupon the General Greene will begin a post-season cruise in the waters between Labrador and southwestern Greenland. Tentative plans for this cruise include an oceanographic section across the Labrador Sea from South Wolf Island, Labrador, to Cape Farewell, Greenland, a few widely spaced stations cutting through the region in which we believe bottom-water of the Labrador Sea is produced by vertical convection to bottom during winter, a section across Davis Strait in the vicinity of Holsteinsborg-Cape Walsingham, and possibly a short section across the Labrador Current about off Nachvac Fiord. Incidental to an extended tour of ice-observation along the American side, a program of closely spaced sonic soundings may give a clue to the presence or absence of submarine canyons in the Labrador shelf.

Bering Sea and Arctic Ocean

During the cruise of the Northland in 1937, over 350 oceanographic stations were established in the eastern portion of the Bering Sea, parts of the Bering Strait, and of the Arctic Ocean. The Commanding Officer of the Northland was assisted by two representatives of the University of Washington in handling the actual oceanographic work. A prearranged schedule had been prepared, checked by Dr. Thomas G. Thompson, and approved by the Commandant of the Coast Guard and the Commander, Bering Sea Patrol Force. The object of the cruise was to study: (a) Upper Bering Sea floor; (b) the chemistry of the Arctic Ocean floor off the west coast of Alaska; (c) the chemistry of various organisms and plants; and (d) the currents in those waters. In all of the stations, temperature- and salinity-measurements were made and observations on the various nutrient salts, as well as dissolved oxygen. The chemical analyses have been completed. The hydrodynamic calculations are now being made at the University of Washington. A résumé of the stations taken is as follows: (1) Dynamic sections 34, cycles taken 15; (2) stations occupied 215, casts taken 350; (3) samples taken for temperatures 950; (4) chlorinities determined 947; (5) determination of minor constituents--(a) silicate 892, (b) phosphate 918, (c) dissolved oxygen 812, (d) nitrogen 22, (e) current-observations at anchor 6.

While at Icy Cape a number of large pieces of ice were secured, placed in a refrigerator-room of the Northland and brought to the oceanographic laboratory, University of Washington, for test. A preliminary bacteriologic study of this ice indicates a good field of investigation. A general chemical study of the ice, which has not yet been completed, indicates that some of the previous observations of sea-ice, most of which were made about 40 years ago, may be in error.

The proposed program of study for 1938 will call for the reestablishment of some of the oceanographic stations taken in previous years in order to note the yearly variations in temperatures and the concentration of the nutrient salts. As the quantity of ice in the Arctic promises to be somewhat less than last year, it is proposed to establish a greater number of stations in the Arctic Ocean area. These stations will be an extension to the eastward of some sections occupied by the Russians off Siberia in the past several years. It is hoped to obtain observations in the deepest portions of the upper Bering Sea. It is also proposed to make an elaborate study of the bacteriology of the sea-ice and also the bacteria of the sea-water in the upper Bering Sea and Arctic Ocean. Various samples of ice will be collected for further chemical study. While in the oceanographic laboratory this winter, Dr. Stefansson conferred with the Commanding Officer of the Northland and Prof. Thompson of the University of Washington, on the desirability of chemical investigations of sea-ice with respect to age. This will be considered.

U. S. Coast Guard,
Woods Hole, Massachusetts

RESEARCH WITHIN PHYSICAL OCEANOGRAPHY AND SUBMARINE GEOLOGY AT THE SCRIPPS INSTITUTION OF OCEANOGRAPHY DURING APRIL 1937 TO APRIL 1938

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In the past year the researches of the Scripps Institution of Oceanography have, as previously, dealt with problems in physical oceanography, chemistry of sea water, sedimentation, and numerous aspects of the biology of the ocean. Studies have been conducted not only by the regular staff of the Institution, which has remained unaltered, but also by visitors who have spent from a few months to a full year at the Institution.

Time and space do not permit a discussion of the many researches within biological fields which are being conducted at the Institution, since emphasis will be placed upon progress within studies which are of direct interest to geophysics. In this place, therefore, it must be sufficient to mention that researches have continued on the bacteria of the sea, phytoplankton, zooplankton, on the biochemistry and physiology of marine organisms, and within fish biology. The Scripps Institution is also engaged in a cooperation with the United States Navy Bureau of Construction and Repair on a study of the organisms which cause fouling growth on ships' bottoms.

Considerable time has been devoted to the problem of a satisfactory method for determination of nitrates in the ocean. Experiments are being made with the method which a few years ago was proposed by W. R. G. Atkins and the results indicate that with slight modifications this method can be used for accurate and rapid determinations of the amount of nitrates.

Problems of sedimentation and of submarine geology have received great attention during the past year, since F. P. Shepard, who received a grant from the Geological Society of America for work in submarine geology, has been associated with the Scripps Institution from July, 1937. Accurately located soundings have been made at the heads of several submarine canyons for the purpose of discovering possible changes. A large slow-moving submarine landslide was charted during its occurrence at the head of La Jolla Canyon. Filling in has been noted in all of the canyons during the winter. Dredging and sounding operations have been undertaken off San Clemente Island, especially in the rift-valley southeast of the Island. Large amounts of rocks have been dredged from this trough and from the fault-scarps at either end. The formations have fossils which have been diagnosed as Miocene and Pliocene. One dredging brought to the surface fossil bones, principally of fish bones, but also containing those of mammals.

During the recent flood in southern California samples of water and mud were collected at the mouths of major streams at varying distances along the shore and out to sea. From the data so obtained it may be possible to obtain rough estimates of the amount of sedimentation during this period, as well as to gain information about rates of coagulation and settling under the conditions of rapid turbulent diffusion existing in the sea. Sediment-traps have been designed and put out in various places, and despite the loss of some of these devices encouraging results have been gathered. In the laboratory a comprehensive program of chemical and mechanical analyses of recent west-coast sediments has continued and is now being supported by a special grant from the Geological Society of America. In this work, Dr. Stina Gripenberg from Finland has rendered valuable assistance by developing a simplified procedure for the determination of organic carbon in sediments and soils.

A shallow-water type of the coring apparatus developed by Varney and Redwine, which utilizes for driving force the hydraulic pressure, has been successfully tested. Cores up to six feet long have been obtained in 100 fathoms of water. An instrument designed for use in water over 400 fathoms deep is now being developed.

Some of the above-mentioned work in submarine geology forms part of the special research undertaken by F. P. Shepard with the support of the Geological Society of America, but other studies, especially those of sedimentation, form an integral part of the Institution's program. It is hoped that a more complete knowledge of the character of the currents off the coast of southern California, of the organisms which contribute to the sediments, and of the fine material which is carried away from the coast, will make possible a better understanding of the present process of sedimentation and a better interpretation of the distribution of certain types of sediments.

Averaging and plotting by months and quadrangles of sea-surface temperatures in the Pacific Ocean have been continued. Investigation of trend of air- and sea-temperatures of the southern California area have continued for the purpose of providing advance indications of temperature. Similarly, study of advance indices of the seasonal rainfall of the Pacific Coast have been carried on. Theoretical studies dealing with the energy relations between the sea-surface and the atmosphere have been completed and studies have been commenced which may throw light on the mechanism involved in the maintenance of the typical distribution of temperature of the deep waters of the oceans. Several applications of statistical methods to biological problems have been developed.

Discussions of the data from the Panama area obtained on board the U.S.S. Hannibal in 1933-35 have been prepared for publication and the Hannibal data from the Caribbean Sea in 1936 made ready for discussion. Similarly, the data obtained between the coast of California and the Hawaiian Islands on board the U.S.S. Louisville and Minneapolis in December, 1936, have been arranged for publication and submitted to the United States Hydrographic Office. Eleven stations were occupied by the latter vessels at positions where the Carnegie had made observations in 1929 and the results from the two sets of stations have been found to be in good agreement.

At the end of the pier of the Scripps Institution a recording current-meter has been installed and it has been in operation for about two-thirds of the year, breaks having been caused by high waves. The records have been evaluated and the results correlated with the phase and range of the tide. An instrument for the study of the period and height of waves at the end of the pier has been completed which gives a continuous record and is in use for a short period each day.

The long continuous records of temperature and water-level at the end of the pier have been used for establishing a correlation between the surface-temperature and the sea-level. Low surface-temperature corresponds to low sea-level and vice versa. By means of oceanographic stations occupied at five and ten miles off the coast in previous years it has been shown that the variations in sea-level are closely associated with changes in the distribution of density of the water off the coast, and it is, therefore, possible that the records of sea-level can be used in a study of the coastal currents.

In last year's report, mention was made of cooperation which had been established between the California Fish and Game Commission and the Scripps Institution whereby three cruises were planned during which 6000 drift-bottles should be thrown out and about 30 oceanographic stations should be occupied along four lines running nearly at right angles to the coast, the northern starting off Point San Luis, and the southern off San Diego. The cruises were completed according to the plans but, owing to conditions of weather, only on the second cruise were all stations occupied. Only a small number of the drift-bottles have been found, but the oceanographic data which soon will be ready for publication have rendered much interesting information.

In the spring and early summer of 1937 a large counter-clockwise eddy was present off the coast of Southern California. During the first cruise between March 25 and April 3 the dynamic topography of the surface relative to the 500-decibar surface shows the presence of a current towards the southeast which, in the most northern part of the area, follows the coast-line and then continues outside the outlying islands. In the extreme southern part of the area the current turns around and a branch runs in the opposite direction at some distance from the coast, forming part of the big counter-clockwise eddy. Prior to the first cruise upwelling had not yet started, since the winds had been from westerly and southwesterly directions, but between this cruise and the second, north-north-westerly winds prevailed north of Point Conception and brought considerable changes in the character of the currents.

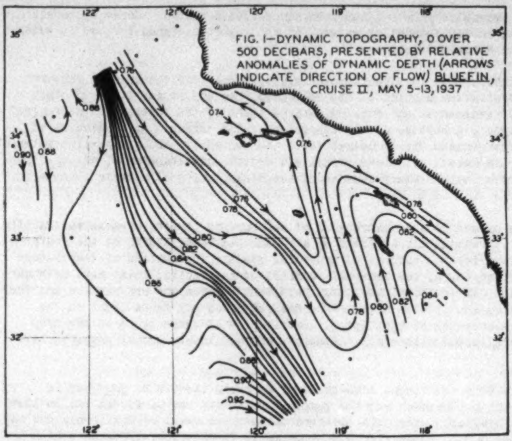

FIG. 1—DYNAMIC TOPOGRAPHY, 0 OVER 500 DECIBARS, PRESENTED BY RELATIVE ANOMALIES OF DYNAMIC DEPTH (ARROWS INDICATE DIRECTION OF FLOW) BLUEFIN, CRUISE II, MAY 5-13, 1937

In May (Fig. 1) the counter-clockwise eddy was even more conspicuously developed than at the end of March, but in the northern part of the area a conspicuous change had taken place. During the first cruise a weak and more or less irregular current was encountered off Port San Luis, but on the second cruise no current was present near the coast nor for a distance of about 60 miles. Beyond that distance a 15-mile-wide current was running parallel to the coast at a velocity of nearly one mile an hour. This change must have been caused by the prevailing winds which had transported the light surface-waters away from the coast. During this process, subsurface waters of low temperature had been drawn to the surface near the coast and the light top-layer had been banked up at a distance of about 60 miles from the coast. A vertical section (Fig. 2) showing the distribution of density along the northern line on the first two cruises indicates the types of motion which had brought about the changed distribution in density. The density is represented by σ_t, heavy lines show the distribution of March 25-27, and thin lines the distribution on May 5-6. The dashed lines with arrows indicate the character of the flow which has transported the surface-layers away from the coast and raised the subsurface layers near the coast.

By studying the vertical displacements of temperature-, salinity-, and oxygen-curves it is possible to find the average vertical velocities during the time-interval between cruises, and knowing the vertical velocities the horizontal flow can be computed by means of the equation of continuity. The result gives a new picture of the motion in a vertical plane during upwelling, which is shown in Figure 3 in a profile at right angles to the coast. The region of the swift current to the south has been indicated by crosses. This region moved away from the coast more slowly than the surface-waters were transported out by the wind and, therefore, descending motion was present at the coastal boundary of the current, and between the coast and the current an eddy with horizontal axis must exist. The axis of the eddy was at a depth of about 50 meters and the total depth of the closed circulation was about 80 meters. The greatest offshore velocity at the surface was 11 cm/sec, and the greatest velocity towards the coast was somewhat over 4 cm/sec. Since the offshore boundary moved away from the coast the eddy increased in dimension and was being fed from below. This process represented, in this case, the upwelling. The upwelling water was drawn to the surface only near the coast but was transported away from the coast by the prevailing winds. It is

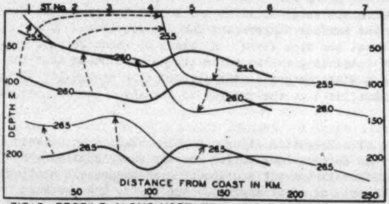

FIG. 2—PROFILE ALONG NORTHERN LINE SHOWING DISTRIBUTION OF DENSITY (σ_t) MARCH 25-27, 1937 (HEAVY LINES) AND MAY 5-6, 1937 (THIN LINES)

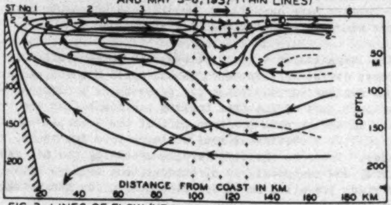

FIG. 3—LINES OF FLOW (HEAVY LINES) IN VERTICAL PLANE AT RIGHT ANGLES TO COAST DURING TIME-INTERVAL BETWEEN BLUEFIN CRUISES I AND II, 1937 (HORIZONTAL VELOCITIES IN CM/SEC REPRESENTED BY THIN LINES)

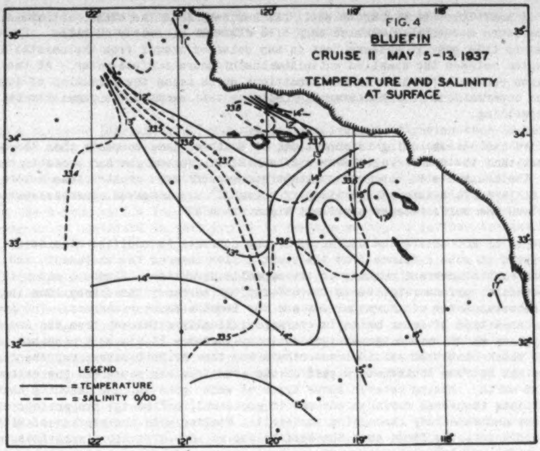

FIG. 4

BLUEFIN

CRUISE II MAY 5-13. 1937

TEMPERATURE AND SALINITY AT SURFACE

122°
121°
120°
119°
118°
35°
34°
33°
32°
33.6
33.5
33.7
33.4
13°
14°
15°
16°
17°
LEGEND
= TEMPERATURE
= SALINITY o/oo

seen that the upwelling was limited to the upper 200 meters and this result, as well as the magnitude of vertical displacement, agrees with previous results obtained by McEwen and others.

The upwelling water also spread south, as evident from the horizontal distribution of temperature and salinity at the surface (Fig. 4). A tongue of low temperature extended towards the south from the area off Point Conception, but in course of time the surface-temperature rose owing to absorption of heat, and the returning branch of the big counter-clockwise eddy was characterized by high surface-temperatures.

The picture of the upwelling which is shown in Figure 4 probably does not represent conditions which can prevail for any length of time. On the third cruise between June 25 and July 2

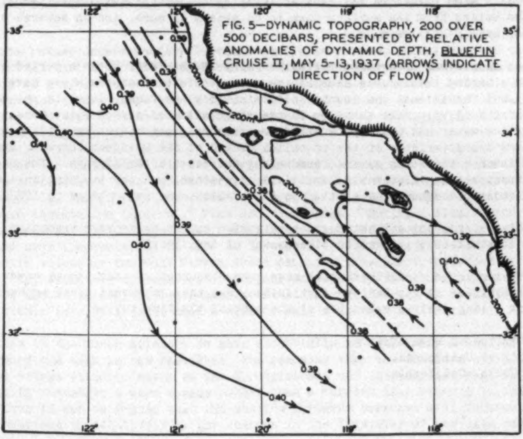

FIG. 5—DYNAMIC TOPOGRAPHY, 200 OVER 500 DECIBARS, PRESENTED BY RELATIVE ANOMALIES OF DYNAMIC DEPTH, BLUEFIN CRUISE II, MAY 5-13, 1937 (ARROWS INDICATE DIRECTION OF FLOW)

122°
121°
120°
119°
118°
35°
34°
33°
32°
0.39
0.38
0.40
0.41
200m

the region of swift current had moved still farther away from the coast, but inside of this region was now found a counter-clockwise eddy of a diameter of nearly 40 miles. From the character of the water in this eddy it appears that it has detached itself from the coastal side of the boundary-region between the upwelled water and the offshore surface-water. At the present time no suggestions can be offered as to the conditions which cause the formation of such an eddy, but the numerous observations from Monterey Bay indicate that such eddies regularly occur during a period of upwelling.

The third cruise was, owing to conditions of weather, less complete than the preceding, but it is evident that the counter-clockwise eddy was still present and had probably reached greater intensity. The contrasts in the surface-temperatures were even greater than before, since off Point San Luis a surface-temperature slightly below 11° was observed, whereas southeast of San Clemente Island the surface-temperature was higher than 20°.

At a depth of 200 meters the character of the currents is modified considerably. On each cruise one found at some distance from the coast a flow towards the southeast, and inside of this a narrow counter-current running in the opposite direction. Figure 5 shows the topography of the 200-decibar surface relative to the 500-decibar surface; the dashed line indicates the boundary-line between the offshore current and the inshore counter-current. The inshore counter-current carries a type of water which is characteristically different from the water of the offshore current, as shown by the temperature-salinity curves. It has been possible to define two water-masses which have been called the southern and the northern water, representing the water which enters the extreme southeastern part of the area from the south and the extreme northwestern part from the north. Mixing between these types of water goes on, and assuming that this mixing takes place along isopycnal surfaces one can in any locality find the proportions of the two types of water and can study the mixing in detail. Dealing with the process of mixing as a problem of diffusion, one finds that the coefficient of eddy-diffusion is in this case of the order of magnitude of 10^6, which corresponds to the order of magnitude of similar diffusion in the atmosphere.

The results of the cruises of the Bluefin, of which the more outstanding have been briefly described here, clearly show that off the coast of southern California one finds an area within which great contrasts are present and within which the prevailing winds exercise a dominating influence upon the character of the surface-currents. The area lends itself, therefore, especially well for a study of non-stationary currents. Since such large contrasts are present in the physical conditions, it appears probable that similar contrasts are found in the biological characteristics of the area. The Scripps Institution, therefore, plans to continue the study of this area and to broaden the scope of the investigations. During the year 1938 it is planned to undertake six cruises with the new research vessel of the Institution, the E. W. Scripps, in order to cover this same area. On all cruises, samples of phytoplankton will be collected at intervals of ten meters from the surface down to 60 meters or more, and on several of the cruises hauls for zooplankton will be made.

The first cruise was undertaken during February 15-25, 1938, after a period of storms from the southwest. During this cruise some of the characteristic features which have been described were present, but the axis of the counter-clockwise eddy was found closer to the coast than on the cruises of the Bluefin, and the flow to the north extended beyond Point Conception. Evidently, light surface-water had been transported towards the coast by the prevailing winds and the conditions were characteristic of the so-called period of the Davidson Current, during which an inshore flow towards the north can be found along the coast of California. The second cruise in April was interrupted by unfavorable conditions of weather, but the two station-lines which could be completed indicated conditions similar to those which were encountered in 1937.

The studies of the phytoplankton and zooplankton appear to be very promising, but at the present time it would be premature to discuss any of the findings.

The above-mentioned researches which have been discussed at some length cover only a small part of the activities of the Scripps Institution, but they have been given emphasis here since they represent a long-desired expansion of the work of the Institution.

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A PROMISING THEORY CONCERNING THE CAUSES AND RESULTS OF LONG-PERIOD VARIATIONS IN THE STRENGTH OF THE GULF STREAM SYSTEM

C. O'D. Iselin

(Contribution No. 180 of the Woods Hole Oceanographic Institution)

A year ago I outlined [Iselin, 1937] the new cooperative investigation that is being conducted by the Woods Hole Oceanographic Institution and the Bermuda Biological Station. This investigation has as its ultimate goal the solution of the problem of whether or not gradual changes in the strength of the Gulf Stream System have any bearing on the climate and fishery of Great Britain. In an article soon to appear in "Nature" the program of observations that will be carried out during the 5-year period for which funds are available has been discussed more fully. However, one suggestion contained in that report is perhaps worthy of further treatment at the present time.

Assuming that the North Atlantic Eddy on the whole fluctuates in strength from year to year by a significant amount, are any of the existing theories capable of pointing to the causes and the most important results of such variations? This question had to be carefully considered before working out a program of observations that might be expected to yield satisfactory results. In the light of modern oceanographic knowledge no existing theory seemed entirely adequate. However, as will be discussed below, it was possible to work out a compromise theory that at present seems promising enough to justify the proposed 5-year program of work at sea.

Most investigators are agreed that the energy of the Gulf Stream System is absorbed in low latitudes from the trade-winds. Now there is little convincing data to show whether or not the trade-winds in the Atlantic do on the whole vary in strength from year to year. All that can be said is that it seems probable that the variations are quite small. Therefore, if it is assumed that the energy imparted to the water is proportional to the strength of the winds, it is difficult to see how any significant fluctuations will result in the current-system.

However, some recent studies of Professor C.-G. Rossby have pointed to a mechanism whereby relatively small changes in the strength of the wind-system over the North Atlantic might cause marked fluctuations in the volume of the currents. He has advanced evidence that there exists a critical wind-velocity below which the air slips easily over the surface of the water. But once the sea-surface has reached a certain roughness, eddies are set up in the lowest layer of the atmosphere and the coefficient of friction suddenly greatly increases. If this can be established by suitable observations at sea and if it is found that the critical velocity falls within the range of the variations in strength of the trade-winds, then it is clear how the current-system can absorb very much greater amounts of energy in years when the winds are only slightly stronger than average.

The available rather unsatisfactory evidence [Montgomery 1938] indicates that the Gulf Stream has a seasonal variation in strength that results in January and July in an increase in volume of roughly 15 per cent over the minimum values of April and October. But it remains to be established whether or not there exists annual variations of the same order of magnitude. On the whole, however, in view of Rossby's theory mentioned above it seems worth while to look for significant annual changes in the strength of the North Atlantic Eddy.

In the case of the continental slope of North America between the Florida Straits and the Grand Banks, the influence of long-period variations in the Gulf Stream seems relatively straightforward and direct. As the current decreases in strength, bottom-temperatures in the depths occupied by the main thermocline layer will rise and vice versa. During a single year, for example, north of the current off Chesapeake Bay the average depth of the 8° isotherm is known [Iselin, 1936] to have fluctuated by as much as 70 meters. Concerning these effects at mid-depth of changes in volume of the Gulf Stream there can be little doubt, provided it is admitted that the current approximately follows the principals embodied in the circulation theorem of Bjerknes. But it is the possible effects nearer the surface of changes in strength of the North Atlantic Anticyclonic Eddy as a whole that I wish to call attention to.

Assuming that in the North Atlantic we have essentially an eddy, narrow and intense on the west side and broad and weak on the east side, and assuming that the northern off-shoot from this eddy, which brings Atlantic water to the Norwegian Sea and the Labrador Sea, is relatively shallow and that it resembles a warm tongue rather than a current that behaves in the classical dynamic sense, then it can be argued that the shallow northern branches will fluctuate greatly in strength in response to relatively slight changes in the intensity of the main eddy. These assumptions do not seem unreasonable in view of what is known about the distribution of temperature and salinity in the northeastern North Atlantic. The deeper waters, carried eastward by the continuations of the Gulf Stream, appear to cross the mid-ocean ridge north of the Azores and then curving southward to be absorbed into the beginning of the Northern Equatorial Current, while near the surface a shallow relatively warm tongue seems to advance towards the northeast more or less as pictured in LeDanois' transgression-theory.

Now if the main North Atlantic Eddy should on the whole increase in strength, the circulation-theorem tells us that the dynamic height at the center of rotation must increase, which is only another way of saying that the superficial, warm-water layer must increase in thickness. Unless the volume of warm water is at the same time increased, then the eddy must decrease in diameter.

On the other hand, if the strength of the eddy should decrease, then the dynamic height at its center must fall and either the eddy must increase in diameter or some of the warm superficial water of the central region must be discharged beyond the limits of the deep, circular current-system. Because it seems probable that the volume of warm water at the center of the North Atlantic Eddy does not fluctuate much and since the size to which the Eddy can expand is restricted by the shape of the ocean-basin, it is possible that with a decreasing current-system warm water in large amounts will be discharged to the north. It can be argued that this mechanism will cause very marked fluctuations in the warm northern branches which have been thought to originate from the Gulf Stream, but the relationship is exactly the reverse from that popularly accepted. Under this theory the discharge of warm water to the north will increase when the Gulf Stream and the North Atlantic Eddy are diminishing in strength and not when they are increasing in transport. It should be added that this is also the correct relationship under Ekman's wind-current theory. Easterly winds, which can be expected to retard the North Atlantic Current, would also tend to force warm water northward and westerly winds would drive the surface-water southward.

A relatively simple test of this reasoning will be carried out during the next few years. The changes in dynamic height in the Bermuda region will be followed at selected stations that will be occupied periodically and it should be possible thereby to learn whether or not a decrease in thickness of the central mass of warm water is indeed accompanied by rising sea-surface temperatures in the northeastern area and a strengthening of such movements as the Norwegian Atlantic Current.

If the mechanism outlined above is at all valid, the volume of warm water that must be discharged to the north of the main North Atlantic Eddy during periods of decreasing circulation is surprisingly great as a simple calculation will show. Supposing that the main thermocline at the center of the eddy must rise by 50 meters, which in theory should accompany a general decrease in volume of the surrounding circulation of approximately only 15 per cent, then assuming a mean diameter for the eddy of 2400 km, it turns out that the northern branches of the Gulf Stream System might be strengthened by approximately 226,000 cubic km during the period of the decline. On the other hand, an increase in strength of the North Atlantic Eddy by a similar small percentage might cause a temporary interruption of the movements towards the north with significant consequences to surface-temperatures in the northeastern area.

The chief justification for such speculation is that if the apparently quite steady thermal transfer of the ocean-currents can occasionally cause important climatic and biological variations and if these changes are to be predicted, we must find some mechanisms whereby relatively small variations in the strength of the anticyclonic wind-system over the North Atlantic can produce a relatively great fluctuation in the transport of warm water to the northeastern area.

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THE REPRESENTATION OF SUBOCEANIC RELIEF ON MAPS OF INTERMEDIATE SCALE

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The maps of most frequent occurrence that give information as to the configuration of the ocean-floor are generally at the two extremes of scale, being either coast-charts showing numerous spot-soundings, often by stippling grouped into depth belts, or general bathymetric maps of the oceans, usually with gradational tints. Examples of the former are the coast-charts of the great hydrographic offices of the world, such as the 1:10,000 to 1:80,000 series of our Atlantic, Gulf, and Pacific shores by the United States Coast and Geodetic Survey or the British Admiralty charts of British and foreign coasts on similar scales. Examples of the latter are the General Bathymetric Map of the Oceans on an equatorial scale of 1:10,000,000 [see 1 of "References" at end of paper]; Groll's three maps of the Atlantic, Pacific, and Indian oceans, each 1:40,000,000 [2]; Schott's maps of the Atlantic, 1:30,000,000, and of the joint Pacific and Indian oceans, 1:60,000,000 [3]; the recent map of the Atlantic, 1:20,000,000, in the scientific results of the Meteor Expedition [4]; the new map of the South Pacific, equatorial scale 1:27,000,000, by the United States Hydrographic Office [5]; and the American Geographical Society's map of Antarctic waters, 1:20,000,000 [6].

The hitherto undisclosed greater variety of relief that has been revealed by echo-soundings, both as to the major lineaments of the deep-sea basins themselves and as to the more detailed features of their landward margins, such as submarine valleys incised on continental slopes and structural troughs fronting on island-arcs, lends itself especially well to representation on maps of intermediate scale, since these afford a scope that is sufficient and yet that is not greater than is justified by the available detail. On intermediate scales are the general charts published by various hydrographic offices, such as the 1:1,200,000 to 1:1,600,000 series of the United States Coast and Geodetic Survey, but these, being intended for the navigator rather than the oceanographer, rarely carry other than spot-soundings alone [7]. However, to afford a synoptical view of submarine relief, a synthesizing method of representation is needed, such as the use of submarine contours with bathymetric tints.

International map of the world--A series that fulfills these conditions is the International Map of the World on the scale of 1:1,000,000. Although this map is primarily devoted to the land-surface of the Earth, many of its sheets include considerable portions of the ocean, and its governing principle of relief representation both of the land and of the sea by means of contours reinforced by height and depth tints makes it a bathymetric map in its water-areas. The series provides for a total of about 2100 sheets. Between the 60th parallels the sheet-area covers 4° in latitude and 6° in longitude. North and south of these parallels the longitude-extent of sheets may be increased to two or more multiples of 6°. Of the total of 2100 sheets it has been estimated that about 1000 sheets would be wholly oceanic and about 400 wholly terrestrial. Hence about 700 sheets would include both land and sea on the same sheet. As it is unlikely that the 1000 wholly oceanic sheets will be undertaken within an appreciable time, if at all, the effective program of the series is limited to about 1100 sheets. Of the 1100 sheets about 330 sheets have been published so far, and of these about 200 include both land and sea on the same sheet.

Bathymetry on that map--The regulations for the preparation of the International Map were established and developed at three official conferences, held at London in 1909, at Paris in 1913, and at London in 1928. As regards bathymetry the regulations laid down at the first conference seem to have taken into consideration shallow depths only. "The form of the bed of the sea or of a lake shall be shown by blue contours, the vertical intervals being normally 100 metres, but intervals of 10, 20, or 50 metres may be used" [8]. The accompanying sheet of conventional signs shows the sea-level contour and submarine contours at 100 and 200 meters and gives three tints of blue for the depths defined by those contours, the inference being hence permitted that all depths greater than 200 meters should be colored with the deepest of the three tints. The prescribed 100-meter interval can hardly have been intended to apply to great depths since much larger intervals were chosen to portray the far better known relief of the land.

The second conference included in its delineation the consideration of the whole question of the representation of suboceanic relief. Applying to the sea the principle of obligatory "principal contours" and optional "auxiliary contours" it had established for the land, the conference adopted the following resolutions as to bathymetry: "Principal sea- and lake-bed contours shall be drawn at depths of 100, 200, 500, and 1000 metres, and then at 1000-metre intervals. Auxiliary sea- and lake-bed contours may be added if necessary between 1 and 100 metres, at the same regular intervals (10, 20, or 50 metres) as on land. The representation of the sea-bed by contours shall be supplemented by soundings, particularly at great depths" [9]. Six tints of blue, defined in the text and shown on the sheet of conventional signs, grouped the

Table 1--Bathymetric scale for the International Map of the World

According to Paris Conference, 1913				
Obligatory principal contours	Optional auxiliary contours			Tints (intensity increasing with depth)
0	(3 alternatives[a])			
	10			
	20	20		
	30			
	40	40		1st blue
	50		50	
	60	60		
	70			
	80	80		
	90			
100				
200				
500				2nd
1000				3rd
2000				4th
3000				
4000				5th
5000				
6000				
7000				6th
8000				
9000				
10000				

As proposed by Stocks, 1936			
Obligatory principal contours	Optional auxiliary contours	Tints (intensity increasing[b] with depth)	Major relief form
0			
10[a]	Also, any critical contours needed between 0 and 200 meters	1st yellow	Continental shelf
20			
100			
200		2nd	
500		3rd	
1000		1st green	
1250[c]		2nd	
1500			Continental slope and rises
1750		3rd	
2000			
2250		4th	
2500			
2750		1st blue	
3000			
3250		2nd	
3500			
3750		3rd	
4000			
4500		4th	
5000		5th	Deep sea
5500		6th	
6000		7th	
7000		1st violet	Troughs
8000		2nd	
		red	

[a] See reference [8] at end of paper.

[b] Except for the greens, which on Stocks's map decrease in intensity with depth, although his text, Table 3, p. 14, has them increase.

[c] Based on an incidental reference in Stocks's text, p. 13, advocating "250-meter contours in depths greater than 1000 meters." The lower limit seems set by his statement that a 500-meter interval is to be used in representing the deep sea, which he defines as extending from 4000 to 6000 meters.

depths outlined by these contours into the following layers: 0-200, 200-500, 500-1000, 1000-3000, 3000-5000, over 5000 meters. The third conference made no changes in these regulations [10].

Stocks's proposed modification--The regulations of the second conference, while constituting a great advance over those of the first conference, are yet susceptible of improvement, as has recently been pointed out by Stocks [11], in view of the whole development of echo-sounding and the growth of dynamic oceanography that have since intervened. The wealth of new data on relief provided by echo-sounding not only calls for more detailed representation, that is, a smaller contour-interval, but it also has affected our general concepts concerning submarine relief in dispelling the former notion of the dominant uniformity of the ocean-floor. These major aspects hence should be taken into account in devising any bathymetric gamut, and the representation on an individual sheet should be treated on the background of the broad features of relief of the larger ocean-area of which it forms a part.

The salient new features of Stocks's proposal are the introduction of a 500-meter contour-interval, the expansion of the depth-tint-producing colors from one to five, and the grouping of depths by these colors to express the major types of submarine relief. To illustrate his scheme the author has prepared an equivalent of sheet South 0-26 of the International Map, hitherto unpublished, and incorporated these features in it. This sheet includes the South Sandwich Islands of sub-Antarctic waters, and in its range from high volcanic peak to abyssal trough encompasses a wide difference in elevation.

The gradational scale of Stocks's proposal is compared with that of the International Map in Table 1.

From Table 1 the first impression of Stocks's gradational scale is perhaps that it is over-detailed. When it is remembered, however, that such a map as the General Bathymetric Map on one-tenth the scale carries all the contours that are prescribed for the International Map and that even such small maps as Schott's Atlantic, on one-thirtieth the scale, and his Indian-Pacific, on one-sixtieth, include them all except the 500-meter contour, then it is apparent that Stocks's suggestion of a 500-meter interval between the depths of 500 and 6000 meters is both legitimate and feasible. Indeed, an inspection of Stocks's map shows that it portrays extremely well the enormous range of submarine relief in that area. Even the practical question of color printing offers no obstacles since the 17 bathymetric tints (as well as the 6 hypsometric tints) are apparently produced by means of only three color-plates--yellow, blue, and red.

In view of these circumstances Stocks's proposal seems well worth serious consideration by the International Map Committee and recommendation for adoption, if approved.

References

[1] The present 3rd edition is being published by the International Hydrographic Bureau in Monaco. Four of a total of 24 sheets have so far appeared. The map was initiated by the oceanographer Prince Albert I of Monaco, and the first two editions (early 1900s and 1912-30) were published by his Oceanographical Institute in Paris. Beginning with the second edition, the hypsometry of land has also been shown.

[2] Accompany Veröffentl. Inst. für Meereskunde zu Berlin, New Series, No. 2, Berlin, 1912. Wall-map versions in 1:20,000,000 were published in Brunswick in 1915.

[3] In his "Geographie des Atlantischen Ozeans," Hamburg, 1912 (2nd edition, 1926) and "Geographie des Indischen und Stillen Ozeans," Hamburg, 1935.

[4] Wiss. Ergeb. der Deutschen Atlantischen Expedition auf dem ... Meteor, v. 3, Part I.

[5] On back of Pilot Chart of South Pacific Ocean for June-July-August, 1938.

[6] Accompanies W. L. G. Joerg's "Brief history of polar exploration since the introduction of flying," Amer. Geogr. Soc. Spec. Publ. No. 11, 2nd revised edition, New York, 1930. A revised edition of the map, reduced to 1:25,000,000, appeared as Pl. 1, Geogr. Rev., v. 22, 1932.

[7] This feature was stated above to be characteristic of large-scale coast-charts. Examples of large-scale charts which exceptionally employ more synoptical modes of representation are the chart of Georges Bank in two sheets on the scale of 1:225,000 published by the United States Coast and Geodetic Survey, on which the soundings are stratified by blue tints, and the forthcoming series of submarine contour-maps by A. C. Veatch in 1:120,000 showing the submarine valleys on the continental shelf from the Gulf of Maine to the Chesapeake entrance (contour-interval five fathoms on the shelf and 25 fathoms on the continental slope), to be published by the Geological Society of America, of some of which the preliminary drafts were exhibited at the annual meeting of the Division of Geology and Geography of the National Research Council at Washington on April 30, 1938 (see also Trans. Amer. Geophys. Union, 18th annual meeting, 1937, p. 203, Fig. 4, and text p. 226).

[8] "Resolutions and Proceedings of the International Map Committee Assembled in London, 1909," London, 1910, Resolution 6e on p. 4. The wording seems clearly to indicate that 10-, 20-, or 50-meter contour-intervals are meant and not 10-, 20-, or 50-meter contours. The same provision is repeated, slightly more specifically, in the regulations of the second conference (see text below). Hence on our Table 1 three alternative sets of contours between 0 and 100 meters are indicated, namely, 10, 20, 30 meters, etc., 20, 40, 60 meters, etc., and 50 meters. Provision for so small intervals seems reasonable in view of the wide extent of the continental shelf in such areas as the Grand Banks, the North Sea, the Patagonian shelf, the Yellow Sea, the Malayan shelf-sea, and Arafura Sea. On the other hand, this regulation seems to have been interpreted by Stocks [11, pp. 10 and 13] as referring to the contours and not to the intervals, and in this form to have been taken over into his own system. This interpretation has been represented on Table 1. Weight is lent this interpretation by the sheet of conventional signs that accompanies the proceedings of the second conference [9], on which the symbol for auxiliary contours is illustrated by the 10-, 20-, and 50-meter contours in juxtaposition.

[9] "Comptes Rendus des Séances de la Deuxième Conférence Internationale, Carte du Monde au Millionième, Paris, décembre 1913," with separate case of plates, Paris, 1914, Resolution 7f and 7g on p. 112.

[10] "Report of the Meetings of the Commission on the Carte du Monde au Millionième [London 1928]," in "Carte du Monde au Millionième: Rapport de 1928," Bureau Central, Southampton, England, 1929, pp. 4-7, 23-29, and 43-44.

[11] Theodor Stocks, Die Darstellung des Meeres auf der Internationalen Weltkarte 1:1,000,000, Wiss. Veröffentl. Deutsch. Museums für Länderkunde zu Leipzig, New Series, No. 4, 1936, pp. 5-16, with equivalent of sheet South O-26 of the International Map.

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DEEP-SEA MEASUREMENTS WITHOUT WIRES OR CABLES

Maurice Ewing and Allyn Vine

In the deep-sea seismic measurements which were commenced last summer on the *Atlantis*, the principal problems for which no good solution is in sight are connected with the cable. It seems likely that the best solution is to dispense with the cable altogether.

Figure 1 shows the apparatus as used last year. It consists of a series of bombs, a series of seismographs, and an oscillograph-chamber. The cable leading from the ship to the oscillograph is a wire rope which serves only to lower and hoist the apparatus. It is necessary to place the 600-pound oscillograph-chamber on the bottom of the ocean in such a way that the 4200-foot string of seismographs and bombs shall lead away from it in a line which is very nearly straight. The actual distances from each bomb to each seismograph are determined from the travel-time of the water-wave. Only three tests have been made to date, but it is revealed by the water-waves that the line of instruments did not extend to nearly its full length in any test. In one test the 20-pound bomb was actually less than 600 feet from the oscillograph. Perhaps further practice in the maneuver of laying out the apparatus would overcome this difficulty to some extent, but it will always be difficult to stretch this line out straight without parting it. This is the first problem to be solved.

After the apparatus is on the bottom a timing mechanism inside the oscillograph-chamber operates the amplifiers and oscillograph, and detonates the three bombs at intervals of about 90 seconds. During this period it is absolutely essential that no disturbances be transmitted down the cable to disturb the seismographs. To avoid these disturbances it is necessary to pay out an excess of cable, but a slack cable takes kinks when hoisting begins. After three tests the cable showed serious damage from kinks, indicating a short probable life for the cable. Even so, the results of one of the three tests were seriously impaired by disturbances from the roll of the ship which were transmitted down the cable. Elimination of disturbances of cable without incurring serious damage to the cable through kinking is the second problem. Captain N. H. Heck has suggested that the disturbances from roll of the boat may be diminished by a spring-device between the cable and the ship.

It is proposed to solve these two problems by using a float or balloon filled with oil or wax of low density to replace the cable for lowering and hoisting. For several years the authors have been considering the advantages to be gained by using buoyant bodies for lifting apparatus from the depths of the ocean, but no suitable substance for providing the buoyancy suggested itself. The point was discussed with experienced oceanographers who agreed that buoyant devices would be useful, but that most buoyant materials became ineffective at depths. The idea of using oil for buoyancy was suggested by an article in the newspapers of some five or six months ago, in which Professor Picard described a proposed man-carrying bathysphere which would use an oil-filled free balloon instead of cable for support.

In Figure 2 is a schematic diagram of the proposed apparatus. The ballast is sufficiently heavy to take the apparatus to the bottom and the kite gives enough lateral movement to string the apparatus out in a straight line. A small-scale model of the balloon, kite, ballast, and line of apparatus has been built. This model functioned perfectly and laid the string of apparatus out in a straight line on the bottom of the pool in which it was tested. After the apparatus has reached bottom and performed the seismic measurements under the automatic control-system used previously, release of the ballast will permit the balloon to bring the apparatus to the surface.

The problem of locating the apparatus after it has reached the surface is a serious one. A mast ten or fifteen feet high and a system of signal-emission--either acoustic, radio, or visual--can be provided to aid in finding the balloon.

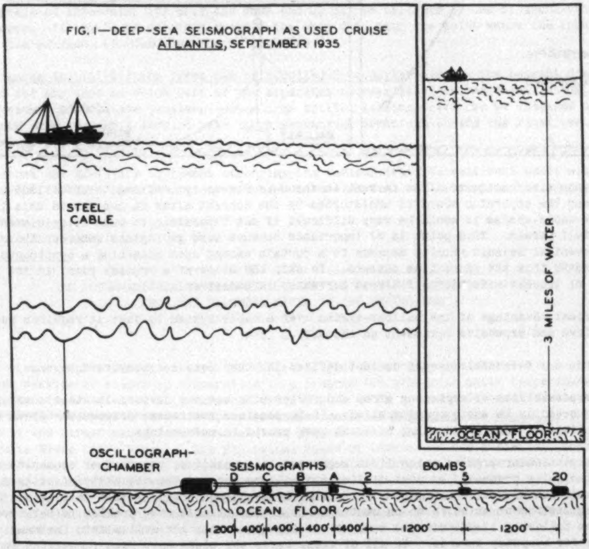
FIG. 1—DEEP-SEA SEISMOGRAPH AS USED CRUISE ATLANTIS, SEPTEMBER 1935

A large balloon is not required. The weight of the present set of seismic apparatus is excessive and could be reduced greatly should a second set be built. Even so, an oil-filled sphere six feet in diameter would supply ample buoyancy for the present set of equipment. It may be pointed out that the ballast is the only heavy piece of apparatus which rests actually on the bottom, and it is left behind when the apparatus ascends. The danger of the apparatus sticking in the mud is thereby minimized.

The system described above would eliminate the two principal difficulties encountered in the work of last summer. In addition it brings other advantages over operation with cable. If perfected for use in moderate depths, the balloon-system may be used without modification in the very deepest parts of the ocean. Another great advantage is apparent when measurements in the

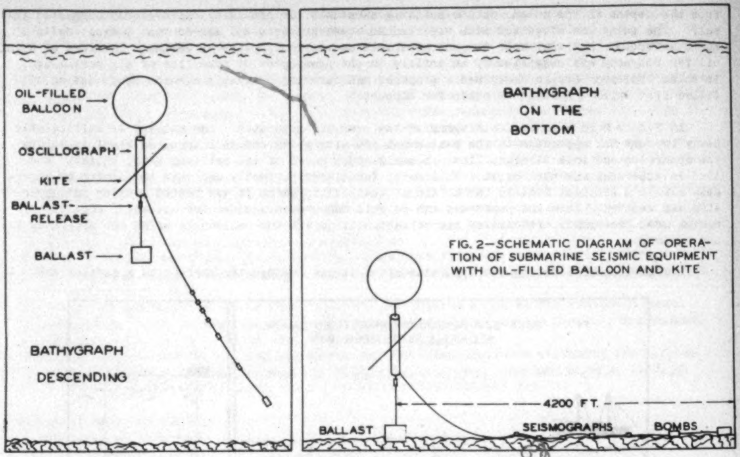

FIG. 2—SCHEMATIC DIAGRAM OF OPERATION OF SUBMARINE SEISMIC EQUIPMENT WITH OIL-FILLED BALLOON AND KITE

Gulf Stream are considered. The current in the Gulf Stream is confined to about 1000-meter depth, and the apparatus would be undisturbed by the current after it had passed this level. With the cable-system it would be very difficult if not impossible to make seismic measurements in the Gulf Stream. This point is of importance because some geologists consider that the value of the eventual seismic results depends to a certain extent upon obtaining a continuous line of measurements from the shore-line outward. To skip 150 miles at a crucial point in the profile, because of trouble with the Gulf Stream currents, is undesirable.

A final advantage of the balloon-system over a cable-system is that it requires much less specialized and expensive equipment on the ship.

Possible use of buoyant devices in other deep-sea measurements

The possibility of replacing wires and cables with buoyant devices in other oceanographic problems seems to be worth consideration. It is possible that these devices may prove as useful in this field of work as sounding balloons have proved in meteorology.

The particular problem which first suggested this method to us--that of submarine seismic measurements--is probably the most difficult of all because of the necessity of using a kite to provide horizontal motion for laying out a line of equipment nearly a mile in length. Where the measuring instrument which is to be sent down is a compact unit, the problem is relatively simple. The following items indicate some of the features which are well within the range of possibility for buoyancy devices. In all of these cases the plans have been formulated in sufficient detail to remove the proposals from the realm of speculation.

(1) Long, undisturbed placement of recording instruments on the bottom--Any recording instrument may be placed on the bottom, left undisturbed for several hours, days, or months, and then be brought to the surface by buoyancy. For example, we may place seismographs at points where earthquake-data are badly needed but islands are absent. Recording current-meters for investigating tidal or other phenomena could also be left undisturbed for a long period of time.

(2) Recall by time-clock or by signal from surface--If an instrument is to remain down for more than a day it would be possible to recall it by means of a signal--say sound-waves--from the surface rather than with a time-clock, in order to avoid danger of surfacing in stormy weather.

(3) Multiple operation--By using several instruments, dropped overboard in sequence, it is

possible to occupy several stations simultaneously. This would be an important advantage in many types of work.

(4) Stations at specified distance or at series of distances above the bottom--By using a reel of piano-wire as the connector between instrument and ballast, and providing the reel with a suitably controlled brake, one or more stops of desired duration may be made on the way to the surface. An instrument held at mid-depth by an anchored buoyant body would be less subject to disturbance than one suspended from a surface-buoy.

(5) Depth unlimited--This device will work in the greatest depths if it will work in moderate depths. It is quite superior to cable-devices in this respect.

(6) Ballast-release by contact with the bottom--For instruments which require no stops during the trip down and back, a ballast-release which functions upon contact with the bottom can be made. A velocity of six to ten knots during descent and ascent can be attained readily, so the round trip would not require an excessive length of time.

(7) Can follow progress down and up by sound-ranging--By causing the apparatus to emit sound-signals at intervals, its progress down and up may be followed by use of sound-ranging hydro-phones. It should be possible to have the ship very near the point where the apparatus comes to the surface, if sound-ranging be employed.

(8) Large initial lifting force can be supplied by a pulley-system--The buoyant scheme would be useful for any case in which part of the apparatus becomes stuck on the ocean-bottom, as, for example, occurs with bottom-samplers. The large initial lifting force can be obtained by having the balloon work through a machine with large mechanical advantage during the first part of its ascent.

Balloons and auxiliary equipment embodying the various features mentioned above will be tested at the earliest possible date, in conjunction with work on the deep-sea seismograph.

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AN INTERNATIONAL PROGRAM FOR COLLECTING SAMPLES OF SURFACE-WATER FROM THE COLUMBIA RIVER TO THE BERING SEA

Phil E. Church

The Oceanographic Laboratories of the University of Washington has enlisted the aid of the Lighthouse Service of Alaska in cooperating in a program for procuring daily temperatures and samples of surface sea-water at the five watched lights along the outside coast of Alaska. At present, surface temperatures and samples are taken daily at Swiftsure and Umatilla, the seaward entrance of the Strait of Juan de Fuca, and the program calls for similar data to be taken at the Columbia River Light Vessel. The Biological Board of Canada has had a similar program in operation for more than three years, daily temperatures and samples being secured at seven lighthouses along the outside of Vancouver Island and the Queen Charlotte Islands. Such close cooperation exists between the Pacific Biological Station and the Oceanographic Laboratories of the University of Washington that this amounts to a unified program for the study of inshore waters from the mouth of the Columbia to the Bering Sea, a distance of 2000 miles.

The procedure requires that the samples and temperature be taken each day at the latter half of the flood-tide at a depth of one meter. At that depth and stage of tide the water is quite representative of surface-conditions. The method is simple and straightforward and involves but little time from the keepers. A two-ounce sample together with its temperature is sufficient.

The instrument to be used by the keepers in Alaska and on the light vessels off the Strait and mouth of the Columbia River was designed at the Oceanographic Laboratories last year and tested at Kingman Reef. It has proved satisfactory in every respect. Essentially it consists of two heavy brass plates, four inches square, which form the top and bottom. Four spring brass rods, 1/4 inch in diameter and about 18 inches long, hold the two ends rigid and act as protectors for the thermometer. A heavy brass tube, 3/4 inch in diameter, which has been milled out over about 12 inches of its length, holds the thermometer. This tube forms the center of the instrument. The thermometer is enclosed within a glass tube which prevents the sea-water from coming in contact with and erasing the division-marks. The bulb is freely exposed to the water which enters the well through a number of holes. High-grade thermometers with a range from 0° to 22° C and scale divisions of 1/5° are used. The scale is so open that it is easy to read. The thermometer comes to equilibrium in about one minute.

A spring-clip, screwed and soldered to the brass tube, holds the neck of the bottle. A guide to hold the bottom of the bottle in place is soldered to the bottom brass plate. The top of the sample-bottle is on the same level with the thermometer-bulb. Just above the open mouth of this two-ounce prescription bottle is a brass sheltering lip which prevents rain from entering the sample when the instrument is hoisted out of the water. There are no moving parts to the instrument and all metal is brass.

The bottles have bakelite screw-tops with a cork insert which is coated with an enamel that resists both acids and alkalis. To further eliminate the possibility of evaporation and to insure that no salt will form around the screw-threads of the bottle, the neck of each bottle has been dipped in melted paraffin. Each bottle has been numbered with a diamond-point and a paper label, which does not wash off, has been glued on it. The date and temperature are entered on the label at the time the sample is collected. The bottle fills slowly when submerged.

When a sample is collected, the instrument is lowered by a rope to the proper depth, held there for more than one minute, and then brought up. The thermometer is then read, the bottle slipped out, the cover securely screwed on, the label filled out, and the bottle restored to its position in the box. Periodically, the boxes will be returned to the Laboratories where the samples will be analyzed.

Owing to the large volume of surface-drainage from the Columbia River to the Bering Sea and the heavy rain along the coast, the lighthouse keepers will also keep records of rainfall. Other meteorological data which they will observe will include air temperatures, estimated cloudiness, and direction and force of wind.

The program herein outlined will result in a more complete knowledge of (1) the surface-temperatures along the coast, (2) the salinity and chlorinity and the changes wrought by the rainfall and land-drainage, and (3) the density of the surface-water along this 2,000 miles of the Pacific Coast. In addition to the above, and to the meteorological data which is all too scarce along the outside passage, these lighthouse-stations will act as base or control-stations from which profiles may be run from time to time to enable the computation of the shore-currents of this coast.

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REPORT ON THE WORK OF THE INTERNATIONAL COMMISSION ON CONTINENTAL AND OCEANIC STRUCTURE

R. M. Field

In 1932 the Dutch Geodetic Commission tabled a resolution advocating the appointment of a Joint Committee for the Study of the Earth's Crust for discussion by the International Union of Geodesy and Geophysics at its Lisbon meeting. At the Lisbon meeting, in 1933, it was resolved that the Bureau of the Union should appoint such a committee for the Edinburgh meeting. Early in 1936, the American Geophysical Union brought forward a similar, but perhaps more specific, proposition in advocating a joint discussion on "The use and value of geophysical methods in the attack upon the structural problems of oceanic and continental areas." The value of the complete interrelation of geophysics and structural geology was especially emphasized in this proposal. During the last Pacific Science Congress, the same general question was also raised. At the Edinburgh meeting of the International Union of Geodesy and Geophysics on September 24, 1936, the General Assembly unanimously approved the formation of a Commission on Continental and Oceanic Structure, and elected the following members: G. Angenheister, R. M. Field (Chairman), J. A. Fleming (Secretary), B. Helland-Hansen, Harold Jeffreys, F. A. Vening Meinesz, and O. T. Jones. Steps have been taken to increase the membership so as to include representatives of all the leading maritime nations, especially France, Russia, Italy, and Japan.

The Edinburgh Assembly particularly instructed the Commission to: (1) Pay particular attention to the structural study of oceanic basins and their margins; (2) urge those nations which support the International Union to create similar national committees.

From the foregoing condensed history, it will be seen that the International Commission has developed naturally, and has been largely modeled on the actions, methods, and experiences of the Special Committees on the Geophysical and Geological Study of Continents and Oceanic Basins of the American Geophysical Union. Since this is so, it should also be noted that the American committees have studiously adhered to the exact definition of their titles, and have never considered or promoted any phase of general, or even structural, geology which did not require the application of geophysical methods and techniques. This also should be the policy of the International Commission on Continental and Oceanic Structure, although it is not so clearly defined in the title of the "Commission" as in the titles of the American committees.

In August 1937, the International Geological Congress held at Moscow created a Committee on Geophysics, with Professor P. M. Nikiforov as Chairman. The duties of this Committee are perhaps not so clearly defined as those of the International Commission, but it is interesting to note that the Chairman and several members of the International Commission were appointed as members of the Geophysical Committee of the International Geological Congress, and that the Commission on Continental and Oceanic Structure is doing its best to cooperate with the new Committee. Following a meeting of the Commission on Continental and Oceanic Structure, held in London last December, Brigadier General Winterbotham, General Secretary of the International Union of Geodesy and Geophysics, wrote to Professor Nikiforov as follows:

"The International Union is naturally as pleased as it is interested to hear of the appointment of your Committee. A point which has probably struck all of us is that a duplication of committees will not be good unless there is real and constant touch between them. That touch promises well from the very fact that both include the names of those prominent in this intermediate field. We shall make it closer, however, if we agree to invite to our respective congresses and assemblies all members of our respective committees. We shall be happy to follow this procedure if you are also willing to do so."

In connection with this statement, it should be noted that the next Assembly of the International Union of Geodesy and Geophysics will be held in Washington in September, 1939, and that the next International Geological Congress will be held in London during the summer of 1940. The Commission on Continental and Oceanic Structure is preparing a report to be completed, published, and thus ready for discussions at the Washington Assembly in 1939. This report will attempt to cover the application of geophysical methods to the study of submarine and suboceanic geology, and will not trespass upon the domain of oceanography, as generally recognized under the terms of marine biology and physical oceanography, except in so far as these important fields of research are directly related to the surveying and study of the topography, sedimentation, lithology, and structure of the suboceanic lithosphere.

While it is clearly recognized that marine biological and oceanographic laboratories have rendered, and it is hoped will still continue to render, fundamental services to the marine geophysicist and the marine geologist, it is also becoming equally evident that further progress in the application of existing geophysical machinery and methods, and their ultimate extensive use in widespread and continuous suboceanic surveys, will require vessels and personnel greatly exceeding present private or institutional resources; especially when we take into consideration that oceanographers and oceanographic institutions can not, and should not, allow the requests of the geologists to interfere with their important researches in marine biology and physical oceanography. On the other hand, from the point of view of both the International Union of Geodesy and Geophysics and its American branch, irrespective of the Section of Oceanography, there is no phase of the Earth sciences which affords such a new and promising field for the application of geodetic and geophysical methods as the surveying and mapping of the suboceanic lithosphere, especially when we realize that this can be best accomplished, and in most phases _only_ accomplished, by geophysical methods.

What the student of suboceanic geology desires first and foremost are relatively accurate topographic maps of the ocean-bottom, particularly of what he considers to be significant areas. These maps are also necessary to the oceanographer, although, for the present at least, the oceanographer and the geologist may not agree as to what constitute the most significant areas. It should also be noted that good topographic maps of the submarine lithosphere are not only exceedingly important in themselves, but are also fundamental to all types of suboceanic surveying by geophysical methods. Second, the geologist desires undisturbed core-samples of suboceanic sediments, particularly in relation to suboceanic topography. Third, and most important of all, the geologist wants to know the _structure_ of the suboceanic lithosphere, as determined by the gravimetric, seismic, and magnetic surveying-methods now being developed and perfected.

During the past year, it has become increasingly clear that there is one phase of oceanography which is of equal importance to both the marine biologist, physical oceanographer, and marine geophysicist, namely, the problem of control of all types of apparatuses at all depths. It is hoped that the marine geophysicist and engineer, in cooperation with one or more navies, will improve old and possibly invent new methods which will help to recompense the oceanographers and naval officers for the time and trouble which they take in educating the geophysicist in the ways of the sea. What are badly needed are new types of hoisting apparatus, new types of cables for deep-sea work (including one-way and two-way electrical cables), and other methods still more remote, such as might be accomplished by submarine balloons, with, or without, mechanical or radio control.

Since the Commission fully appreciated the fact that its duties included the organization, as well as the discussion, of productive research, and also since, with the exception of the determination of gravity at sea, all other marine geophysical methods have been developed in the United States, Sir Gerald Lenox-Conyngham, Director of the Department of Geodesy and Geophysics of the University of Cambridge, was invited to send a geophysicist to the United States to make a critical study of all the methods used in this country. The Director appointed Dr. E. C. Bullard, who arrived in New York during July 1937 and returned to England the following September. During his stay here, Dr. Bullard particularly investigated the new submarine seismic methods, as developed by Dr. Ewing and Captain Heck, and the improved method of timing the Meinesz pendulums, as developed by Dr. Marrison, Dr. Ewing, and Lieutenant Hoskinson. Dr. Bullard also observed geophysical operations on board the Atlantis of the Woods Hole Oceanographic Institution and the methods of the United States Coast and Geodetic Survey for submarine mapping during a cruise of the Oceanographer. It was also arranged that Dr. Bullard should consult opponents as well as exponents of existing American geophysical techniques, and particularly in relation to Dr. Ewing's experiments in submarine seismology. Following Dr. Bullard's return to England, very careful consideration was given to his criticism and suggestions. In December the Chairman and the British members of the Commission attended several conferences in London with representatives of the British Navy, the Geological Survey, the Geological Society, and the International Union of Geodesy and Geophysics. During these meetings, plans were discussed for an immediate marine gravimetric survey and a submarine seismic survey of the British and Irish coastal shelf, and the ultimate topographic survey of significant portions of the Middle Atlantic Ridge. Plans were also discussed with the Department of Mineralogy of the British Museum of Natural History for core-sampling in the North Atlantic.

On April 8, 1938, Sir Gerald Lenox-Conyngham notified the International Commission that the Royal Society had supplied the necessary funds and had appointed a British Executive Committee for marine geophysical exploration in the Atlantic, that the cooperation of the Admiralty had been secured, and that Rear Admiral Edgell had accepted chairmanship of the Executive Committee. As a most important contribution to the marine gravity-survey, the American Bell Telephone Laboratories are lending their crystal chronometer, which was first used for timing the pendulums on the recent United States Navy-American Geophysical Union Expedition.

Not the least interesting phase of the activities of the International Commission on Continental and Oceanic Structure has been the fact that it has had to carry on its work with naval authorities during a pronounced period of wars and rumors of wars. We have found, however, that this has been a stimulus rather than a detriment to international cooperation in fundamental research. It is not only hoped, but expected, that eventually similar marine geophysical-geological programs will be developed with other navies, and so coordinated as to form a truly international survey of the suboceanic lithosphere.

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OBSERVATIONS ON FJORD-WATERS

Thomas G. Thompson and Kenneth T. Barkey

In a previous paper one of the authors attempted an explanation of the zones of minimum dissolved oxygen observed in the North Pacific Ocean and Bering Sea [see 1 of "References" at end of paper]. In these waters it was found that the concentration of dissolved oxygen decreased with depth until minimum values were obtained between 800 and 1200 meters. Below these depths the dissolved oxygen increased with depth toward the bottom. It was postulated that the waters of this minimum oxygen-zone represented water that had been in contact with the ocean-bottom in shallower regions for indefinite periods of time and thus subjected to reactions of reduction of both a chemical and biochemical nature, the dissolved oxygen serving as the oxidant. High values for phosphates [2], borates [3], iron [4], etc., were generally found in these minimum oxygen-zones, thus indicating more or less previous leaching of material which had reposed on the ocean-floor. This water, depleted of much of its oxygen, very slowly flowed toward deeper water and became superimposed upon water of greater density. In order to substantiate this postulate, it was thought that observations made in long deep inlets and adjacent to shallower sounds or arms and with just a narrow and shallow access to the sea, might give some valuable information, as such inlets may be considered as miniature oceans.

This paper is a summary of studies made on the waters of a series of inlets that extend into the mainland of British Columbia on the northeastern shore of Queen Charlotte Strait. These inlets lie between 50° 57' and 51° 13' north latitude and 126° 40' and 127° 38' west longitude. Two passages lead to the entrance to the inlets, Slingsby Channel and Schooner Passage. The former is about seven miles long, has an average breadth of 600 meters, is of irregular depth with a mean of some 40 meters and with a tidal current that varies from five to ten knots. The latter, which is the better passage for entrance, is some two miles in length with a width of 200 meters which narrows to 80 meters at one point because of a large rock in the channel. In Schooner Passage, which has a rather regular depth of 17 meters, the tidal currents vary from two to five knots. Slingsby Channel and Schooner Passage unite about one mile from the entrance to the inlets. This entrance, known as Nakwakto Rapids, is about one-quarter of a mile long, 400 meters wide with a small island in mid-channel and with a depth from 11 to 21 meters. A maximum tidal velocity of between 20 and 25 knots, one of the greatest known, occurs in Nakwakto Rapids. There is approximately six minutes of slack water during which it is possible to enter the inlets. The range of the tide in the inlets is from three to four feet, while that on the seaward side of Nakwakto Rapids may be as great as 15 to 18 feet. The rapids are about 12 miles from the regular steamer-routes between Seattle and Alaska and in a region dotted with islets and infested with sunken rocks and reefs.

Description of the inlets--There are three main inlets, their relative positions to one another being shown on the chart of Figure 1. The total shore-line of this series of inland waters

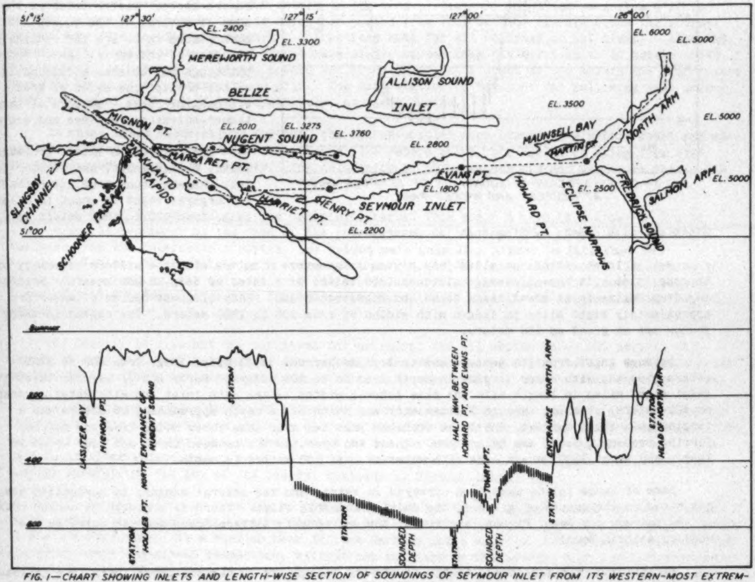
51°15'
127°30'
127°15'
127°00'
126°00'
EL. 2400
EL. 3300
MEREWORTH SOUND
ALLISON SOUND
BELIZE
INLET
EL. 6000
EL. 5000
EL. 3500
NORTH ARM
EL. 5000
MAUNSELL BAY
MARTIN IS.
EL. 2010
EL. 3275
EL. 3760
NUGENT SOUND
EL. 2800
MIGNON PT.
MARGARET PT.
NAKWAKTO RAPIDS
SLINGSBY CHANNEL
SCHOONER PASSAGE
HARRIET PT.
HENRY PT.
SEYMOUR INLET
EVANS PT.
EL. 1800
HOWARD PT.
ECLIPSE NARROWS
EL. 2500
FREDRICK SOUND
SALMON ARM
EL. 5000
51°00'
EL. 2200
200
400
600
LASSITER BAY
MIGNON PT.
STATION
HOLMES PT.
NORTH ENTRANCE OF NUGENT SOUND
STATION
STATION
SOUNDED DEPTH
STATION
HALF WAY BETWEEN HOWARD PT. AND EVANS PT.
TOWRY PT.
SOUNDED DEPTH
STATION
ENTRANCE NORTH ARM
STATION HEAD OF NORTH ARM

FIG. 1—CHART SHOWING INLETS AND LENGTH-WISE SECTION OF SOUNDINGS OF SEYMOUR INLET FROM ITS WESTERN-MOST EXTREMITY TO HEAD OF NORTH ARM IN EAST

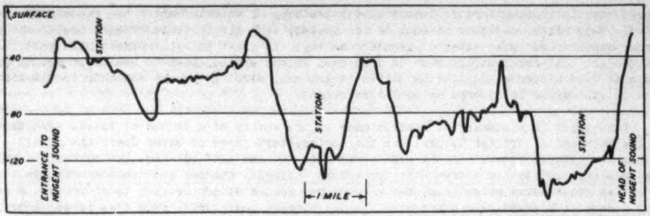

FIG. 2—LENGTH-WISE SECTION OF SOUNDINGS THROUGH DEEPER PORTIONS OF NUGENT SOUND

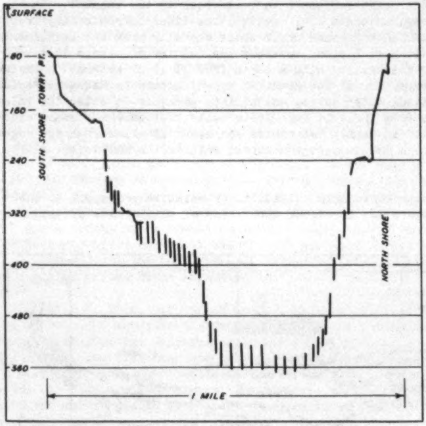

FIG. 3—CROSS-SECTION OF SOUNDINGS OF SEYMOUR INLET AT DEEPEST AND WIDEST PART

is over 600 miles. The entrance to each of the inlets and the sounds leading from them are sometimes characterized by rather shoal water or thresholds. The shore-lines are exceedingly steep and rocky with deep water extending directly to the shore. The hills are densely wooded with spruce, cedar, and hemlock with much exposed rock of fantastic shapes and rise to heights of from 3000 feet to 5000 feet. Countless water-courses abound. Ancnorages of less than 30 meters are few. However, it is possible to moor to trees on the shore. Rivers enter at the head of several of the sounds and in such places there are the remains of a number of Indian villages. Indian paintings may be seen occasionally on the precipitous sides of great overhanging rocks. Numbers of large timber-wolves may be seen and heard.

The three main inlets are Nugent Sound, Belize Inlet, and Seymour Inlet. Nugent Sound from its entrance at Margaret Point is about 12 miles long, from 400 to 1800 meters in width and with depths varying down 150 meters.

Belize Inlet extends 24 miles from Mignon Point where it unites with the western extremity of Seymour Inlet. It has an average width of 1800 meters with water as deep as 280 meters. Branching from Belize Inlet are Allison Sound and Mereworth Sound. Both of these bodies of water are approximately eight miles in length with widths of from 200 to 1800 meters. The depths in these sounds are as great as 185 meters.

Seymour Inlet from its western end to Eclipse Narrows is 33 miles long, from 400 to 2700 meters wide and with water varying in depth from 35 to 605 meters. North Arm of Seymour Inlet is about eight miles in length with the same general widths as the main inlet and with water as deep as 400 meters. Passing through Eclipse Narrows, which has a depth approaching 25 meters and a considerable tidal current, the inlet branches into two arms some three miles from the narrows forming Frederick Sound and Salmon Arm. These two branches are between three and four miles in length and about 1300 meters wide with water of over 200 meters in depth.

None of these inlets have been surveyed in detail and the several dangers to navigation are not noted on the charts or given in the British Columbia Pilot. There is a dangerous sunken rock in the fairway off Point Mignon, another at the entrance to Allison Sound and the third in the bend in Allison Sound.

Excepting on the thresholds to the sounds and inlets and in regions where there is a noticeable tidal current, the bottom is mostly ooze and soft mud rich in hydrogen sulfide. However, hydrosulfuric acid could not be detected in the waters taken within less than five meters of the bottom.

A line of soundings were run in mid-channel of Seymour Inlet and Nugent Sound with soundings every 82.5 meters using a fathometer, type-431 of the Submarine Signal Corporation. In the deeper parts of Seymour Inlet soundings greater than 480 meters could not be obtained with the fathometer because of the very soft nature of the bottom. A few wire-soundings gave depths varying from 601 to 607 meters in the localities where the fathometer failed to record. Figure 2 is a lengthwise section of Nugent Sound, while Figure 3 is a cross-section of Seymour Inlet at one of its widest and deepest parts.

Experimental--Two cruises were made to the inlets, one during the first week in September 1937 and another during the week of March 20, 1938, in which a total of 23 stations were occupied in all the sounds and inlets. The results of the cruise in September gave the general conditions of the waters after the biological activities of the summer and before the influx of the more concentrated sea-water of the fall. The cruise in March gave the conditions existing at the close of the winter, before the maximum spring runoff and at the beginning of the period of marked biological activity.

Seymour Inlet--(a) Temperature: Above 75 meters the temperature of the water showed a change from September to March of from 1° to 9° C, the change being the greatest near the surface. (Surface in September 14°.73 to 16°.04 and in March 6°.83 to 7°.19; at 75 meters 7°.87 to 8°.49 in September and 7°.06 to 7°.95 in March.)

The surface-waters were about 8° to 9° warmer in September than the surface-waters of Queen Charlotte Strait. In March the waters of Queen Charlotte Strait were 3° colder than the same waters of the previous September but just a fraction of a degree warmer than the waters of the inlets.

The waters between 250 and 600 meters in the deep portions of the inlet were from 0°.10 to to 0°.21 colder in September than in March and varied from 7°.01 to 7°.55 (in all cases the thermometers were read to 0°.01 and necessary corrections made for all readings at all times). Also the waters from a meter or two of the bottom were always warmer than the water 25 to 50 meters above. For example, in September, between Howard and Evans Point, at a depth of 550 meters the temperature was 7°.02 while at 600 meters it was 7°.15. The same station in March of the following year showed temperatures of 7°.19 and 7°.47, respectively, at these depths.

To check these observations further, the bottom-bottles were placed five meters apart and always the bottom-waters were warmer than those five meters above, the variation being from 0°.06 to 0°.50 depending upon the locality. The chlorinity of these waters did not vary more than 0.012 o/oo.

(b) Chlorinity and salinity: The chlorinity varied from about 3 o/oo (5.5 o/oo) to 7 o/oo (12.7 o/oo) in September at the head of the inlets to about 14 o/oo (25.3 o/oo) in March at the surface. For the intermediate depths, the values were generally higher in fall than in spring, while the deeper waters were very much the same, 16.02 o/oo to 16.06 o/oo (28.95 o/oo to 29.02 o/oo). However, the chlorinity of the bottom-waters for the fall was in general about 0.020 o/oo (0.03 o/oo) higher than for the spring, a value of about twice as great as the experimental error.

(c) Density in situ but not corrected for pressure: For all depths below 200 meters, the densities were practically identical for September and March, there seldom being a variation greater than 00.02. The slight increase in chlorinity was compensated by the increase in temperature.

(d) Dissolved oxygen: In the shallow portions of Seymour Inlet and the several sounds branching from it, the oxygen-values showed a decrease from the top to the bottom, the percent saturation varying from over 100 per cent at the surface to 3.9 per cent to 33 per cent at the bottom. The bottoms of these inlets varied from about 100 to 200 meters. The data of Table 1 show the percentage saturations for two of the deepest stations in Seymour Inlet.

It is at the depths of from 150 to 200 meters that the lowest oxygen-values are obtained and these depths correspond to the bottom-depths of the adjacent arms which gave very low oxygen-values in the bottom-waters. At a station near Eclipse Narrows where water of 400 meters was encountered and where there was direct connection, without any barriers on the bottom, with the deeper water further down the Sound, much lower values for dissolved oxygen were obtained. Here the data for temperature, chlorinity, and density were almost identical with that of the deeper stations. The

Table 1--Percentage saturations of two of the deepest stations in Seymour Inlet

Depth	Henry Point				Howard-Evans Point			
	September		March		September		March	
	Per cent saturation	σ_t	Per cent saturation	σ_t	Per cent saturation	σ_t	Per cent saturation	σ_t
S	90	11.86	108	20.19	93	11.10	129	19.65
5	63	16.66	98	20.31	69	17.94	105	20.44
10	71.5	20.19	101		63	20.69	93	21.00
20	57.9	21.70	84.4	21.63	60.0	21.76	76.6	21.82
30	55.8	21.85	84.4	21.91	59.5	21.93	69.7	21.95
50	55.1	22.08	85.7	22.09	59.2	22.04	80.9	22.13
75	56.9	22.35	85.0	22.18	59.1	22.28	83.2	22.24
100	45.8	22.61	80.1	22.30	36.5	22.51	73.3	22.39
150	33.8	22.64	28.7	22.66	35.8	22.65	33.0	22.65
200	43.1	22.68	35.4	22.68	33.0	22.65	32.0	22.65
250	....	22.69	41.0	22.70	....	.....	38.2	22.67
300	52.5	22.70	49.8	22.69	45.9	22.69	50.2	22.71
350	56.3	22.71	52.4	22.69	....	.....	52.0	22.72
400	56.8	22.71	52.6	22.70	51.5	22.73	52.9	22.71
450	58.5	22.65	....		....	22.70	51.0	22.72
489			53.3	22.70	....	.....	....	.....
500					62.6	22.70	52.4	22.69
550					....	22.71	52.0	22.66
600					46.0	22.69	52.7	22.67

zone of minimum oxygen occurred at 150 meters. The oxygen-values immediately above this zone to about 50 meters and all values below the zone were considerably lower than that at other stations. At 50 meters and at 350 meters in March, the values were 69 per cent and 44 per cent saturation respectively. This indicates that the waters of low oxygen-values from North Arm and Salmon and and Frederick Sound were carried into the main deep portions of Seymour Inlet and had a tendency to diffuse downward and upward.

Table 2--Concentration of silicates and phosphates, micromilligram-atoms, Seymour Inlet

Depth	Henry Point				Howard-Evans Point			
	September		March		September		March	
	Silicate	Phosphate	Silicate	Phosphate	Silicate	Phosphate	Silicate	Phosphate
S	37	0.36	22	1.00	34	0.15	22	0.80
5	36	1.40	23	1.00	42	1.40	26	1.00
10	42	1.70	26	1.55	50	1.60	29	1.42
20	48	1.94	34	1.60	52	1.84	33	1.80
30	53	2.18	34	1.86	51	1.90	43	2.20
50	54	2.16	34	2.28	50	1.90	40	1.95
75	55	2.14	34	1.95	48	1.92	34	1.95
100	63	2.50	43	2.08	61	2.20	35	2.16
150	67	2.68	67	2.80	70	2.35	56	2.76
200	68	2.52	63	2.64	78	2.54	56	3.10
250	72	2.56	67	2.60	..	....	58	2.90
300	66	2.28	62	2.45	75	2.50	58	2.70
350	64	2.28	62	2.42	..	....	56	2.60
400	67	2.23	63	....	72	2.14	52	2.55
450	68	2.18	..	....	..	....	56	2.60
489			66	2.35	..	....	..	....
500					69	2.16	54	2.52
550							52	2.80
600							60	....

The above data also show that the dissolved oxygen is greater in the fall than in the spring for the deeper waters, a reverse statment being true for the surface-waters. The zone of minimum oxygen shows lower values in the spring than in the fall.

(e) Nitrites: In the arms of Seymour Inlet, North Arm, Frederick Sound, and Salmon Arms, nitrites did not exist below ten meters. Between this depth and the surface the nitrites varied from 0.02 to 0.27 milligram-atoms N-NO_2. At the Howard-Evans Point Station, the nitrites were found to a depth of 30 meters and at the Henry Point Station to a depth of 100 meters.

(f) Silicates and phosphates: The concentration of these substances varied considerably as to season, depth, and location, as shown by Table 2 where these constituents are reported as micromilligram-atoms per liter of the element.

Discussion--The movement of the deeper water in the inlets may be considered as (a) influx of sea-water entering the inlets and (b) flow of water from the sides of the inlets and from the shallow portions of the arms and sounds to deeper water as the result of slight gradient-currents.

Table 3--Percentage saturation at various depths of different inlets

	Salmon Arm		Frederic Arm		Head of Mereworth Sound		Middle of Belize Inlet		Head of Belize Inlet		Middle of Allison Sound		Head of Allison Sound	
	mg-at O_2	o/o sat.	mg-at O_2	o/o sat.	mg-at O_2	o/o sat.	mg-at O_2	o/o sat.	mg-at O_2	o/o sat.	mg-at O_2	o/o sat.	mg-at O_2	o/o sat.
0	0.632	112	0.624	112	0.521	95	0.590	103	0.545	98	0.554	96	0.570	90
5	0.480	84	0.521	89	0.504	91	0.346	59	0.431	75	0.428	73	0.544	92
10	0.434	74	0.436	75	0.420	71	0.386	66	0.427	72	0.379	65	0.229	38
20	0.364	61	0.360	60	0.320	53	0.375	65	0.370	61	0.337	57	0.151	25
30	0.345	57	0.346	57	0.275	45	0.372	64	0.354	58	0.326	55	0.068	11
50	0.224	36	0.252	41	0.178	30	0.360	61	0.262	43	0.215	36	0.012	2
75	0.093	15	0.090	15	0.209	35	0.190	32	0.020	3.4	0.362	60	0.018	3
87	.....	..	.....	..	.....	..	.....	..	.....	..	.....	..	0.021[a]	3.5
00	0.118	19	0.120	20	0.216	36	0.193	33	0.084	14	0.177	29		
22	.....	..	.....	..	0.195	33	.....	..	.....	..	.....	..		
25	0.108	17	0.108	18	0.169[a]	28	.....	..	.....	..	0.098[a]	16		
40	.....	..	.....	..			.....	..	.....	..				
50	.....	..	0.097	16			0.378	63	0.366	60				
56	0.070	11.5	.....	..			.....	..	.....	..				
75			0.067	11			.....	..	.....	..				
82			.....	..			.....	..	.....	..				
00			.....	..			0.358	59	0.366	60				
07			0.024[a]	3.9			.....	..	.....	..				
50							0.370	61	0.360	59				
69							0.254[a]	42	0.357[a]	58				

[a] About two meters from bottom.

The entering of sea-water has been subjected to considerable turbulence as the result of tidal action and erose nature of the bottom. This water, relatively rich in dissolved oxygen and of slightly greater density will tend to seek the greater deeps as it flows into the inlets and thus lift some of the bottom-water or diffuse and mix with it, giving higher oxygen-content values at the bottom. This action assumes ready access of the inflowing sea-water to the deeper parts of the inlets. In localities where the access is markedly limited, very low oxygen-values are obtained in the bottom-water. The data demonstrate these assumptions.

In Seymour Inlet where far greater depths are obtained than in other portions of the series of inlets, the water might be said to become stagnant and thus practically no water-movement would occur in the deeper depths of the inlet. An examination of the density of the waters of these depths at the two different seasons would logically lead one to this conclusion if other factors were not taken into consideration. However, all of the constituents show seasonal changes in concentration and thus there must have been a change of water.

The bottom-muds of many of the inlets contained noticeable quantities of hydrogen sulfide which would tend to react with the dissolved oxygen in the bottom-water generally at depths approximating 150 to 200 meters. The reaction of dissolved oxygen with hydrogen sulfide is probably the predominating reaction tending for the removal of oxygen. This water would be slowly moved out into a locality where much greater depths were obtained and would be retained at approximately the same depth occupied in an arm of the Inlet. On the seaward side of Seymour Inlet, there is a gradual and continual supply of sea-water higher in oxygen and its greater density causes it to flow down to the greater depths.

If this statement is true for the deeper portions of the Inlet, then similar conditions should be noted in other inlets where the depths are not as great but where the deep water of the shallower inlets may have an intermediate zone influenced by the influx of bottom-water from still shallower portions of the inlets. That such zones of minimum oxygen actually exist is seen from Table 3 showing the percentage saturation at various depths of different inlets, the samples being taken in September, 1937.

The density of the waters of the zones of minimum oxygen varies from inlet to inlet. However, these zones in the deeper inlets will have the same density as the bottom water, low in oxygen, as the immediate adjacent sound or inlet.

Summary--Intermediate zones of minimum values for dissolved oxygen were found in a series of inlets in British Columbia. The existence of these zones is explained as being due to the movement of water that previously was subjected to the reducing actions on the bottom of shallower inlets.

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AMERICAN GEOPHYSICAL UNION

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SECTION OF VOLCANOLOGY

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IGNEOUS ACTIVITY IN THE COMSTOCK DISTRICT, NEVADA

F. C. Calkins

(Published with the approval of the Director, U. S. Geological Survey)

(Abstract)

The oldest igneous rocks in the Comstock District are amphibolites probably derived from basalts and of Triassic age. These are intruded by pre-Tertiary quartz monzonite and by granodiorite of Sierran facies, the latter not being exposed on the surface but found on mine-dumps.

Igneous activity recorded mainly in volcanic rocks was almost continuous throughout the Tertiary. Its products, in order of age, were as follows:

Eocene--Rhyolitic flows aggregating a few hundred feet in thickness: One intrusive plug of rhyolite occurs.

Eocene or Miocene--Hornblende-augite andesite intrusive into the rhyolite, type locality American Ravine.

Miocene--(1) Breccias and lavas, chiefly hornblende-pyroxene andesite; 500 feet or more in thickness. (2) Water-laid tuff, in part rhyolitic, in which Miocene leaves have been found at one locality; maximum thickness about 200 feet. (3) Pyroxene andesite, chiefly lava, hornblende-bearing in part; thickness 500 to 1,000 feet. (4) Hornblende-pyroxene andesite lavas; 500 feet or more.

Probably Miocene--(1) Dikes of hornblende-biotite andesite and pyroxene andesite. (2) Pyroxene-hornblende-biotite diorite of Mt. Davidson. This rock is clearly intrusive in the Miocene volcanic rocks, in which it has produced strong contact metamorphism near the contact. The "propylitization" of the andesites appears to be a low-grade metamorphic effect of the intrusion.

Pliocene--(1) Andesitic lavas of Kate Peak and Flowery Range, ranging from hornblende biotite andesite to hornblende-bearing pyroxene andesite with sporadic olivine. Early Pliocene leaves are found in diatomites interbedded with these lavas 6 miles northeast of Virginia City. Thickness about 2,000 feet. Plugs and probably a sill of hornblende-bearing andesite were intruded later than the eruption of some of the lavas. (2) A gray pyroxene andesite forming a plug and flows of small area in the southern part of the District, tentatively correlated with similar-appearing but olivine-bearing lava widely developed to the north and northeast. (3) Black pyroxene andesite often showing vestiges of resorbed olivine and rarely a little residual olivine.

Pleistocene--Olivine-rich basalt; remnants in American Ravine about 200 feet above the present stream.

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IGNEOUS SEQUENCE IN SOUTHEASTERN UTAH

F. G. Wells

No manuscript or abstract of this paper was submitted.

U. S. Geological Survey,
Washington, D. C.

LAVAS OF THE AFRICAN RIFTS, THEIR CHEMISTRY AND TECTONIC SETTING

N. L. Bowen

(Abstract)

The state of knowledge and opinion as to the African Rift valleys and their lavas was, a decade ago, such as to suggest that a definite correlation might be made between the tectonics and the chemistry of the lavas. The Western Rift Valley seemed to have been formed as a result

of compressive stresses in the crust and to have associated with it lavas of potash-rich character. The Eastern Rift Valley seemed to have been formed as a result of tensional stresses and to have associated with it lavas of soda-rich character. The picture now presented is less simple. As a result of further investigation there has arisen much diversity of opinion as to the tectonic forces controlling the formation of both Rift valleys and there seems therefore no adequate basis for an attempt to correlate lava-chemistry with contrasted tectonics. In addition, recent studies of the lavas have brought to light many exceptions to the general tendency towards preponderance of potash in the Western Rift and preponderance of soda in the Eastern Rift. Some new analyses are given which illustrate these exceptions and emphasize the fact that, as in the case of the tectonic relations, no simple picture of the chemistry of the lavas can be outlined at the present time. Indeed, when the question of the origin of the dominant lavas of the Western Rift is reviewed in the light of latest knowledge, their potash-rich character seems to be a relatively accidental circumstance. This question is discussed and the lack of any convincing relation between lava chemistry and tectony in the Rift valleys is emphasized.

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THE CLASTIC FLOW OF 1932 AT FUEGO IN GUATEMALA

E. G. Zies

(Abstract)

The 1932 eruption of Fuego was accompanied by a flow of hot clastic material which poured over the rim of the Crater. No fresh lava was ejected; the eruption apparently confined itself to clearing out the rock that had plugged up the Volcano; the rock had, however, been exposed to thermal activity.

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VOLCANIC ACTIVITY AT MAGNET COVE, ARKANSAS

C. S. Ross

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The igneous rocks and the minerals of Magnet Cove, Arkansas, have long interested geologists and mineralogists, but in much of the area rock-exposures are so sparse that many of the geologic relations have remained obscure. However, recent prospecting and the mining of titanium ores have uncovered rocks that throw new light on the geology of the region.

The regional rocks of the area are sandstones, slates, and novaculite of Paleozoic age. They were folded and underwent metamorphism in late Paleozoic time. In Cretaceous time they were intruded by a series of igneous rocks.

The intrusive igneous rocks that are of alkalic character form a nearly circular and almost continuous rim that rises 200 feet or so above a central depression, about two miles in diameter, that is known as the Cove. Within this area are highly metamorphosed slates, small intrusive masses, pegmatite-like rocks, and a calcite rock that contains abundant secondary minerals formed by hydrothermal processes. Among the minerals recognized within the Cove are calcite, monticellite, aegerite-augite, wollastonite, nepheline, microcline, eudialite, titanium garnets, magnetite, biotite, vesuvianite, perofskite, brookite, rutile, and the magnesium-lithium mica taeniolite.

Near the north side of the Cove, rutile is being mined by steam-shovel methods, and these operations have uncovered large areas of rock. The exposed rocks consist for the most part of clastic materials cut by dikes. The clastic aggregate contains blocks that range up to ten or more feet in diameter, but fragments from a few inches to a few feet in diameter predominate. A nearly white clay forms an interstitial material between the fragments. Most of the fragments are of volcanic-rock types, others are evidently of hypabyssal intrusives, and lesser numbers are evidently of local slate and novaculate. The white interstitial material, although now a clay, retains the structures that characterize a coarse-grained glassy volcanic ash. The material in this part of the Cove is, therefore, a volcanic agglomerate.

The materials of the volcanic agglomerate are all highly altered by hydrothermal solutions, and only the orthoclase and biotite of the original igneous rock have partially escaped alteration to clay minerals. Some of the dikes are altered in the same manner as the agglomerate, but others are fresh below the zone of weathering and are, therefore, of post-mineralization age. This extreme alteration was accompanied by the abundant introduction of rutile, pyrite, and leucoxene (a hydrous titanium oxide). These materials have been introduced into feldspar rocks, the glassy tuffs, the fragments of sedimentary rocks, and some of the intrusive dikes.

Prospect-pits have been sunk in an area about one-half mile northeast of the rim of intrusive rocks around the Cove. Here irregular masses of a quartzose rock and a few of igneous material litter the surface of the ground. The quartzose rock-masses have been formed by the recrystallization of novaculite and are coated with small brookite crystals. Pits sunk to a maximum depth of 50 feet have encountered other materials of the same character--rounded boulder-like masses of novaculite and abundant volcanic ash now altered to clay-material. The rounded masses have been formed by the partial replacement of novaculite by taeniolite. It seems evident that the rock-fragments have been mixed with ash during explosive volcanic eruptions.

The materials northeast of the rim rock have been altered in essentially the same manner as those within the Cove. However, brookite has formed rather than rutile, the other form of TiO_2. In both localities, leucoxene is very abundant.

Near Magnet Post Office, which lies east of this rim of intrusive rocks and about one mile south of the last locality, the novaculite has been recrystallized and brookite has formed in geode-like cavities.

Thus, two localities in the Magnet Cove area show evidence of explosive volcanic eruptions and the subsequent alteration and mineralization of the pyroclastic materials.

Magnet Cove thus represents one of the volcanic centers in the group that makes a great arc around the Gulf of Mexico. Such centers have been recognized at several points in Texas, at Monroe in Louisiana, near Nashville in Arkansas, and Jackson in Mississippi. It is the only one so far as known that has undergone subsequent mineralization.

U. S. Geological Survey,
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DIABASE DIKES OF THE FRANKLIN FURNACE, NEW JERSEY, QUADRANGLE

Charles Milton

(Published with the approval of the Director, U. S. Geological Survey)

(Abstract)

Two of the numerous small dikes mapped on the areal geology sheet of the Franklin Furnace Folio (U.S. Geological Survey 161) as "Mostly basic, including nepheline tinguaite, leucite tinguaite, and camptonite" of post-Ordovician age, have been found to be quite distinct from these alkalic rocks, and the two dikes in question are indistinguishable from the Triassic diabase found elsewhere in New Jersey and adjoining States.

The association of diabase with highly alkalic rocks has been observed and commented on by numerous writers. There is evidence pointing to the post-Triassic age of the Beemerville alkaline rocks, and a possible correlation with a volcanic ash of Cretaceous age found by Stephenson and Carter at Haddonfield, New Jersey, near Camden, in 1936.

U. S. Geological Survey,
Washington, D. C.

PRIMARY BANDING IN NORITE AND GABBRO

H. H. Hess

The remarkable regularity of banding in such complexes as the Stillwater, the Bushveld, and a number of other large mafic intrusions, has attracted attention of petrologists for the last two decades. Many hypotheses have been advanced to account for this banding; though no one hypothesis is completely in accord with all the facts concerning them, nearly every hypothesis has an element of truth in it. Coats [see 1 of "References" at end of paper] has ably reviewed the literature on the subject, has presented an hypothesis of his own to account for banding, and has backed that hypothesis with laboratory-experiments. This work seems to the writer to be the most satisfactory that has appeared on this subject. It may be the correct explanation for the banding of certain gaboros, but it is not entirely adequate to explain the banding which the writer has studied in the Stillwater Complex. The writer independently came to the conclusion that differential settling of the crystalline constituents of the bands is a major factor in the explanation of their origin. In this he completely agrees with Coats. He diverges from Coats' views, however, as to the cause of rhythmic repetition of the bands. Only a brief analysis of the problem will be presented at this time. The subject will be treated more comprehensively at a later date.

A number of different types of banding may be found in the Stillwater and Bushveld complexes. Only one type is being specifically dealt with in this paper, namely, a variety of banding on a small scale which is found typically in the Critical Zone of the Bushveld Complex, and a similar zone in the Stillwater Complex. It can also be found in the Duluth and in the Baltimore gabbros. It consists of bands or layers of crystals of calcic plagioclase and magnesian pyroxene (both orthorhombic and monoclinic). The banding is a result of variations in the proportions of these two constituents.

Summary of salient facts concerning bands

(1) Thickness--From a fraction of an inch, two or three crystals thick, to a number of feet.
(2) Length--Bands can rarely be traced along the strike for more than a couple of hundred times their thickness.
(3) Attitude--Approximately parallel to the floor of the intrusive.
(4) Contacts--May be sharp or gradational; crystals interlock across sharp contacts.
(5) Relations between bands--They never crosscut each other as dikes, though they commonly lens out along the strike or may divide to form two or more individual bands.
(6) Mineral variation across bands--The plagioclase and pyroxene maintain the same composition across a whole series, regardless of the proportions of two constituents present.
(7) Orientation of grains--The plagioclases are generally tabular and pyroxenes more or less elongated parallel to their c-axes. The grains are well oriented with their longer axes parallel to the plane of banding, giving the rock a well-developed gneissic structure in any plane perpendicular to the banding. The elongate grains (with rare exceptions) show no linear orientation in the plane of banding [see 3].
(8) Density-gradations--A very common type of band is one which has sharp boundaries and grades from pyroxene-rich at the base to anorthosite at the top. This feature, which may be compared to gradational bedding in sediments, makes it possible to tell top from bottom in many banded outcrops [see 2 and 3 for an application of this principle]. Bands which show little or no gradation are also common, and even a few with reversed specific-gravity gradation can be found.
(9) Regularity of bands--Most bands are remarkably straight and regular, but cases of disturbances of bands presumably during a crystal-mush state may occasionally be found. These irregularities, though uncommon, serve to throw some light on conditions during the formation of banding, and will be discussed below.

Zones of disturbed banding

Disturbances of banding in the Stillwater are, from the quantitative point of view, very rare, and may be considered as exceptions to the rule of great regularity of the banding. They may, however, be found at a number of horizons in the complex and can be followed along a given horizon intermittently for considerable distances. The zone of disturbed banding is commonly about ten feet thick. The floor below the disturbance is quite smooth and regular. Upwards the disturbance dies out rather gradually, and is overlain by an undisturbed series of bands forming the roof. The ten-foot disturbed zone represents the layer of crystal-mush present on the floor of the complex during crystal-settling, the undisturbed floor below it, the first layer of solidified rock at the instant of disturbance, and the layer above it as the next shower of settled crystals succeeding it. In a number of cases it may be noted that the lowermost disturbed bands must have been approaching the consolidation-stage, for they show some tendencies to break into blocks and thrust upwards as slices into the mushier material above. That they are not entirely solid is indicated by the manner in which they bend and in part deform plastically. Above these semi-solid bands, the disturbed zone has a completely plastic type of deformation.

In all of the disturbed zones noted in the Stillwater Valley area, the overturning of folds and the direction of movement of small thrusts was northeasterly. It seems likely that the disturbances are to be correlated with a regional sagging of the floor of the lopolith resulting in a northeastward dip. The disturbances are in the nature of landslides of unconsolidated crystal-mush. In one case a disturbance was found which mushroomed upwards in the manner of a salt-dome type of structure.

This evidence indicates fairly clearly the amount of crystal-mush present at any one time during crystallization--about ten feet--and nature of that mush, specifically, loose crystals, closely packed with relatively little liquid present. Furthermore, it may be concluded that they were deposited on a sloping surface which was near their angle of repose, and were set in motion either by a slight increase in that slope or perhaps some force such as might result from an earthquake.

Distribution of banding

Not all of the Banded Zone of the Stillwater (= Critical Zone of the Bushveld) is banded on a small scale. The following measured section indicates the proportion of non-banded norite present:

20± feet, banded (top)
135 feet, non-banded norite
1 foot, banded
11 feet, non-banded norite
2 feet, banded
26 feet, non-banded norite
5 feet, banded
30 feet, non-banded norite
10 feet, banded
50 feet, non-banded norite (bottom)
Total: 252 feet non-banded norite and 38 feet banded

Of course, the zones of non-banded norite may themselves be considered bands, but from the point of view of origin this distinction is significant, as will be seen below.

Theory of origin of banding

Banding by heterogeneous intrusion [4,5] obviously does not fit the field-evidence relating to this type of banding. Theories which depend on rhythmic changes of temperature, pressure, or composition [6,7] are inconsistent with the lack of variation in the composition of the crystalline constituents across a series of bands. Banding by fluxion [7] is out of accord with the observed lack of linear orientation of elongated minerals in the plane of banding. Finally, that portion of Coats' hypothesis which deals with settling of the heavy crystals and rise of the interstitial liquid from a rather dispersed crystal-mush and by this means floating up of the lighter crystals, does not seem to agree with (1) the indicated close packing of crystals in the mush, (2) the great regularity of the bands, and (3) the lack of roughly equal proportions of pyroxene and plagioclase for each particular band. (Such a relationship is necessary since Coats would have the settling of pyroxene through the crystal-liquid mixture cause an upward current of liquid which would float the plagioclase. But well-developed banding is present in anorthositic rocks with only 5 to 15 per cent pyroxene. In this case the upward current produced by so small an amount of pyroxene would be quite inadequate to float the plagioclase.)

In the writer's opinion two factors are primarily responsible for banding of the type under consideration: (1) The relative density-differences between each of the two main constituents settling, plagioclase and pyroxene, compared to the density of the magma; (2) disturbance of the normal state of quiescence in the magma by short epochs of mild, but irregular, turbulence.

Suppose plagioclase and orthopyroxene were settling from the magma. Disregarding size and shapes of the grains, the pyroxene, being denser, should settle faster. A slight stirring of the magma will be sufficient to slow or even stop the settling of the lighter plagioclase crystals while the heavier pyroxenes continue to settle at but a slightly slower rate. A strong disturbance might conceivably stop both minerals from settling; but when quiescence is restored the pyroxenes, sinking faster than the plagioclases, will reach the floor first and the result will be the same as in case 1 above--a band having a more pyroxene-rich portion followed by a more plagioclase-rich portion. Dependent then on the motions in the magma and relative densities of magma and crystalline phases, it would be possible to produce almost any type of band or sequence of bands and all of the observed phenomena described above. A very common type of band in the norites, however, should be (and is) one which grades from more pyroxene-rich at the base to more feldspar-rich at the top, such as would be produced if the normal quiet condition of the magma were disturbed by slight movement followed by the return to quiescence. A more complex series of motions in the magma would produce the other observed types of bands and relations between bands.

In the table given above, it is interesting to note that non-banded norite greatly predominates over banded combinations of norites and anorthosites indicating, according to the writer's hypothesis, that quiet conditions in the magma prevail most of the time but were spasmodically interrupted by epochs of disturbance of the liquid. These slight disturbances may occur a considerable distance, even thousands of feet, above the floor on which the crystals are accumulating.

The units of settling are not individual crystals but rather clusters of from three to a dozen crystals. For some reason plagioclases seem to adhere to plagioclases and pyroxenes to pyroxenes in these clusters. The pyroxene spots in anorthosite which appear to be single crystals in hand-specimen examination may be seen to be clusters in thin-section examination. The same may be noted for plagioclase spots in mafic specimens.

Note on some other types of banding

In a broad way the great zones of the Bushveld and Stillwater may themselves be considered bands. Thus the ultramafic zone near the floor of these complexes, which consists largely of pyroxene and olivine crystals, would represent one great band, and the overlying zone of norite and anorthosite, consisting of plagioclase and pyroxene crystals, another great band. Such bands are clearly the result of the course of crystallization of the magma and the settling of crystals formed. Pyroxene and olivine crystallize first and thus form the first band, and later plagioclase starts to crystallize and olivine ceases to, thus forming the next band. Similarly the occurrence of a chromite band or bands in the ultra-mafic zone may be attributed to the crystallization and settling of chromite at a particular instance or instances in the course of crystallization of the mafic magma. The same may be said of the ilmenite-magnetite horizons higher in the stratigraphic sequence of the Bushveld Complex. Such bands as the last two are of great lateral extent as compared to their thicknesses and do not hold to the rule that the length of a band is only a couple of hundred times its thickness, as formulated for the banded norites in a preceding paragraph.

The Merensky Reef of the Bushveld and similar disseminated sulphide-horizons of the Stillwater represent still another variety of band. The Merensky Reef is essentially a pyroxenite, commonly only a few feet thick, with which is associated a small amount of chromite and platinum-bearing sulphides. Compared to its thickness, it has very great lateral extent, hundreds of miles. Unlike other bands mentioned previously, with it is related a rather sharp change in composition of crystal-constituents. The orthopyroxene becomes abruptly more magnesium-rich than that of the rocks immediately below it, and the plagioclase more calcium-rich. The change in plagioclase composition, however, seems to occur at some distance above the base of the reef (±200 feet). [These data are based on a suite of samples across the Reef obtained by E. Sampson in 1929.] The writer would suggest that the Merensky Reef is not the product of a normal event in the course of crystallization of the magma, but represents an addition of undifferentiated magma to partly differentiated magma of the lopolith at that time. On mixing of the magmas, the chromite- and sulphide-content of the undifferentiated addition was immediately precipitated; and a pyroxene-rich band formed as a result of the disturbance produced by the intrusion of more magma which would cause the plagioclase to be relatively retarded in settling. The increase in magnesium-content of the pyroxene and calcium-content of the plagioclase is due to the slight increase in the temperature as a result of the addition of the hotter undifferentiated magma. The fact that the composition-changes are small (a few per cent increase in En and An) and the amount of chromite and sulphide also small, indicates that the volume of new magma was small compared to the bulk of magma present. With regard to this hypothesis of origin of the Merensky Reef, it is interesting to note that the Reef lies at the base of the section in the Potgietersrust area where a transgression of the magma into country rocks occurs.

A number of other types of banding may be noted in the Stillwater and Bushveld which cannot be dealt with at this time.

Acknowledgments

The writer wishes to express his indebtedness particularly to J. W. Peoples, on whose foundation of Stillwater geology he has leaned heavily in his own investigations in that area. With Peoples, the writer went over most of the Stillwater Complex in the field, and in the last seven years has spent much time with him discussing the problems involved in this and related investigations.

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STOPING AND ASSIMILATION IN A GRANODIORITE STOCK AT JAMESTOWN, COLORADO

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Introduction--At Jamestown, Colorado, a granodiorite stock of late Cretaceous or early Eocene age has intruded into pre-Cambrian schists and granites. Though there is some evidence that the magma intruded partly by pushing aside the enclosing rocks, there is also considerable evidence that stoping and assimilation played an important part in the emplacement of the stock. Schist breccia at the border and several small xenoliths scattered in the stock seem to represent various stages in the process of assimilation.

The granodiorite stock is in the Jamestown Mining District, 33 miles northwest of Denver, and at the extreme northeast end of the Front Range mineral belt. It is one of the earliest of a series of porphyry intrusions that show a close genetic relationship to the ore deposits of the District.

Local geology--The Jamestown District is in the pre-Cambrian complex of the Colorado Front Range. Highly metamorphosed biotite schists of sedimentary origin, belonging to the Idaho Springs formation, have been invaded by the mid-pre-Cambrian Boulder Creek and Silver Plume granites (see Fig. 1). A strong northwest-trending fault of late Cretaceous age cuts diagonally across the District and the junction of this fault with the northeast-trending contact between granite and schist seems to have been the chief factor that controlled the localization of the granodiorite stock. The horizontal dimensions of the stock are about 1-1/2 by 2-1/2 miles. Platy and linear structures seem to indicate that the body pitches to the south at about 65°.

Composition of the stock--The stock is largely medium-grained, nonporphyritic, gray to greenish-gray hornblende granodiorite, the chief minerals of which are andesine, hornblende and orthoclase, with minor amounts of quartz. Though the stock is fairly homogeneous, it grades imperceptibly from fine-grained diorite at the borders to slightly porphyritic granodiorite in an eccentric core, in the northwestern part of the stock. The diorite zone is widest and most pronounced along the southern and eastern borders of the stock. It is a fine-grained dark-gray rock that contains variable amounts of biotite and small amounts of augite in addition to the minerals of the granodiorite.

Mode of emplacement--It seems probable that the granodiorite stock was emplaced partly by pushing aside the surrounding rocks, for in places on the southern and western borders the foliation of the schist swings away from its regional course. However, stoping and assimilation also seem to have played an important part in the emplacement of the granodiorite during the later stages of intrusion.

At several places along the south and southeast borders of the stock there is a zone five to ten feet wide of schist breccia cemented by quartz diorite. Abundant schist-fragments one-fourth inch to one foot in diameter are oriented in various directions. The borders are gradational and the fragments appear to be in various stages of digestion by the magma. Many of the fragments have largely recrystallized to a dark-gray, fine-grained material. Microscopic examination shows this material to be made up of lenticular areas of intergrown andesine, quartz, magnetite, and spinel, which are enclosed by irregular seams of fine-grained biotite and some sillimanite. These seams preserve the foliation of the original schist, but the blades and rods are oriented in various directions. The recrystallized material encloses residual grains of the original orthoclase, quartz, and biotite of the schist. Bordering this recrystallized material is a zone of quartz diorite containing abundant large flakes of biotite. Masses of coarse-grained biotite, locally segregated in the diorite, apparently represent the last remnants of schist-fragments. In many places thin dikes of diorite extend out along the foliation of unbrecciated schist and the contacts show the same gradation as that just described. It therefore appears that the schist was first altered to the fine-grained material by contact-metamorphism and then was gradually absorbed by the magma to form a biotite-rich quartz diorite. On the west border of the stock the granite-diorite contact appears sharp to the naked eye but in thin section it is transitional over an inch or two. Minute irregular dikelets and wedges of diorite extend into the granite and surround residual orthoclase grains. Biotite in this diorite bordering the granite is much less abundant than in the diorite bordering the schist, and the zone of biotite diorite is much narrower along the granite contact. It therefore seems probable that the magma assimilated the schist more readily than the granite and that the biotite in the border facies of the stock was derived chiefly from assimilated schist.

Blocks of wall-rock (xenoliths) occur at several places within the stock (see Fig. 1). The largest is on the northeast slope of Walker Mountain, about 150 feet from the top and 700 feet from the west border of the stock. This block is about 200 feet long and 100 feet wide and is composed of schist and pre-Cambrian granite. Apparently this block has been partly assimilated, for it is deeply indented by granodiorite along the schist-granite contact. The granite is not much altered, but under the microscope its orthoclase grains are found to contain abundant interlacing veinlets and irregular borders of albite. Adjacent to the biotite-areas are irregular

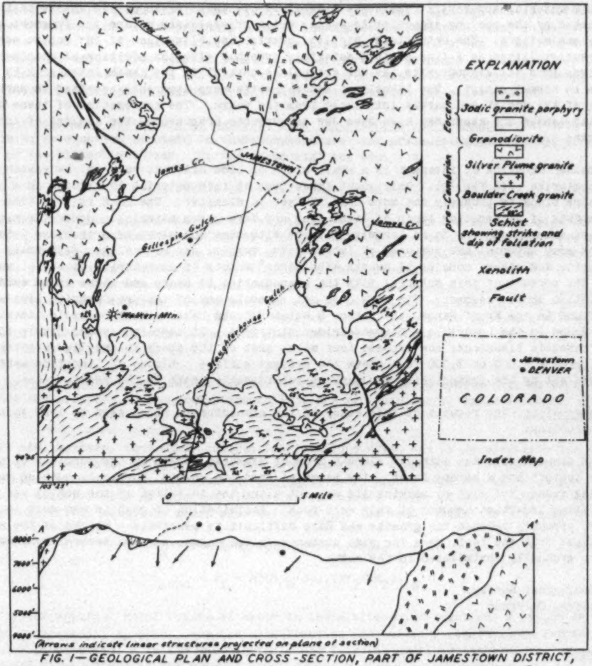
EXPLANATION
Eocene
Sodic granite porphyry
Granodiorite
pre-Cambrian
Silver Plume granite
Boulder Creek granite
Schist showing strike and dip of foliation
Xenolith
Fault
Jamestown
DENVER
COLORADO
Index Map
Little James Cr.
James Cr.
JAMESTOWN
James Cr.
Gillespie Gulch
Slaughterhouse Gulch
Walker Mtn.
40°05'
105°23'
0
1 Mile
A
A'
8000'
7000'
6000'
5000'
4000'
(Arrows indicate linear structures projected on plane of section)

FIG. 1—GEOLOGICAL PLAN AND CROSS-SECTION, PART OF JAMESTOWN DISTRICT, SHOWING EXTENT OF GRANODIORITE-STOCK AND ROCKS SURROUNDING IT

areas of sericite flecked with magnetite, apparently formed at the expense of the biotite.

The schist is much more strongly altered than the granite. The foliation is largely obliterated and the rock has the appearance of a dark-gray fine-grained quartzite. Under the microscope the rock shows a slightly gneissic inequigranular texture. Irregular areas of medium-grained orthoclase, quartz, biotite, and oligoclase appear to be original, for the feldspars are much sericitized. Grains of fresh andesine, which appear to be later than the altered material, are scattered through it. Another generation of material is represented by large lenticular areas of fine-grained andesine containing abundant small grains of dark-green spinel with some brown spinel, magnetite, and ilmenite. These lenticular areas are more or less parallel to the gneissic structure and make up from 25 to 75 per cent of the rock.

Near the center of the stock, about 1,200 feet below the xenolith above described, there is an inclusion that appears to be a partly absorbed block of schist. It is exposed only in a prospect pit and its borders are concealed. The material is a gray-and-black mottled rock of very uneven texture. It is a fine-grained gray-mixture of feldspar, biotite, magnetite, and quartz containing irregular masses and stringers of medium-grained hornblende. In thin section, calcic oligoclase is found to be the only original mineral left in the rock. It forms only a small part of the specimen and occurs in relatively large irregular grains, which are strongly corroded and are largely altered to sericite and chlorite. These grains are surrounded by a fresh, fine-grained ground-mass of orthoclase, biotite, magnetite, some quartz, and small amounts of andesine. Subhedral grains of green hornblende form irregular groups and veinlets in the fine-grained material and in place poikilitically encloses grains of orthoclase. Irregular grains of magnetite are fairly abundant.

This material apparently represents a xenolith of schist or injection gneiss that was largely absorbed by the cooling granodiorite magma, which crystallized before the fragment was completely assimilated. The orthoclase, biotite, quartz, and oligoclase of the schist were apparently recrystallized as a fine-grained material, probably with the addition of considerable orthoclase from the granodiorite, as the orthoclase-content in the assimilated rock is much higher than in normal schist. The hornblende and magnetite were apparently derived in part from the biotite of the schist and partly introduced from the magma. The segregation of these grains into irregular lenses and seams may have been due to currents produced by the sinking of the block in the magma.

Another xenolith of interest is a small block of lime-silicate rock in the eastern tongue of granodiorite (see Fig. 1). Only about three feet of this material is exposed in a pit, and the entire block is probably not more than 15 feet in diameter. The rock is very fine-grained and consists of alternating bands of light-gray and dark-green material. Under the microscope the green material proves to be largely diopside with some andesine and orthoclase. Some bands of light-gray material are composed of labradorite, quartz, and orthoclase, with small amounts of diopside and others consist of quartz with minor amounts of labradorite, biotite, and diopside. The contact of this material with the granodiorite is sharp and there is no evidence of assimilation or replacement. The rock does not resemble any of the pre-Cambrian lime-silicate rocks found in the Front Range, but rather a metamorphosed Paleozoic or Mesozoic limestone, such as are found in the Leadville and Breckenridge districts. It therefore seems likely that some of the Mesozoic limestones now exposed four miles east of the stock at one time overlay the District, perhaps 2,000 or 3,000 feet above the present surface. Blocks of limestone were apparently stoped out by the granodiorite and sank to considerable depth in the magma.

Conclusions--The results of this study of the granodiorite stock seem to lead to the following conclusions:

The granodiorite was intruded along a zone of weakness marked by the junction of a schist-granite contact and a strong fault. The magma widened its channel partly by pushing aside the enclosing rocks, but also by working its way out along the foliation of the schist and gradually assimilating important amounts of this wall-rock. Assimilation of granite was much less pronounced, probably because the granite was more difficult to penetrate. Blocks of the wall-rock were stoped from the roof, sank for some distance in the magma, perhaps several hundreds of feet, and were gradually assimilated in the magma.

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HIGH TEMPERATURE AND PRESSURE PHASE-EQUILIBRIA IN THE ALBITE-WATER AND ORTHOCLASE-WATER SYSTEMS

Roy W. Goranson

A preliminary report [see 1 of "References" at end of paper] of the phase-equilibria relations for the systems, albite ($NaAlSi_3O_8$)-water and orthoclase ($KAlSi_3O_8$)-water in the temperature-range 800° to 1,200° C and pressures to 4,000 bars (15-km depth) is presented herein. This work represents the first laboratory-investigation on phase-equilibria of truly rock-forming silicates which include water as a component.

In these investigations the homogeneous parts or phases with which we are concerned consist of: (1) A vapor-phase which is essentially water; (2) a liquid phase which is a saturated solution of water in silicate-melt; and (3) solid phase or phases consisting of one or more silicates.

Now in order to evaluate certain other interesting properties of these systems and to apply the results obtained to the understanding of other silicate-water systems, it is necessary to know not only the heterogeneous equilibria-relations but also the thermodynamic properties of each of the homogeneous parts or phases. To do this it was necessary to obtain p-v-T data for water and the solubility-relations of the silicate solutions in this temperature- and pressure-range.

The high-pressure furnace or bomb in which the runs were made is a modification of the apparatus which has been described elsewhere. The charges, consisting of water and finely ground silicate powder, are sealed gas-tight in platinum capsules. The pressure-medium is nitrogen gas which acts only on the exterior of the platinum capsules. Temperature is raised by applying electric power to a platinum-resistance furnace inside the bomb. Although the silicates investigated herein crystallize very sluggishly when in the anhydrous condition, they do so very readily from hydrous solutions; in fact, relatively large crystals have been grown in half an hour. These charges are held at the desired constant temperature and pressure for about three hours and then quenched. The capsules are then slit open and the contents examined for changes in phase. If no crystallization has occurred the glass is analyzed for water content.

The thermodynamic properties of water at high temperatures and pressures--The data available for obtaining an equation of state in these temperature- and pressure-ranges are not very complete or of the highest accuracy. It was found that the equation

$$p = [(4.619\ T)/(v - 1.9)]\ \exp\ [-1.08 \times 10^5/vT^{3/2}] \qquad (1)$$

will express, to a good approximation, the existing p-v-T relations for water at high temperatures. Some of these data were obtained by a modification of the method just described except that the temperature was continually raised until the tubes punctured from excess internal pressure. Here p is expressed in bars, v in cc/g, and T in degrees Abs.

According to this equation the Boyle point should be at 1200° C, that is, the equation reduces to $pv = RT$ for pressures less than 100 bars at 1200°. This is in agreement with entropy and enthalpy extrapolations calculated from spectroscopic measurements and the data presented in the Steam Tables of Keenan and Keys.

The system, albite-water--The solubility relations of water in albite-melt as a function of temperature and pressure, discussed elsewhere [2], are very similar to those for Stone Mountain granite melt.

There are, however, some other interesting relations which may be deduced from the experimental data, one of these being the apparent specific volume occupied by the water in solution. If the solution were an ideal one then the condensed water-phase, that is, the analog of liquid water, would dissolve with no apparent diminution in volume. These relations are evaluated from the following derived equations which also define v_{cond}, the molal volume of the condensed phase,

$$v_2 = v - (\partial N_2/\partial p)_T\ [RT(\partial \gamma_2/\partial N_2)_{T,p} + 1] \qquad (2)$$

$$v_{cond} - v_2 = RT(\partial N_2/\partial p)_T(\partial \gamma_2/\partial N_2)_{T,p} \qquad (3)$$

where v_2 is the apparent molal volume of water in the albite-water solution, v is the molal volume of the water-vapor, N_2 and γ_2 are the mole-fraction and activity-coefficient, respectively, of the water in solution, R is the gas constant, and T is the absolute temperature. v_{cond} denotes the condensed molal volume of the water.

v is obtained from equation (1); $(\partial N_2/\partial p)_T$, the concentration-pressure slope of the solubility curve, from the solubility relations; and $(\partial \gamma_2/\partial N_2)_{T,p}$, the activity coefficient-concentration slope, is deduced from the three-phase freezing-point relations of albite.

The ratio v_2/v_{cond} is found to be approximately constant and equal to $0.77_3 \pm 0.03_3$, that is, the apparent volume occupied by a gram of water in solution is 77 per cent of the volume assumed as that of the condensed-water phase. On examining some water-bearing obsidians it was found that, for the rock-glasses containing up to one per cent water, the apparent specific volume of water in solution was 72 per cent of the specific volume of liquid water, which is conformable with the result obtained above.

It appears rather unlikely, from many considerations, that rock-glasses containing several per cent of water have all of such water in solution, and this result may indicate a very useful criterion for the determination of water of solution.

The experimental freezing-point curve of albite in this three-phase vapor + solution + crystals system extends into temperature-concentration-pressure three-dimensional space and passes through the points (t° C, p bars, weight per cent water): (1115, 1, 0); (1000, 350, 3); (950, 560, 4.5); (900, 940, 6.1); (850, 1700, 8); (818, 3000, 9.4).

The initial slope of the curve projected on the pressure-temperature coordinate plane dT/dp, is computed to be -40-1/4° per hundred bars, and on the temperature-concentration coordinate plane, dT/dx_2, is -41.4° per weight per cent of water.

The liquidus curve for the granite-water system will be a discontinuous one but have the same general trend as that of the albite freezing-point curve but displaced down the temperature-axis by about 200°, that is, the atmospheric pressure liquidus point is about 950° C.

The system, orthoclase-water--This system is more complicated than the albite-water system in that at atmospheric pressure orthoclase melts incongruently to leucite and liquid at about 1115° ± 15° C, the leucite in turn dissolving completely to form liquid of $KAlSi_3O_8$-composition at 1535°.

The solubility-relations of water in orthoclase melt roughly parallel the curves for albite but are displaced toward the pressure-axis by about 1.5 to 2 weight per cent water, that is, water is less soluble in orthoclase melt by this amount.

At 1000 bars external pressure orthoclase in contact with water melts incongruently to leucite and solution at 990°, the leucite in turn redissolving completely at 1130°. At 2000 bars these respective temperatures are 956° and 990°. The leucite field disappears at 2600 bars above which pressure orthoclase melts congruently; at 4000 bars the freezing-point of orthoclase is 930° C.

General considerations--There is no point of maximum pressure on any of these freezing-point curves but there may be a minimum temperature, that is, beyond a certain pressure the freezing-point may increase with pressure.

In order to illustrate the tremendous pressures that may be built up in such systems as these on cooling and crystallizing, let us assume that we have an albite-water solution at a pressure of 606 bars (corresponding to a depth of 2-1/4 km) and temperature of 1100° C; the water-content of this solution at saturation is 4.2 weight per cent. Now let it cool. When the temperature has fallen to 960° the pressure will have dropped to 500 bars. At this stage albite will begin to crystallize out, and if the chamber-walls of this "magma" are impervious and competent to retain the pressures developed the system will move along the freezing-point curve. Thus, when the temperature has fallen an additional 59°, that is, to 819° C, the pressure will have increased sixfold to a value of 3000 bars (corresponding to a depth of about 12 km). At this stage the system will consist of 56 per cent albite crystals and a melt containing 9.5 per cent water if the amount of water that must be evaporated to build up this pressure be neglected. This process may be continued still further and the system develop at least five or six thousand bars pressure. There is here, then, a mechanism for developing all the pressure a volcanologist may desire.

This picture differs from that suggested by Morey [3], who used as analogy the system, H_2O-K_2SiO_3-SiO_2, studied by Morey and Fenner. They found a maximum pressure of 340 bars on the freezing-point curve of the mixture $KHSi_2O_5$ and quartz. If such relations were applicable to rock-forming silicate systems then the maximum pressure that could be developed would be of the order of 340 bars instead of the thousands of bars pressure realized herein.

Similarly in the orthoclase-water system if we start with a solution at 1130° and 1000 bars and let the system cool in a competent chamber, leucite will crystallize out, the system moving along the liquidus curve and the solution increasing in water content and decreasing in silica content until it reaches a pressure of about 2600 bars and 955°. At this point leucite will redissolve and orthoclase will crystallize from the solution. On further cooling orthoclase will continue to crystallize and at 930° the pressure will have increased to 4000 bars (corresponding to a depth of about 15 km). If the chamber were not competent to withstand more than a thousand bars, leucite would continue to crystallize and water to evaporate off, and, if the cooling were too rapid to permit the leucite to react with the liquid at 990° and form orthoclase, the final product would be a glassy rock studded with leucite crystals.

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[1] A more detailed account will be published shortly in Amer. J. Sci.

[2] R. W. Goranson, Amer. J. Sci., v. 22, pp. 481-502, 1931; Trans. Amer. Geophys. Union, 17th Annual Meeting, pp. 257-259, 1936.

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GENERAL CONSIDERATIONS OF THE EFFECT OF PRESSURE ON SOLUBILITY

R. E. Gibson

(Abstract)

The general problem of how the temperature-composition equilibrium-diagrams constructed from observations at atmospheric pressure are to be modified to represent equilibrium-conditions at the high pressures prevailing below the surface of the Earth is a complex one and this paper, essentially a progress-report, deals with one aspect of the question, namely, a survey of the factors which determine whether rise of pressure will increase or decrease the solubility of a solid in a liquid.

From thermodynamics it is known that the pressure-coefficient of the solubility of a solid is positive or negative according to whether the volume-change when unit-amount of the solid solute dissolves in the saturated solution is negative or positive. Consideration of the effect of pressure on the melting points of pure substances and the ideas current in the theories of solutions leads one to expect that the solubility of a pure solid in a liquid will be diminished by rise of pressure. A number of cases are known, however, where the opposite effect is found, and it becomes of interest to correlate the properties of the pure components and solutions in chemical systems with the effect of pressure on the solubility of the solids in these systems to the end that one may at least tell from a knowledge of the components whether it is probable that pressure will increase or decrease the solubility of the solid.

The volume-change when unit-amount of a solid dissolves in a liquid may be regarded as the sum of two effects Δv_m, the volume-change when unit-amount of the solid melts, and Δv_x, the volume-change when the liquefied solute mixes with the saturated solution. The quantity Δv_m, except in very rare cases, is positive and continues to be so even at very high pressures. Thus rise of pressure decreases the solubility of solids in liquids except in cases where Δv_x is large and negative. Empirical and theoretical arguments lead to the conclusion that the following factors lead to large negative values of Δv_x.

(a) Increasing difference between the compressibilities of the solvent and the solute. For a given solute the largest values of $-\Delta v_x$ are found for the solutions in the most compressible solvents, and for a given solvent $-\Delta v_x$ rises as the compressibility of the solute falls.

(b) Decreasing external pressure. This follows from (a) because increase of pressure lowers the compressibility-differences between substances.

(c) Decreasing concentration of solute in the saturated solution.

(d) Increasing mutual potential energies of the solvent and solute molecules. Greatest values of $-\Delta v_x$ are found when the solute exists as ions and the solvent is polar, for example, aqueous or alcoholic solutions of electrolytes. In such cases $-\Delta v_x$ increases as the charges on the ions increase and as the ionic radius diminishes. A reservation must be made about this statement. Liquids, it is now recognized, possess a peculiar type of structure due primarily to the presence of directed cohesive forces between the molecules. A solute, particularly a highly polarizing one, may affect the structure of a liquid in such a way as to produce anomalous volume-changes.

A semi-empirical equation summarizing the effects of factors (a) to (d) on the volume-change on mixing was developed.

These considerations have led to two conclusions of direct geochemical interest. The solubilities of sparingly soluble substances such as the sulfates, sulfides, fluorides, carbonates, or hydroxides of the alkaline earths, the rare earths, or the heavy metals in water are undoubtedly increased by hydrostatic pressure and the increase is significant. For example, a rise of hydrostatic pressure from atmospheric to 1000 atmospheres increases the solubility of calcite in water by over 50 per cent of its value at one atmosphere and the same rise of pressure increases the solubility of calcium sulfate by roughly the same amount.

On the other hand, in systems containing solid silicate minerals and molten silicates the compressibilities of the components are close together and small. Unless the structural effects in the liquids play a predominant role we may conclude, therefore, that the solubilities of the minerals in the liquid silicates are probably lowered by increase of pressure. In other words, a silicate liquid in equilibrium with a solid phase will contain less and less of that solid as the hydrostatic pressure increases.

[This paper is to be published in an expanded form in the American Journal of Science.]

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TEMPERATURES IN A SINKING ZENOLITH

T. S. Lovering

(Abstract)

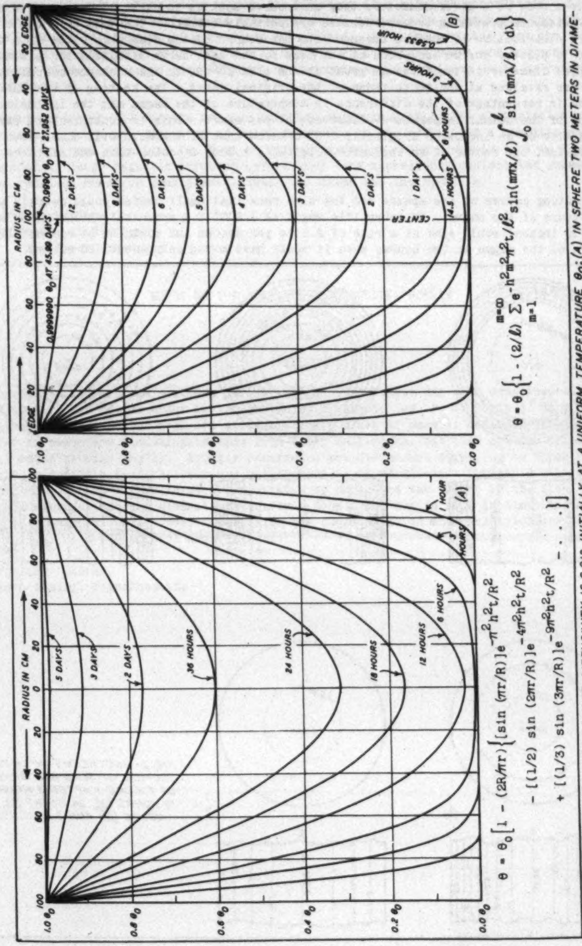
A zenolith may sink or float or remain stationary in a magma depending on the relative densities of the inclusion and the melt. If the density of the two differ perceptibly, the rate at which the zenolith moves through the magma will be a function of its diameter and its difference in density.

The experimental work of Rouse shows good agreement with the formula given by Rubey for the general case of the movement of spheres through liquids. Calculations were made with Rubey's formula for spheres of different diameters whose density was 0.23 greater than that of the liquid which was assumed to have a density of 2.29 and a viscosity ranging from 38,000 to 135 poises. The densities chosen seem ones that may be reasonably assumed for an igneous rock near its melting-point and the viscosities are those that have been experimentally determined for basalt in the temperature-range between 1,150° and 1,400° C. When the calculations have been graphed for representative viscosities it is found that, for the conditions assumed, an inclusion about 60 cm (2 feet) in diameter will sink about 1 km per day in a basaltic mass having a viscosity of 38,000 poises, equivalent to a temperature 1,150° C. In the same melt a zenolith 200 cm (6-1/2 feet) in diameter would sink about ten km a day neglecting the length of time necessary to acquire terminal velocity. An inclusion two cm in diameter would only sink one meter in a day in the same magma but a temperature rise of 50° C would change the viscosity to 3,200 poises and accelerate its fall 125 times.

The change of temperature in a sinking zenolith depends on many complex factors. If there is a chemical reaction between the inclusion and the magma, it may be perceptibly exothermic or endothermic. If heat is evolved, as it may well be with certain types of inclusions, the reaction will be self-propelling and complete solution would result whether the inclusion were large or small. If heat were absorbed during the reaction, a condition that seems unlikely, the temperature would rise more slowly in the mass than normal but complete solution could still take place. The chemical action would be conditioned by the temperature-distribution in the zenolith and either solution or melting would be closely related to the rate of temperature-increase

caused by conduction. If chemical reaction were slow, the assimilation of an inclusion would be due chiefly to a temperature-rise sufficient to cause fusion. In general, this temperature probably would be close to that of the magma. Although the special conditions which may be properly assumed for individual masses are innumerable, the general case of an unreactive zenolith initially at a lower temperature than the magma is instructive.

If a zenolith is falling through the magma at appreciable speed, the liquid moving past it brings fresh supplies of heat at approximately constant temperature to the surface of the body. If we may neglect the thermal gradient in the melt, it can be assumed that the surface of the



$$\theta = \theta_0\left[1 - (2R/\pi r)\left\{[\sin(\pi r/R)]e^{-\pi^2h^2t/R^2} - [(1/2)\sin(2\pi r/R)]e^{-4\pi^2h^2t/R^2} + [(1/3)\sin(3\pi r/R)]e^{-9\pi^2h^2t/R^2} - \ldots\right\}\right]$$

$$\theta = \theta_0\left\{1 - (2/\ell)\sum_{m=1}^{m=\infty} e^{-h^2m^2\pi^2t/\ell^2}\sin(m\pi x/\ell)\int_0^{\ell}\sin(m\pi\lambda/\ell)\,d\lambda\right\}$$

FIG. 1—TEMPERATURE IN ROCK WHOSE DIFFUSIVITY IS 0.012 INITIALLY AT A UNIFORM TEMPERATURE θ_0; (A) IN SPHERE TWO METERS IN DIAMETER, AFTER SURFACE HAS BEEN MAINTAINED AT A CONSTANT TEMPERATURE θ FOR GIVEN LENGTHS OF TIME (CALCULATED BY WILLIAM HUMENZCHEK) AND (B) IN SLAB TWO METERS IN DIAMETER AFTER BOTH FACES HAVE BEEN MAINTAINED AT A CONSTANT TEMPERATURE θ FOR GIVEN LENGTHS OF TIME (CALCULATED BY H.L. SPICER)

zenolith is maintained at constant temperature unless the movement is so slow that the chilling effect of the inclusion is perceptible in the approaching magma. At the end of one second the chill due to a large zenolith 1,000° C cooler than the surrounding magma would cause a fall in temperature of only 0°6 C at a distance of 0.5 cm and would be entirely negligible at a distance of one cm. Thus it would seem that a body falling at the rate of one cm per second would have its surface maintained at the temperature of the melt. Even with the simplifying assumption that the surface is maintained at a constant temperature, it is impossible to calculate the temperature-distribution for bodies of all shapes. Calculations were made for the sphere and the slab and most all other shapes would lie between these two extremes. If the temperature-distribution is graphed for a body of a given diameter, this same curve applies to other similar bodies of larger or smaller diameter providing the coordinates are suitably changed. The time at which a given temperature-distribution is reached changes with the square of the diameter. Thus if a temperature-curve is plotted for an inclusion at the time t, the same curve will hold for a similar inclusion whose diameter is ten times as great at the time 10^2 t, if the distance-coordinates of the curve are revalued at one-tenth those of the original curve. The heating of the inclusion is measured in percentage of the difference in temperature of the magma and the inclusion--it is independent of the actual temperature-difference at the start. This is equivalent to saying that the center of an inclusion originally 100° C below that of the melt will have risen 50° C at the time that the center of an inclusion originally 1,000° C cooler than the melt has risen 500° C.

The heating curves of the sphere and the slab show that small bodies would quickly achieve the temperature of the magma. In a basaltic magma at 1,200° C a spherical inclusion 25 cm in diameter (10 inches) would sink at a rate of 2.5 cm per second and would be at essentially the temperature of the magma in two hours, when it would have moved only about 180 meters.

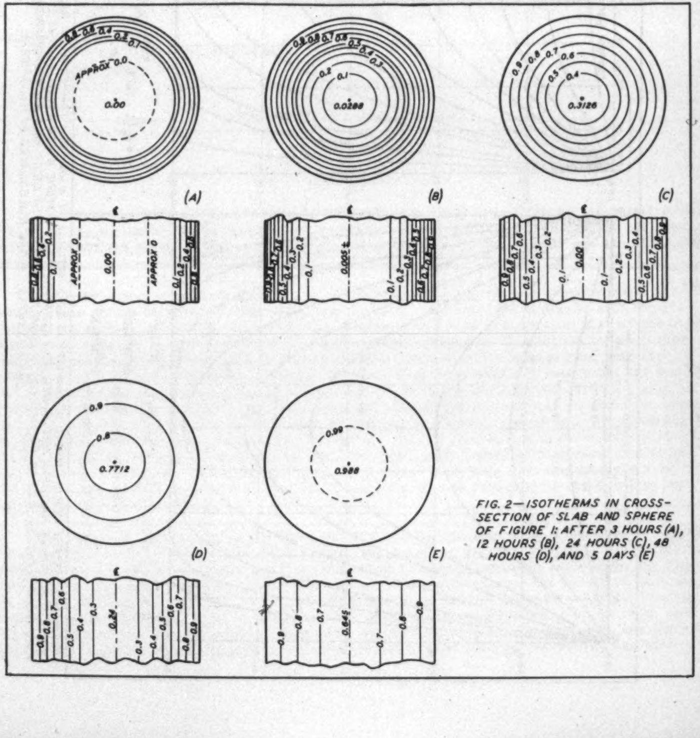


FIG. 2—ISOTHERMS IN CROSS-SECTION OF SLAB AND SPHERE OF FIGURE 1: AFTER 3 HOURS (A), 12 HOURS (B), 24 HOURS (C), 48 HOURS (D), AND 5 DAYS (E)

Even in a very fluid magma (basalt at 1,400° C) this inclusion would only sink 2,000 meters before it reached the temperature of the melt. A spherical inclusion two meters in diameter would sink at 13 cm per second in a magma of 38,000 poises viscosity--equivalent to basalt at 1,150° C. The temperature at the center of this inclusion would rise much more slowly than in the small one and it would sink four or five km before the temperature at the center began to rise perceptibly.

Certain modifying factors should be considered. In general, the temperature of the magma would not be uniform and would increase with depth. This thermal gradient would correspond with a decrease in viscosity which would hasten the movement of a zenolith accordingly. If volatiles were present, the chemical activity of the magma would probably be greater but the viscosity would be less and the descent of a zenolith more rapid. It seems probable that the rate of solution would be slow compared with the rate of descent in an active magma and conversely magmas sufficiently viscous to cause slow movement of moderate-sized zenoliths would be relatively inactive chemically.

Consideration of the factors briefly sketched suggests that active assimilation would take place chiefly through the digestion of small fragments in any portion of a magmatic chamber exposed to view. Most zenoliths two feet or more in diameter would probably sink to the bottom of even moderately large magmatic chambers before they were appreciably assimilated unless an exothermic chemical reaction took place between the magma and the zenolith.

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NOTES ON THE EGERSUND ANORTHOSITE, NORWAY

Robert Balk

(Abstract)

Located in the extreme southern part of Norway, the region has been described in part by F. K. Kolderup, and is now under investigation by Professor Tom. F. W. Barth of Oslo. Among the outstanding features of the rock are straight, sharp dikes of massive medium-grained anorthosite, the rocks of which are indistinguishable from their wall-rocks, but they cut the foliation of the wall-rocks at high angles. Equally remarkable are discordant dikes, up to three feet thick, of coarse, pegmatitic texture, composed megascopically of plagioclase (probably calcic andesine), pyroxene, and ilmenite. Preliminary examination by Barth and the writer in the field failed to reveal any hydroxyl-bearing minerals or quartz. Some dikes are so rich in ilmenite that they constitute commercial ore. These dikes raise the question as to what volatiles are capable of precipitating the above mineral associations.

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AMERICAN GEOPHYSICAL UNION

Nineteenth Annual Meeting

SECTION OF HYDROLOGY

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REPORT OF THE COMMITTEE ON SNOW, 1937-38

J. E. Church, Chairman

The snow-cover

The snow-cover during 1937-38 represents the continuing value of research for human adjustment to assets and liabilities of snow. In the far Eastern States the snow-cover has been scarcely sufficient to afford a surface for ski-sports. On the Western Coast the snow-cover is so heavy that in Lassen Volcanic National Park skiing will be possible into August.

But even in the West the snow-cover was oscillatory and attained unexpected depths. This in turn is traceable to barometric lows, unusual temperature, and mountain barriers. The snow-cover of December was melted by heavy rain and high temperature, resulting in disastrous floods. January, usually a month of high precipitation, was relatively dry, but February and March rebuilt the snow-cover to over-normal proportions along the Sierra-Cascade Barrier--so much so that the Owens Basin in the south now has a seasonal snow-cover varying from 154 to 234 per cent of normal. Lake Tahoe is again after a quarter-century rising to its high level. This event marks an important cycle in the development of snow-surveying, for it was the high water of 1906 to 1910 that caused the application of snow-surveying to forecasting runoff in order to quiet the fear of lake-dwellers regarding a return of danger. The present season a Lake Tahoe Conference was again called at Sacramento to determine the maximum height to which the Lake should be permitted to rise. An agreement was promptly reached on the basis of certainty with fear and suspicion completely in abeyance. Better still, the cycle proved that recovery after severe and long-continued water-depletion was possible.

However, in the Great Basin immediately eastward of the mountain barrier, the snow-cover, while better than last year, is still seriously below normal. Such disparity is naturally disheartening. The only consolation is that foreknowledge gives opportunity for readjustment.

Coordination

The policy of the present administration of the Section of Hydrology to seek coordination rather than segregation has been eagerly accepted by the Committee on Snow as strengthening its normal tendency toward breadth of interest. The interlocking and expanded personnel has brought mutual interchange of problems and new ideas from detached points of interest. The apex of this movement is the Planning and Project Committee of the Union under the chairmanship of Dr. William Bowie, that has invited proposals for projects that could be more readily advanced through the cooperation of committees or sections under the advice of specialists. The individual results of coordination will be seen in the proposals for research.

Ralph G. Stone, Editor, Bulletin of American Meteorological Society, has raised the question "whether the Committee on Snow should not be a joint one of the sections of Hydrology, Meteorology, and Seismology (Geology) of the American Geophysical Union and the Geological Society of America. The hydrologists are apt to push only phases of practical value to them, whereas the problems of snow are much larger. The International Commission of Snow deserves an American committee or group that balances the various phases of the subject pretty well, don't you think? This is not meant to imply any criticism of the Committee, but in the future we cannot be so sure that a hydrologists' committee on snow would keep up the broad basis we now have by virtue of the broad interest of the present personnel (possibly an accident of the era). This is just a quick thought on the subject; I may be concerned unnecessarily about the Committee set-up."

The present world tendency is toward the union of all forms of snow and ice in a single commission and also their humanization--an inevitable evolution irrespective of administrative relationship.

Expansion

The public need has brought natural expansion. Snow-surveys have been established in the Powell River Basin, British Columbia. The earlier survey in the Bow River Basin, Alberta, long abandoned, has been reestablished with lower and more stable courses. The Black Rock Forest on the Hudson is incorporating the conservation of snow with certain watershed-projects being laid out there.

Snow-surveying has been adopted by the Soil Conservation Service in connection with its runoff- and erosion-projects.

Broadcasts of snow-sports conditions are now being made wherever snow is available. Snow-fences are being erected to control drifting along highways. The value of weather in retarding snowslides in mountain passes is being studied.

The Susquehanna Collieries Company of Pennsylvania desires to forecast the amount of water that will enter their mines through numerous mine-caves during spring thaw.

Regional and project reports

Only the reports of activity will be given here. The suggestions of research-problems are so numerous that it seems best to present them in a unit for better comprehension.

Newfoundland (Eric Hinton)

Grand Lake was frozen over from the northern end of Glover Island to Howley. Channels of open water near the northern end of the Island prevented the party from reaching the Island itself; consequently it has been necessary to deduce a figure to represent the water-content of the snow in this sub-area. The logging operations in sub-area "a" have denuded the timber to the extent that no satisfactory sampling sites can be found. It is expected, however, that within the next three or four years the older "cut-overs" in this region will have reproduced sufficient young timber to obtain suitable sampling sites.

The water-content of the snow-blanket is considerably below the 8-year average at all points. The estimated flood-runoff is the minimum for the period 1926-38, being only 91 per cent of the previous actual flood-minimum of 1928.

An unusual feature of the snow-blanket this year is the complete absence of ice-crusts. This was due to the prevalence of abnormally low atmospheric temperature during January, February, and the first half of March and the consequent complete absence of the intermittent showers of rain commonly experienced.

An important uncertainty in preparing our annual spring flood-forecast is our lack of knowledge of the state of the ground-water. To overcome this we propose to install wells at various points within the sub-areas of the watersheds. These wells would consist of 4-inch pipes, 12 feet long, pointed and perforated at the lower end. The pipes would be driven into soft soil. The water-level in the pipes would be ascertained at the time of making the snow-survey and at any other times considered desirable. These observations would probably have to be continued for three or four years before attempting to determine their significance.

New England, particularly Maine (Paul L. Bean)

The water-year from October 1, 1937, to date has been a very wet one and the snow-cover very light. The usual snow-readings are being carried on in the area and at one station, located at elevation 1,600 feet, daily snow-readings are made. This last station has been in operation for about five years and is yielding much valuable information for this part of the country. The conditions which prevail this year on this area would probably be rated as unusual in that such a winter is not received here much oftener than once in eight years.

There is need for more stations at elevations 2,000 feet above sea-level and I would anticipate that it would take some time to get the trails cut so that work at these stations could be carried on in a fairly regular manner.

This year considerable trouble is being experienced from ice-jams in the river. These are receiving much attention and much of value is being compiled.

I have no recommendations for improvement in the Committee's work, except that it would be, I think, desirable to coordinate the gathering of all snow-data so as to make comparisons between different sections of the country and different elevations as uniform as possible.

The time is here, and interest is sufficiently great throughout this region, so that a uniform method of procedure in locating and operating snow-measuring stations should be made available to all parties interested in the subject. In brief, standardization of this important work is very desirable at this time.

New York (E. S. Cullings)

The Adirondack Snow-Survey has been extended to cover the Catskill Mountain Region under the chairmanship of Arthur W. Harrington, District Engineer, United States Geological Survey, Federal Building, Albany, New York.

The winter of 1937-38 was a season of comparatively light snowfall in the Adirondack Region.

A period of high temperature during the latter half of March resulted in the melting of most of the snow and moderately high flood-flows in the Adirondack streams.

Alberta, Canada (O. H. Hoover and W. T. McFarlane)

Various snow- and ice-fields are to be found on the upper reaches and tributaries of the Bow River and these to a large extent retard the flow of the stream well into summer. As well, normal and flood-flow conditions are to a degree dependent upon this deposit of snow, and as the waters of the Bow River from the point of view of power and irrigation have now become one of the Province's primary resources, it has been decided, for forecasting purposes, to collect some data each spring on the snow-deposit of the Basin.

Pursuant to instructions from J. T. Johnston, Director of the Dominion Water and Hydrometric Bureau, Ottawa, dated September 17, 1936, snow-courses were selected in September 1936 and the second annual survey over such courses was conducted on March 30 and 31, 1938.

Previous surveys of 1916 through 1921 had been abandoned because of decentralization and the wind-swept character of the snow-courses then selected. Reports are now being based upon 1937.

During the winter and prior to the end of March, 1938, very little abnormal flow due to Chinook winds or unusually mild winter-conditions have occurred in the Bow River Basin and on March 31 from all data available the snow-deposit is fully 100 per cent more than in 1937 and will probably represent about an average year. The whole valley and mountain range from Banff to Lake Louise and higher up contains a good snow-cover and the snow-storage without a doubt will contribute very materially to the early summer runoff in the Bow Basin. A forecast for the flow May to July is not as yet being attempted. It may be stated, however, that the flow for this period will probably be about normal.

Colorado River Watershed (John R. Riter)

This is the third year that the Bureau of Reclamation, in cooperation with other agencies, has been engaged in the operation of snow-survey courses and precipitation-stations in the Colorado River Watershed. At present, monthly readings are secured by the Bureau at 29 courses located in the Colorado and Gunnison River watersheds in Colorado, and precipitation-data are secured at 26 stations. The report on precipitation is made direct to this office. The snow-survey readings are taken through Ralph Parshall, Senior Engineer, United States Bureau of Agricultural Engineering, who also secures and makes available the results of measurements at courses on the White, Yampa, San Juan, and Dolores watersheds in Colorado.

These data are then correlated with snow-survey and precipitation-data secured by other agencies in the States of Utah and Wyoming, in making the forecast of Lake Mead inflow.

The period of concurrent observation has not extended over a long enough period of years to draw any conclusions regarding the relative merits of snow-survey courses and high-altitude precipitation-stations in making forecasts of runoff.

British Columbia, Canada (C. E. Webb)

The records of the Meteorological Service of Canada indicate precipitation during the past winter was about average. In the first part of the winter abnormal precipitation occurred, but in the latter part there has been a deficiency. This condition seems general throughout the Province. Temperatures during the same period were, almost without exception, above average, and there were no severe cold periods worthy of comment. The snowfall as reported by local observers has generally been about average, but at this date there is less than usual remaining on the ground except in Columbia Basin where observations indicate there is more than usual at the higher altitudes.

The runoff of the Fraser drainage above Hope has been 99.1 per cent of normal for the winter and of the Columbia at Trail 102.8 per cent.

There is always sufficient snow in storage to cause heavy runoff through the major stream-systems of the Province, and therefore the stages which these major streams will reach during the flood-season depends entirely on the climatic conditions of the next three months.

(a) Upper Columbia (R. C. Farrow)

The snowfall in general seems to have been heaviest in the upper reaches of the River. It is estimated that the runoff at Golden will be 138 per cent of normal or a little under 3,000,000 acre-feet, while at Trail it is estimated at 125 per cent of normal or a little over 45,000,000 acre-feet.

In 1936 the maximum error between the forecasted and actual runoff at eight points of measurement on the upper Columbia and Kootenay rivers and their tributaries was 11.5 per cent, and in 1937 was only 7.0 per cent.

(b) Powell River Basin (R. C. Farrow)

The Powell River Company, Limited, manufacturers of newsprint paper, has decided to lay out a limited snow-survey system.

The watershed of Powell River Basin is very rugged with elevations running up to 8000 feet and more, although within a few miles of tide-water. The area is densely timbered and the undergrowth very heavy.

The total watershed-area tributary to the outlet of Powell Lake is approximately 500 square miles. Of this, roughly half is tributary to the mouth of upper Powell River where it flows into the Lake. This half is undoubtedly the most productive in runoff, and probably originates about two-thirds of the total runoff and considerably more of that portion attributable to melting snow.

This upper watershed consists principally of the basins of (upper) Powell River and Daniels Creek. Both run far back into the high mountains of the Coast Range, in a region which includes its highest peaks and greatest ice-fields, and where its passes accumulate a depth of 25 and 30 feet of snow (on the level). Under these circumstances a rapid increase in depth of snow and decrease in winter temperatures can be expected with distance from the sea.

The remainder of the watershed consists of a comparatively narrow strip tributary to Powell Lake itself. It is in this area that the previous snow-surveys were conducted. They were a good pioneering effort, but, with the limited portion of the total watershed which they represented, the relatively light snowfall, and early melting, coupled with primitive equipment, there was little chance of success.

On the other hand, courses on the upper watershed can hardly be expected to represent this area, and as the runoff from it may be sufficient to upset forecasts to some extent based on the upper courses it may ultimately be necessary to have courses at one or two of the old locations.

Topographic maps and aerial photographs were used in determining the most feasible approaches to the selected snow-survey areas. The two courses selected have involved the building of some 25 miles of trail and two cabins. Both courses are at an elevation of about 3400 feet and in this area should be quite safe from premature melting. The runoff from Grouse Mountain area at 3800 feet does not begin before May. The Company is doing the sampling at the end of March. The data and snow-survey records have been standardized with those of the Comptroller of Water Rights of the Province.

The depth of the snow at the courses this season was 124 inches and 128 inches; water-content, 46.9 inches and 59.3 inches. On the basis of the 21-year precipitation-record at Powell River, the season is 97.3 per cent of normal. The normal runoff for 20 years is 879,990 acre-feet. The anticipated runoff this season has been set at 880,000 acre-feet.

The increase in precipitation eastward from the sea is enormous--in a 30-mile distance on Powell Lake rainfall the present season (November to March) increased from 19.75 inches to 47.08 inches.

The effect of precipitation during runoff is problematical. On the basis of 4:1 (winter: summer), at tide-level, the summer precipitation should remain a minor factor. However, the relative intensity of the summer precipitation may increase with altitude.

Oregon (J. H. Ryan)

Unusually heavy rainfall during November, December, and January established new precipitation-records at many stations of the Weather Bureau throughout the State of Oregon. Although the snow-cover in the higher mountain areas was very light, the soil was saturated and in excellent condition to receive the heavy snowfall which came in a series of storms during the period February 6 to 22. Timberline Lodge, a new winter-sports resort situated on the southwestern slope of Mount Hood at an elevation of 6000 feet, held its opening program on February 6. On that day 36 inches of snow fell in 14 hours and several hundred cars were marooned until snow-plows opened the roads.

The snow-survey network has not been greatly expanded during the past year, although several new stations have been established to complete the coverage as experience has indicated the need for more information. A goodly number of shelter-cabins have been established where they were most needed.

California (Harlowe M. Stafford)

The program of snow-surveys in California during the past year was essentially the same as in the previous year. It has recently been aided materially, however, by the active cooperation of the United States Forest Service whose field-forces took over several of the old established courses in their territory and also undertook to regularly survey in the future several new courses situated in territory where, up to now, the coverage by snow-surveys has been very meager and inadequate for forecasting purposes.

Forecasts for 1937 were essentially upheld. In the northern watersheds of the Western Sierra slope the runoff in general was slightly less than the forecasts while in the southern watersheds of Kaweah and Kern it was slightly in excess.

We are this season experiencing unusually heavy precipitation and snowfall and record runoffs and floods throughout the State. We should be able, therefore, to get some very good "high points" on our relation-curves.

Conservation (Joseph Kittredge)

Records for the fifth year are being collected from the ten different conditions of vegetative cover on the Stanislaus River drainage, between elevations of 5000 and 7000 feet, in the project of the California Agricultural Experiment Station. Compilation has not progressed to a point where much can be added to what has been said in previous years. It is becoming evident, however, that in the case of the evolution of the snow, as in other climatic phenomena, five years, each one different from every other, are far from sufficient for the establishment of averages or generalizations for longer periods of time.

A comparison of the water-equivalents of freshly fallen snow recorded in three different open areas where concurrent records from snow-boards and standard 8-inch rain-gages, without the funnels, were obtained may be of interest. The data apply to 16 storms in which snow but no rain fell. The totals ranged from about 10 to 13 inches water-equivalent. At the station most closely protected from the wind by a screen of coniferous forest the ratio of rain-gage to snow-board was 0.97. At the two stations most nearly centered in 40 to 80 acres open, the ratios were 0.92 and 0.90. The figure of 0.92 may be compared with corresponding ratio previously reported from the same area of 0.85 for a different year. The fact that the ratio is greatest in the most protected station suggests that the deficiency of catch in the rain-gage is caused chiefly by wind.

The Black Rock Forest at Cornwall-on-Hudson, New York, and the Susquehanna Collieries Company at Glen Lyon, Pennsylvania, are planning projects on the effect of forests upon conserving snow.

Snow-removal (J. D. MacVicar)

We are happy to state that in the State of Washington in the winter of 1937-38 there were not any unusual or even just ordinary problems encountered. The snowfall in our mountain passes was somewhat below normal, and the average temperatures were rather higher than usual. This latter condition resulted in some light rainfalls in the mountain areas during the winter that had the effect of compacting the snows and rendering them less susceptible to sliding.

In the plains sections east of the Cascade Range, and where the usual difficulties are due to drifting snows, we also experienced very little trouble. In these sections we use quite extensively picket-type of snow-fences and these quite effectively reduce the effort that is necessary in order to keep the highways open to traffic. In the fall of 1937 we added to the amount of picket-type snow-fence that we have used in seasons past by something in the neighborhood of 35 miles.

For uses in the mountain passes we added to our snow-fighting equipment four rotary snowplows. For general uses throughout the State we added to our blade and push-plow equipment some 250 pieces.

All of the above additions to our snow-fighting appliances and equipment were occasioned to some extent through additional roads added to the State system, these being former County roads now embraced in a Secondary State system of 2,237 miles.

Winter sports

Both the East and the West have become keenly interested in winter sports. W. W. McLaughlin estimates the number at 2,000,000. In the East the Ski-Bulletin has long carried weekly reports of winter-sports opportunities. In the West during the past year the Division of Irrigation of the United States Bureau of Agricultural Engineering has organized and conducted a weekly radio broadcast service of winter-sports throughout the Western mountain states. This includes the character of the snow-fields and frozen lakes, the condition of the highways leading to them, and the probable weather-condition. Broadcasts are issued from San Francisco, Salt Lake City, and Denver.

The United States National Park and Forest Services and other organizations have issued guides to winter-sport areas. The Secretary of the Interior has appointed special advisory committees on camping, hiking, and skiing. Robert G. Stone, Editor, Bulletin of American Meteorological Society, under authorization by the National Park Service has prepared eight snow-cover maps in the Eastern states.

The Winter-Sports Committee of the Sierra Club has extended its earlier tests of ski-craft by the addition of a final test known as "Ski mountaineers," for those who would seek the remoter portions of the High Sierra in winter for its beauty and its thrills. A Tahoe syndicate has been testing the winter-sports possibilities on the northern rim of Lake Tahoe, which seems destined to become a little Switzerland.

To aid in standardization so far as local conditions permit, a nomenclature of snow- and ice-forms is presented as an introduction to the Bibliography. The following regional reports also have been received:

(a) New England (H. I. Baldwin)--Reports of snow-conditions for winter sports are issued weekly by the Ski Bulletin, published every Friday in Boston during 15 weeks in the winter. The reports are received from a large number of volunteer observers at winter resorts in New England who telegraph in reports each Thursday morning. The Boston and Maine Railroad also issues snow-reports which are copied in many of the local daily papers throughout the region. The Associated Press also distributes information.

A relatively new venture is the broadcasting service of the Yankee Network, which is in constant radiotelephone communication with their observer on Mount Washington, and also receives reports by short-wave radio from other radio stations. Salvatore Pagliuca, formerly connected with the Mount Washington and Blue Hill observatories, is in charge of the new service which has opened a special office and meteorological station on top of the Buckminster Hotel in Boston. Their special feature will be not only reporting but forecasting.

(S. Pagliuca)--Tropical Gulf air this year has raised havoc with the snow-cover in New England. Pinkham Notch lost more than 19 inches of snow in less than a week. The skiing has been abruptly washed out in the last few days, except in the immediate vicinities of Mount Washington and Mount Mansfield. The annual downhill race from the summit of Mount Washington over the headwall of Tuckerman's ravine had to be temporarily postponed because of unfavorable conditions.

(b) Montana (H. T. Gisborne)--The economic value of winter sports, including a canvass of local merchants to determine the present and to indicate the future public interest in this possible application of snow studies, was studied. A preliminary canvass of the ten principal winter-sports dealers in Missoula shows a public expenditure this winter of about $20,000 for 20,000 total population of this community. This is clearly a material interest, entirely independent of the value of snow as a source of municipal water-supply, irrigation-water, flood-damage, etc. It is conceivable that over a period of years this investment by the public may amount to as much or more than the cost of some water-conservation and flood-control measures requiring expensive dams.

As a further indication of the increasing interest in winter sports, in the northern Rocky Mountain Region developments of the past winter include among several others: (1) The operation by the Milwaukee Railroad of a ski-train from Missoula to the Upper Blackfoot, carrying 370 skiers on the first trip and 320 on the second; (2) the operation of ski-trains out of Wallace, Idaho, and Helena, Montana, to nearby natural ski-courses.

(c) Lassen Volcanic National Park (John C. Preston, Superintendent)--Skiing will be good in Lassen Volcanic National Park until late August, but after that winter-sports enthusiasts will have to wait until next year's winter storms, according to Chief Ranger Eugene J. Barton, who has just returned from a ski patrol-trip through the Park.

The depth of snow at headquarters is 79 inches at the present time. With 23 feet of snow at Lake Helen, excellent skiing conditions shall continue until August. It will not be necessary to go to Chile or other South American countries for skiing this summer.

Equipment

(a) Snow-samplers--Two solders have now been found suitable for joining steel to duralumin or duralumin to duralumin. These are Alcoa T by the Aluminum Company of America and Alunize by the Coventry Aluminum Engineering Products Company, Coventry, England. The process is practically identical with that of ordinary solder. The temperature of these solders when flowing is only 392° F or 100° F below ill effects on the cutter. An ordinary wood fire, if necessary in the field, can be used. Joints can also be soldered instead of shrunk. Thus the sampler of duralumin can be constructed solidly as in the case of steel. However, curvature of the tubing during the stamping of graduations and numbers still persists. This could apparently be corrected by the use of a form-roller.

The persistent tendency of snow in near-freezing weather to adhere to duralumin and thus clog the sampler has been completely checked by coating the metal heavily with shellac. A test will be made of enamels as being more durable. The problem is apparently one of heat-conductivity. Philip S. Cowgill and John T. Ryan are conducting the tests for the Nevada Agricultural Experiment Station.

An improved and more effective spanner-wrench of duralumin has been designed to release the couplings, which adhere much more readily than in the case of steel. However, R. C. Farrow, Hydrologist, Water Rights Branch, Province of British Columbia, has found that "most of the seizing of the joints occurred where the edges of the couplings came into contact rather than in the threads themselves." Therefore, to prevent the two duralumin surfaces from scaring, thin brass washers were inserted between the couplings so that the bearing surfaces were brass against duralumin. A test was made with a 7-section sampler in deep, dense snow with heavy intermediate crusts. Despite driving the sampler by standing on the driving wrench and twisting the tube, the sections were unscrewed without difficulty with bare hands, without resorting to the coupling wrenches. However, anti-seize compound was also used. As compared with this, a 3-section sampler without brass washers but with anti-seize compound after driving in soft shallow snow could be unscrewed only by a vise and a pipe-wrench. The efficiency of the brass washer will naturally depend upon the original looseness of the threads.

There seems to be a tendency on the part of the tubing to bend or buckle under the weight of driving in deep or hard snow. A repeated test should be made in depths of 30 feet in order to determine the maximum strength of the sampler.

(b) Snow-course markers--Because of weight and tendency to chip, snow-course marker of enameled metal will probably be abandoned for Masonite board. Ralph L. Parshall, United States Bureau of Agricultural Engineering, has found this board quite unaffected by saturation in water and subsequent drying. He, therefore, concludes that "Masonite board, if painted with an enamel base and with a liberal coat of linseed oil on the reverse side, should provide ample protection for such a sign over a long period of years. I believe that under ordinary exposure these Masonite marker-signs should last more than 20 years."

Since the test-pieces were relatively small, larger pieces simulating a marker should be tested. A test also of alternating melting and freezing should be conducted to determine the effect of extreme weather-conditions.

(c) Storage snow-gage--A compact storage snow-gage has been assured by the United States Weather Bureau by changing the salt solution (sodium chloride) to a solution of calcium chloride. Ashton R. Codd, Hydrologic Supervisor, found that the former congealed at a temperature of -6° F, thus rendering the more exposed mountain snow-gage units ineffective.

A very successful project is being conducted by the Bureau in the comparison of the accuracy of several units of "Shielded-storage precipitation-gages" with that of adjoining snow-surveys. The shields--variations of the original "Nipher screen"--have been developed by J. C. Alter, Meteorologist.

(d) Snow-rain recorders--The Stevens snow-rain recorder, long under development, has had its first successful winter test on Mount Hood. It should, however, be subjected to conditions of rime and intense cold, and its recorded catch should be compared with the water-content of adjacent snow-survey courses.

Polar regions (Laurence M. Gould)

(a) Arctic--The events of chief importance in the Arctic regions are those concerned with the continuation of the comprehensive survey under the leadership of Professor Otto Schmidt of the U.S.S.R. First place should be given to the establishment of the party at the North Pole and to the safe return of the whole party with complete records after a drift which ended off the east coast of Greenland. Significant to oceanography and meteorology are the outstanding results of this expedition. The two successful flights of Soviet airmen across the North Pole to the United States must rank with the greatest flights yet made in the polar region.

To give details of all the other aspects of Soviet Arctic exploration would take more space than this report should have. There have been no less than 15 expeditions sent out from the Arctic Institute of Leningrad which is under the direction of Professor R. L. Samoilovitch. These have been directed chiefly to the study of the Soviet Arctic coast-line and adjacent regions.

The writer was a delegate to the Seventeenth International Geological Congress which met in Moscow during the last ten days in July 1937. Following the Congress he joined a small party which boarded the Vologda at Archangel and spent the month of August studying the geology and glaciation of Novaya Zemlya. Particular attention attaches to this ice-sheet for it is no longer receiving an appreciable amount of nourishment. The heavy winds sweep the snow into the sea. As a result it presents interesting phenomena indicative of extremely rapid retreat.

Comprehensive studies of the various aspects of the natural resources of Svalbard (Spitzbergen) have been carried out by several expeditions under the general direction of Dosent Adolf Hoel.

Greenland has been a center of attack from many different countries and for a variety of purposes. Of first importance is the continuation of the brilliant work of Lauge Koch in East Greenland. Four different stations were established to carry out the varied and comprehensive program of research. There have been no less than ten other expeditions to Greenland from Denmark, Norway, Great Britain, France, and the United States.

Iceland has been the scene of six expeditions, three of which have been from Great Britain. These have added detailed information to our knowledge of the glacial phenomena of this island.

Arctic Canada and Alaska have been fields for intensive study by Canadian, British, and American expeditions. J. M. Wordie renewed his activities in the Arctic with an expedition to the Canadian Arctic as well as West Greenland. The important work of Beutham continues in Southern Ellsmere Land. The British-Canadian Arctic and the British Arctic expeditions continue their detailed studies in Arctic Canada. MacGregor, a meteorologist of the United States Weather Bureau, leads an expedition in Ellsmere Land which is carrying on meteorological and geological surveys.

Six other major expeditions have undertaken work in other parts of the Arctic, including the conquest of Mount Lucania, hitherto the highest unclimbed peak in North America, by Bradford Washburn.

(b) Antarctica--Of paramount interest in the South Polar regions has been the successful completion of the three-year expedition under Rymill, known as the British Graham-Land Expedition. A well-organized and integrated program employing sledging and aerial operations was carried out. Much new geological and glacial information was brought back. Chief interest attaches to changes wrought in the map by the surveys made chiefly by the sledging parties. These have demonstrated that the concept of the Graham-Land Sector as an archipelago as mapped by Wilkins from the air in 1928-29 is entirely wrong. Graham-Land is reestablished as a great peninsula from the main continental land-mass.

The research work of the R.R.S. Discovery II and R.R.S. William Scoresby has been continued with new information added about ice-conditions as well as other matters.

Norway was represented by an expedition under the command of Consul Lars Christensen which represented his fourth visit. New land was discovered between Queen Maud Land and Princess Ragnhild Land and great stretches of the coast were accurately mapped from the air.

Dr. William H. Hobbs, after research in America and Europe, has definitely ascertained that Antarctica was discovered from America--however, in the search for whaling grounds.

The Federal-State Cooperative Snow-Surveys

The cycle of development in snow-surveying is now passing from organization to that of administration and cooperation. W. W. McLaughlin, director of the cooperative system, writes: "While the research and investigational side of snow-surveying and irrigation water-supply forecasting has its problems, there are developing, as the work progresses, many administrative questions. As you realize fully, the field-work is carried on by the snow-surveyors for only a very few days in the entire year and consequently the employment of temporary labor or assistants to do this job is about out of the question. This, together with many other factors, makes snow-surveying a cooperative project by very necessity. In this connection the Forest Service, Park Service, and many private agencies have cooperated wholeheartedly and have done a fine job which I wish to acknowledge. The State Agricultural Experiment stations and the State engineers have likewise assumed their full share of responsibility and altogether these agencies have made it possible for us to cover the West as completely as has been done during the past two years--with the small amount of Federal money available for snow-surveying."

U. S. Weather Bureau mountain-snowfall stations

Under the River and Flood Division, the United States Weather Bureau is signalizing its fortieth year of mountain-snowfall reporting in the West by so extending its network of precipitation-stations that "elevation, aspect, and individual needs of the various basins will be taken into account.

"The expanded program of the River and Flood Division of the Weather Bureau embraces the ultimate establishment of approximately 3,400 additional precipitation-stations, about 1000 of which will be equipped with storage snow-gage units. All hydrologic work of the Bureau is being regionalized.

"Of the 20 snowfall-stations now equipped with storage snow-gage units, 13 have been established in the Sangre de Cristo Mountains of New Mexico, four in the Wasatch Mountains of Utah, and three in Yellowstone National Park."

The union of snow-survey systems with the network of mountain precipitation-stations should furnish a stable basis for forecasting water-supplies. Preliminary investigations of recording snow-gages indicate their feasibility, the use of which will provide details of snow-accumulation.

Snow-survey conferences

The development of regional meetings in the Western States by the Section of Hydrology has facilitated the holding of snow-survey conferences in connection with them. Two conferences have been held the past year.

The South Continental Divide Snow-Survey Conference at Denver in June, 1937, was organized by the Regional Committee itself. Special emphasis was given to improvement of snow-survey methods and instruments and to a presentation of problems by the United States Soil Conservation Service and the United States Weather Bureau.

The South Pacific Coast Snow-Survey Conference at Davis, California, in January, 1938, was held by the Western Interstate Snow-Survey Conference, which decided to expand its field to Canada in order to include in its Executive Committee neighboring scientists who had added impetus to the work. Continued affiliation with the Union was generously assured by the officers of the Union and Section of Hydrology with inclusion of its proceedings in the Transactions of the Union. The chief themes of discussion were: (1) The expanded program of the United States Weather Bureau in testing the accuracy of seasonal gages; (2) snow-cover conditions for winter sports and the broadcast program by the United States Bureau of Agricultural Engineering; (3) the economic value of snow-surveying; and (4) further improvement of snow-survey apparatus.

These general conferences were supplemented by a series of State and drainage-basin conferences for purposes of discussing the seasonal snow-survey data and making a forecast of probable runoff. A meeting of Columbia River forecasters was held at Portland on April 12, 1937.

An Eastern Snow-Survey Conference was held at Boston on October 8, 1937, in connection with the fall meeting of the American Society of Civil Engineers. The leaders in the movement were: Merrill Bernard, Chief, Division of Rivers and Floods, United States Weather Bureau, Washington; Leslie F. Conover, Meteorologist, Hartford; and H. B. Kinnison, District Engineer, United States Geological Survey, Boston.

For the past several years, there had been discussions regarding the desirability of making more complete snow-surveys throughout the New England States, but the present meeting represented New York also.

Reports of surveys already organized and active were made as follows: Arthur W. Harrington, United States Geological Survey, Albany, for the New York Cooperative Snow-Surveys; M. R. Stackpole, United States Geological Survey, Augusta, for the power-companies in the Androscoggin and other river-basins in Maine; and H. M. Nelson, Chief Hydraulic Engineer of the New England Power Company, for the snow-survey done by his Company for a number of years. Mr. Kinnison outlined a possible program for his district by the Geological Survey. Mr. Bernard, as chairman, outlined the nature of the work and type and extent of the data desirable to be collected. Samples of snow-sampling equipment were examined and arrangements were made for the procurement by various organizations of a number of sets. "However, there is not as yet any fully organized snow-survey project in New England."

Research

Research has continued steadily in the conservation of snow by the California Agricultural Experiment Station and in the improvement of snow-survey apparatus by the Nevada Agricultural Experiment Station. Records for the fifth year are being collected from ten different conditions of vegetative cover by the former. A study of duralumin for instruments is being made by the latter.

R. C. Farrow has found brass washers a desirable preventive for seizing by duralumin. Ashton R. Codd has protected storage precipitation-gages against the effects of freezing by substituting calcium chloride for sodium chloride in the reservoir. John C. Stevens has completed a long-period snow-rain recorder. Ralph L. Parshall has found Masonite an inexpensive, light, and durable substitute for enameled metal in making snow-course markers. These various items have already been discussed in detail under "Conservation" and "Equipment."

Supplementary to the above projects is a series of suggestions received in reply to a Committee questionnaire. These extend beyond the narrower field of hydrology to health and biological research.

(1) Snowfall--The present purpose of expanding mountain snowfall-stations by the United States Weather Bureau is to obtain basic data of snowfall and snow-cover for the better understanding of potential water-supplies and flood-hazards. The development of a long-period snow-rain recorder looks toward determining intensity as well as total quantity of precipitation in areas difficult of access.

Robert E. Horton suggests a specialized series of snow-maps: "The United States Weather Bureau has for many years published maps showing depth of snow on ground, and I wish someone might undertake an analysis of these data and prepare a map showing two things: (1) The probability of snow-cover at a given date; (2) a map showing the portion of the eastern United States which is normally covered with snow on each of a series of given dates, as, for example, the first of the month, in each winter month."

Ashton R. Codd outlines the present development of mountain snowfall-stations by the Weather Bureau: "Complying with your request for a statement from the River and Flood Division of the United States Weather Bureau relative to their activity in snowfall-work this present season, I hesitate in making any lengthy statement at this time. However, I feel at liberty to tell you that the Division has installed several units of shielded storage precipitation-gages. These units are comprised of five gages each and are installed at the following locations: Three in Yellowstone Park between 7000 and 7500 feet elevation; one at Brighton (Silver Lake), Utah, elevation 8700; one at Kimberly, Utah, elevation 8000; and one at Clear Creek, Utah, elevation 6900. Thirteen units were established in the Sangre De Cristo Mountains in New Mexico on the Rio Grande River drainage-basin."

Where extremely cold temperatures were encountered, it has been necessary to change the solution in the gage from sodium chloride to a solution of calcium chloride, since the former will become eutectic at a temperature of only -6° F, while the latter can be subjected to a temperature of -59°.8 F, before doing so.

(2) The relation of snow-cover to runoff--The effect of snow-cover under temperature and rain even at higher elevations has become a leading problem because of the flashy floods in the Sierra last December. Likewise, the excess in runoff above the percentage indicated by the snow-cover in the Little Humboldt Basin, Nevada.

The distinction between snow- and rain-problems is emphasized by LeRoy K. Sherman, Chairman of the Section of Hydrology: "I am such a novice on the phenomena of snow that I should not give very much advice. I am of the opinion, however, that the most important phase of hydrology of snow is the coordination between the snow-survey and subsequent rates and amounts of runoff. Unlike runoff from rainfall, I assume that snow is subject to large evaporation-losses, whereas with the rainfall-runoff relation, infiltration and interception are the principal losses, and evaporation is a very minor quantity, so far as direct surface runoff is concerned."

W. W. McLaughlin proposes three projects: "In the field of research the question of frost in the ground beneath the snow at the time of melting needs some further investigation. If the ground is frozen at the time of melting, the immediate surface-runoff is of course much greater and the soil storage of water is much less. The consensus of opinion as expressed at Denver and elsewhere is that at the time of spring-melting there is no frost in the ground. This needs verification and especially in the northern climates.

"Another factor that needs investigation is the effect of soil-moisture contained in the ground at the time of spring-melting on the rate of water-yield from the melting snow. Two years ago, for instance, when the watersheds generally were very dry, the first snows and permanent frost reduced the runoff from the snow-pack and materially affected the forecast of water-supply.

"Another field of investigation is the correlation of snow-pack and stream-flow. The methods now in use are laborious and time consuming."

Paul L. Bean seeks a standardization of procedure: "The problems which need to be investigated in the Androscoggin River area are snow-evaporation, absorption of snow-water by ground, capacity of ground to absorb water, and maximum rates of runoff due to melting snow. All problems in the above are being studied here with a view to formulating a permanent method of procedure and we have corresponded with Robert E. Horton as to a proper method of procedure in order that our investigations may conform to general practice throughout the country."

Carl Elges, Forecaster of the Humboldt, comments as follows on the high water in the Little Humboldt: "In the Little Humboldt Basin in Nevada the snow-cover on March 1 was approximately normal and indicated that the runoff would be about normal. However, a survey made on one key-course on April 1 disclosed the fact that the snow-cover had increased over 240 per cent during the month. Since no previous data were available for such conditions, no definite value could be placed upon the increased snow-cover. It was, however, predicted that the flow would be far in excess of normal. This contention has been realized for the streams have been reported as exceeding any previous yields since 1910. It is believed that a similar increase in snow-cover during April 1932 made the yield that year exceed the forecast by over 100 per cent. Unfortunately no measurements in April were made at that time.

"There was also an increase in the snow-cover on the Main Humboldt Basin. However, it was only about a 50 per cent increase. Since the March 1 snow-cover was very deficient, 63 per cent, the forecast was raised to only 70 per cent of normal. At this time (June 5) it appears that the forecast will be quite close. Thus for the main Humboldt, the effect of March snow was evaluated quite successfully.

"However, for the Little Humboldt, a study of the effect of late snow on the yield will be very valuable since no such data have been previously recorded. Furthermore, with the super-normal snow-cover and large runoff, high figures will be available for comparisons of such recurrences."

(3) Ground-water, or pre-snow-pack, storage--The study of ground-water storage is specially emphasized. Eric Hinton of Newfoundland suggests wells: "An important uncertainty in preparing our annual spring-flood forecast is our lack of knowledge of the state of the ground-water. To overcome this we propose to install wells at various points within the sub-areas of the watersheds. These wells would consist of 4-inch pipes, 12 feet long, pointed and perforated at the lower end. The pipes would be driven into soft soil. The water-level in the pipes would be ascertained at the time of making the snow-survey and at any other times considered desirable. These observations would probably have to be continued for three or four years before attempting to determine their significance."

J. H. Ryan reports the taking of test-cores of soil-moisture in Oregon, apparently in expansion of similar tests in Utah: "This year personnel of the Medford Experiment Station, Bureau of Agricultural Engineering, have begun taking test-cores with a soil-auger at each of the snow-survey courses for the purpose of determining the ground-moisture before the snow-cover begins to accumulate. Possibly this information will help us to iron out seeming inconsistencies in results from some of our stations. The State Engineer's office has arranged to assign some time to the study of correlating the snow-survey results with stream-runoff. Although the work is just getting started, results so far appear promising."

Harlowe M. Stafford notes some correlation between the California forecasts for 1937 and "pre-pack" precipitation: "Forecasts for 1937 were essentially upheld. In the northern watersheds of the Western Sierra slope the runoff in general was slightly less than the forecasts while in the southern watersheds of Kaweah and Kern it was slightly in excess.

"It is interesting to note that there appears to have been some correlation between the amount and direction of these departures from the forecasts and what might be termed the "pre-pack" precipitation. At the end of January, 1937, seasonal precipitation ranged from 45 per cent of normal in the Sacramento-Pit watersheds to 90 per cent in the Merced Basin where the deficiency ceased, and to 115 per cent in the Upper San Joaquin and 140 per cent in the Kern Basin.

"Recognizing that ground-water storage at the time the snow-pack starts to accumulate is a modifying factor to be considered in the April to July runoff, the correlation above suggested has led to the thought that this modifying factor might possibly be evaluated in terms of "pre-pack" precipitation. (Where actual determination and evaluation of ground-water conditions are not feasible.)

"The Division of Water Resources has had substantial success in evaluating the influence of abnormal precipitation during the runoff-period and it may be that the influence of ground-water storage can be evaluated in terms of "pre-pack" precipitation in the same manner. At any rate, this is a suggestion for research for whatever it may be worth."

R. C. Farrow reports similar interest in the Columbia Basin: "I attended an interesting meeting of the Columbia River Forecasters at Portland on April 12. A great deal of interest was evinced in the problem of soil-moisture content at the beginning and end of the snowfall-period and its effect on the snow-runoff ratio. I outlined my approach to the problem through the medium of autumn precipitation. Jimmy Marr had worked on parallel lines for one of his areas. This is a worth-while field for further research as outlined in my memorandum to you re 'Farrow Project' of Dr. Bowie's Research Committee."

The project referred to has now been formally accepted by the Planning and Project Committee of the American Geophysical Union for consideration by the National Water Resources Committee.

(4) Evaporation of snow--E. S. Cullings of the Black River Regulating District, New York, urges work in the laboratory on the evaporation of snow: "We have undertaken no new line of research work during the past season, because of lack of staff and funds. I should like some time, however, to see an investigation made of the physical conditions under which snow is evaporated without melting. I think it is a laboratory job which would not be especially difficult, but up to the present time I have found no organization willing to undertake the necessary work."

(5) Weather and snow-cover on highways--As noted by J. D. Mac Vicar under "Snow-removal," above average temperatures and light rainfalls in the mountain areas of Washington have had the effect of compacting the snow and rendering it less susceptible to sliding. The picket-type of snow-fences is effectively reducing the effort necessary to keep the highways open to traffic.

(a) Sleet--At the Denver Regional Meeting in Hydrology, Salvatore Pagliuca outlined the possible procedure for forecasting the occurrence of sleet on power-lines and highways.

(6) Mine-drainage--George A. Lewis introduces the project of mine-drainage: "Because I know you will be interested, I am enclosing a letter from Mr. Mesavage, Forester of Susquehanna Collieries Company of Glen Lyon, Pennsylvania.

"The idea of utilizing snow-surveys in connection with mine-caves may not have occurred to you and it certainly is a distinct departure. Would it not provide a valuable field for research in rate of percolation?"

Stanley Mesavage says: "I want to thank you for the publication on snow-surveys. I find it interesting and helpful. Regarding mine-caves, I wish to say that our surface area of 5000 acres is punctured with mine-caves that came about as a result of mining operations underground.

Since water-control is a problem of major importance to us, I find that if we could forecast the amount of water that would enter the mines through these caves as a result of thawing, we would probably be able to control some of it or prepare ourselves accordingly. Spring thaws always cause some trouble with rapid melting snows and runoff."

(7) Snow-cover and health--Robert E. Horton says: "Here in eastern New York we have had three heavy falls of snow, perhaps about the normal amount, but it has come unusually early, and because of the abnormally warm winter, the ground has remained bare most of February and is now bare, even in the hills. As soon as the ground becomes bare it seems to start an epidemic of certain diseases--flu, pneumonia, and childrens' diseases, such as chicken pox and measles. I am wondering if there is not a rather close relationship between the occurrence or absence of snow-cover and the development of the germs of these diseases. This thought might lead into a quite important field of research in relation to the human side of the effect of snow and snow-cover."

(8) Snow-friction and ski-sports--Robert G. Stone has learned that G. J. Klein of the National Research Council of Canada, Ottawa, has done research on the physics of skis for airplanes. Mr. Stone is working on a classification of snow for ski-sports and is preparing a snow-map of winter-sports areas for the United States National Park Service.

(9) Biological research--The following correspondence is self-revealing. William H. Marshall, United States Bureau of Biological Survey, Logan, Utah, says: "Professor Earl G. Clyde of the State College here has informed me that you are President of the International Commission on Snow. I am writing to you to ask you for a pertinent reference, if any, on the factors influencing the formation of snow-crust in high mountain areas. My query is intended as a short cut to avoid, if possible, my trying to work with unfamiliar literature. The reason for this request is that it has a direct bearing upon the research-work which I am attempting to carry on as regards the rare fur bearers of the Rocky Mountain Region. As you know, these animals--marten, fisher, otter--are active chiefly in snow-areas, and my work is concerned with studying their activities and life histories, largely on the basis of tracks, which, of course, would be influenced by snow-crust." (February 14)

"Apparently, marten and fisher live at the high altitudes to which you refer, although very little is known about them. Possibly the reason you had not seen them in your travels in Nevada is that they have been trapped out, as they are in most of the country. I hope to be able to gather information the next three or four years which will be of help to bring back these animals, since they are a definite economic asset." (March 1)

Arthur H. Blake, Sierra Club, under date of April 16, says: "The letters from William H. Marshall of which you sent copies are very interesting. This year I noted numerous marten tracks up Big Pine Creek below the Palisade Glacier; also I am sure towards Castle Peak north of Donner Summit. I am not an expert on tracks but Norman Clyde who was with me confirmed identification of the marten tracks. I will try to make a brief report and send it to Mr. Marshall. Perhaps our fellow winter-mountaineers can help next winter."

Publicity

Basic snow-survey and irrigation-water forecasts are now being provided through mimeograph and radio by the Federal-State Cooperative Snow-Survey System led by the United States Bureau of Agricultural Engineering. Forecasts are issued for the Missouri and Arkansas rivers, Rio Grande Basin, Columbia River Basin, Colorado River Basin, Utah, Nevada, California, and Oregon.

Forecasts are issued for the international St. Mary and Milk River basins jointly by the United States Geological Survey and Dominion Water and Power Bureau. The latter also publishes a snow-survey of the Bow River Basin, Alberta. British Columbia is cooperating with the United States in forecasting the runoff from the Columbia Basin.

The New York Cooperative Snow-Surveys also issues a report for the streams in the Adirondacks. Individual reports are published by local organizations.

The United States Weather Bureau issues its Mountain Snowfall Bulletins and publishes the cooperative snow-survey data. The United States Geological Survey maintains stream-flow measurements in cooperation with the States.

By cooperative effort the Proceedings of the Snow-Survey Conferences are being published by the American Geophysical Union. A comprehensive mailing list has been prepared by the Nevada

Agricultural Experiment Station. The requests for reports and reprints have become so numerous that the issue of 1600 copies for the Proceedings of the Denver Meeting was insufficient. It is planned to increase the issue of the Proceedings of the Davis Meeting to 2000. The following organizations have shared in this work: Nevada, Utah, and Colorado Agricultural Experiment Stations; United States Bureau of Agricultural Engineering; United States Weather Bureau; United States Soil Conservation Service; Los Angeles Department of Water and Power; and British Columbia Water Rights Branch. Other organizations have offered to aid in extending the work.

Movie films of snow-surveying have been made by Los Angeles Department of Water and Power, Utah Agricultural Experiment Station, British Columbia Water Resources Branch, and by Frank Whitman Wilson and Walter J. Herz in connection with the Nevada Cooperative Snow-Surveys. Talks on snow-surveying and water-prospects are being invited by service clubs.

However, the field for publicity is still wide. W. W. McLaughlin invites further activity: "Another matter for considerations is the question of dissemination of our forecasts, especially to industrial and business organizations in the East. I believe these results are not sufficiently publicized and that greater use could be made of our work than is now in evidence."

International Commission of Snow

The agenda of the International Commission of Snow for the triennial meeting at Washington in 1939 were discussed in the Annual Report of 1936-37.

Through the generosity of the International Association of Scientific Hydrology, some private funds and the hope of reimbursement through sales, the Transactions of the International Commissions of Snow and Glaciers of the Edinburgh Meeting in 1936 are now being printed at Riga. The volume will probably be ready for distribution in July or early August.

In order to lay broad foundations for future research in fields of snow and ice, all of the papers presented at Edinburgh have been published with illustrations except those that have already been printed in Bulletin 22 of the Association or in accessible periodicals. The present volume, Bulletin 23, therefore has reached the unusual total of 800 pages. Furthermore, the number of copies has been increased from 600 to 1000 in order to obtain the widest circulation possible.

The scope of the book is evidenced by the number of papers listed under classifications as follows: (1) General, 4 papers, 71 pages; (2) snow-cover, 8 papers, 73 pages; (3) forecasting stream-flow, 7 papers, 126 pages; (4) runoff, 5 papers, 71 pages; (5) lake- and river-ice, 5 papers, 61 pages; (6) transport, 5 papers, 42 pages; (7) physics of snow and ice, 11 papers, 146 pages; (8) avalanches, 3 papers, 110 pages; (9) glaciers, 9 papers, 68 pages; (10) apparatus, 4 papers, 27 pages.

International Congress of Cryology

In the Annual Report of 1935-36, a statement was made that the Permanent Committee of the Hydraulic Conference of the Baltic States decided not to organize an International Congress of Cryology but rather to seek admission to the International Commission of Snow.

In order to clear the historical record at the suggestion of Dr. Antoni Dobrowolski, sponsor of the Conference and now generously Vice-President of the International Commission of Snow, the following paraphased excerpts from the Minutes of the Baltic Bureau are published:

"On July 17, 1934, the Baltic Bureau decided to approve Dr. Dobrowolski's proposal of the organization of an Independent International Congress of Ice which shall serve as a first step in a permanent International Union for Ice-questions and to take cognizance of the communications of Director Markov and Professor Liakhnitsky that this Congress might be held in Leningrad. However, this decision would not bar the consideration of ice-questions at the V. Conference." (This action followed the proposal of Liakhnitsky that independently of the V. Conference, a special Ice-Congress be summoned in Leningrad and indeed not later than in the year 1937.)

"It was further decided to restrict ice-problems at the Baltic Conference to those local to the Baltic States and to refer to the Permanent Bureau the formulation of proposals for cooperative effort in ice-questions.

"The proposal of Dr. Renquist was unanimously accepted that the organization of an Ice-Commission could be temporarily shelved. It was also decided to support Director Zubrzycki's proposal to unite with the Commission of Snow."

Summaries of leading publications

The present year, because of increasing interest in the physics of snow and ice with reference to snow-sports and exploration, the following reviews of the "Snow-terminology of ski-runners" and Zukriegel's "Cryologia maris" are given by Dr. Henry I. Baldwin and Robert G. Stone of the Committee, respectively.

The snow-terminology of ski-runners

(Review by Henry I. Baldwin, Forestry and Recreation Department, State of New Hampshire, Hillsboro, New Hampshire)

People who travel on snow and use it for sport have developed descriptive terms for different snow-conditions, which, although they are rarely based upon a scientific study, may be of interest. Two groups which have given especial attention to snow classification are mountaineers and ski-runners. The former have had to deal with alpine conditions and frequently so-called perpetual snow-fields, where seasonal variations are important. The latter group is perhaps more interested in changes in texture and surface from day to day or even from hour to hour. Seligman (1936) has given us a careful scientific analysis of snow-types with special reference to alpine conditions, which may well serve to stimulate more detailed study of snow-structure in various lands as has been done by Nakaya _et al_ (1936). References to snow-conditions in most popular ski-literature consist usually of brief mention of different snow-types. Some of these are enumerated here. The review is by no means complete, being limited to publications at hand. It is hoped they will furnish an adequate picture of the prevalent usage.

(1) _European ski-literature_--One of the earliest handbooks of the modern era of skiing is that by Fritz Huitfeld (1896) who speaks of "loose snow-skis" and "crust-skis," two different types of equipment used in Norway at that time for different snow-conditions. Paulcke (1905) makes little reference to snow. Hoek (1910) calls sticky snow "pasty" snow. Fendrich (1910) devotes some space to a discussion of "Pulverschnee," the elysian powder, "Pappschnee (not dry enough and not wet enough)" and then various degrees of körnigen Schnee--Bretterschnee which is stone-hard, verkrusteten Schnee (breakable), Schneeharsch, mehligen Sohnee, and plain Harsch. He quotes Sverdrup who says in his "Neuland" that he knows "hundreds of kinds of snow, but can never say definitely when snow sticks and when it does not." Rickmers (1910), after such general characterizations as "good snow" and "bad snow," mentions "eggshell crust," marble and wind slab. Caulfeild (1914) makes a very sensible suggestion, valid today, when he states (p. 256): "For practicable purposes the ski-runner may consider the snow to be of three distinct varieties, according to the consistency of its surface, namely, soft snow, hard snow, and breakable crust." On the next page, however, he describes also "deep loose snow" and "watery spring-snow." In a later book Caulfeild (1923) elaborates as follows: "_Sticky snow_: The only snow that can stick is snow that is half wet and half dry-powder snow of which the very surface is just being thawed. Hard snow can become breakable crust or soft snow, and breakable crust can become soft snow." Or again: "Ice, when superficially softened becomes, according to its original thickness and to the extent of the softening, either hard snow, breakable crust, or soft snow. These terms ice, hard snow, soft snow, and crust are used to indicate not the appearance or actual nature of the surface, but simply the sort of turning that it permits."

Arnold Lunn (1913, 1920) has given more attention to classification of snow and its description than any other English author up to the appearance of Seligman's book. His classifications are as follows:

Winter snows

(1) Powder-snow.
(2) Trap crust--a crust solid at one point, breaking at another; more common in spring than winter.
(3) Marble-crust--a solid crust formed in winter by wind-action; hard, slippery.

Spring snow

(1) Telemark crust--a hard crust superficially softened to a depth of an inch or two by the Sun.
(2) Wet, thawed snow--the midday sequal to (1); both of these are often erroneously called corn-snow in New England.
(3) Spring powder--this is more compact, less powdery, and not as fast as the dry powder of winter.
(4) Perforated crust--a rougher-textured crust, covered with holes.
(5) Film crust--a hard under surface of solid crust, covered by a very thin transparent film of soft ice.

In his later book "Alpine skiing at all heights and seasons" Lunn elaborates further on these and other snow-types.

In Sweden, Dyhlén (1919a, 1919b, 1932) also gives especial attention to spring snows, "skare," "bårande skare" (unbreakable crust, cold, hard, and crystalline from action of Sun) which softens to a "dagsmeja" (telemark crust) by day. The melt-water sinks and makes the underlying layer porous. Winter snows are "lős" (loose), or "torr" (dry powder), "klabb" (sticky), "mjölig" (mealy). The formation of "skavler," a Norwegian word for wind-formed snow-waves, is illustrated by diagram.

Dyhlén and Rimfors (1931, 1932) have described Swedish snow-conditions as follows:

(1) New snow.
(2) Powder-snow--new snow which has lain a while and contains less air-space; it supports weight better, and has greater adhesion; there is less risk of freezing (to the ski), and is slower than new snow.
(3) Wind-packed snow--this is characterized by still greater adhesion, and an uncrystalline, amorphous structure like powder.
(4) Fine-grained snow--this exhibits but slight adhesion; slight thawing causes it to become small ice-crystals which become densely packed.
(5) Corn-snow--long-continued, or bad thaws and subsequent freezing reduce the snow to ice-pellets like coarse salt; it often forms unbreakable crust; adhesion is insignificant.

Amundsen (1923) also gives several descriptive terms current in Norway: "Snow can be of many kinds, fresh or old, mealy, loose, heavy, crystalline, corned, icy, exposed to greater or less cold, dry or more or less damp." Recently Petersen (1934) has described dry powder-snow, sticky snow, wet snow, and corn-snow--large-grained snow of small ice-crystals.

More recent German books like Fank and Schneider (1926), Lantschner (1936), and others follow much the same classification of fresh-fallen snow, powder-snow, sticky snow, wet snow, crust and corn-snow. The latter book contains excellent photographs of crystalline snow, crust, wind-pressed snow, new snow, wind-crust, etc.

(2) American literature--One of the earliest American booklets on skiing (anonymous 1905) mentions a formidable number of adjectives applied to snow (pp. 6-7): "Then, too, the snow itself is capable of many wonderful changes, including downy, fluffy, powdery, sandy, dusty, flowery, crystalline, brittle, gelatinous, salt-like, slithery, and watery forms. Some snow will be found as hard and white as marble, other snow forms a thick crust which breaks up into big slabs." Proctor (1932) discusses powder-snow, dry snow, dry-wet snow (which will not quite make good snowballs), damp snow, wet snow, old snow, hard icy crust, and wind-crust. Proctor and Stephens (1933) add corn-snow, wet corn-snow, and icy snow. In a later book (1936) they mention only (1) powder-snow, dry snow, (2) damp snow, and (3) wet snow and corn-snow. Dudley (1936) mentions powder-snow, clogging snow, crust, and corn-snow or spring snow. Peter Lynn (1935) attempted a classification by temperature when he termed very cold snow that which is -10° C or below, cold snow and medium snow with progressively higher temperatures, and finally falling snow and wet snow. The only classification which has been proposed for general use is that compiled by Park Carpenter, editor of The Ski Bulletin, for use of cooperators in reporting snow-conditions to the Bulletin. It is given in full as follows:

Soft-snow conditions

Fluffy: Large, light, feathery flakes. Scooped up in the hand, it may be blown off like feathers. Even a tennis-ball would drop out of sight in an accumulation of sufficient depth.

Powder: Dry, cold, powdery snow; the perfect snow for sleighing and all winter sports.

[Note: Wherever fluffy or powder-snow is reported it should be followed by a figure giving the approximate depth in inches to which the fluffy or powdery condition extends; thus, "powder two."]

New: Fluffy or powder-snow that has fallen within the 24 hours immediately preceding the observation should be described also as "new"; thus, "new fluffy" or "new powder."

Heavy: Occasionally a powder-snow, long before changing to crust, becomes somewhat "packed," or "heavy," so that while ski sink in it readily, there is nevertheless a "toughness" to the surface which resists against lateral movement of the ski, causing slight difficulty in turning. Report as "heavy."

Sticky: Snow just beginning to melt. Sticky snow can be used by proper waxing of ski. "Powder-snow begins to stick if the air-temperature rises above the freezing point. Sticky snow adheres in big lumps to the running surfaces of the skis.....Powder-snow begins to stick when it begins to thaw." (Arnold Lunn, Alpine skiing at all heights and seasons, p. 21.)

Wet: A soggy state intermediate between "sticky" and "slush" reached after powder-snow or wind-crust has been thawing for some time. This is the "cotton-batting" snow from which boys roll big balls, build forts and snow-men.

Soft-snow conditions--Continued

Glazed: A light, thin, transparent glaze frequently forms on any one of the above-described sorts of soft snow. It is the result of superficial melting by hot Sun and subsequent freezing. It does not materially hinder use of ski. Report by inserting the word "glazed" just ahead of the word indicating character of soft-snow surface; thus, "glazed powder two."

Slush: Requires no explanation.

Hard-snow conditions (crusts)

Breakable: A crust is "breakable" when it will not hold the full weight of a man on a single ski. This condition renders steering difficult except by jump-turn, and when present should always be reported even though the report also shows several inches of soft snow above the crust. All crusts are first classified as "breakable" or "non-breakable." For practical purposes a person not using ski may test a crust by selecting a board three inches wide and having roughly one inch of length for each two pounds of the person's weight. If the crust breaks when he stands on this board, it is to be reported with the word "breakable."

Wind: "Wind"-crust is frequently formed by action of the wind on powder-snow. Usually ripple marks on the surface reveal the origin of this crust. If breakable, report "breakable wind"; otherwise report merely "wind."

Common: "Common" crust is the crust formed by light thaw and refreezing; it should, if not "breakable," be reported simply as "common."

Icy: "Icy" crust is the hard, ice-like crust formed by freezing of a watery surface after a heavy thaw. Ski, even when sharply edged, get no grip on this surface, which makes skiing quite impossible. Such crust should be reported merely with the one word "icy."

Spring-conditions

Granular: An old, wet snow, in appearance and consistency much like the wet rock-salt used to pack ice-creams. Granular snow is usually found only in the spring and at high altitudes. "Snow that has been melted and refrozen night after night soon acquires a crystalline character....Granular snow yields excellent skiing." (Lunn)

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Cryologia maris [Josef Zukriegel, Travaux Geographiques Tcheques, No. 15, Inst. Geogr. Univ. Charles IV, Praha, 1935, 177 pp., 15 figs., 37 photographs]

(Review by Robert G. Stone, Blue Hill Observatory of
Harvard University, Milton, Massachusetts)

This extensively documented and systematic summary of the knowledge of sea-ice owes its existence to the author's long-standing love of the Arctic regions and to Professor Svambera's unique collection of Polar literature at the Carlova IV University. While the author has visited the Arctic waters a number of times, his work is based mostly on the literature, several hundred sources in many languages being referred to. The treatment is much more extensive than the well-known works of Maurstad and Transehe, but is less original and the photographs are not as adequate.

After a brief introduction which mentions the leading previous contributors to the subject, the history of the study of sea-ice up to Weyprecht's Metamorphosea (1879) is quite thoroughly reviewed, this being probably the most original part of the book. Then the present state of organized research-institutions for ice is very briefly mentioned under the heading of "The importance of cryology." Dr. Zukriegel reveals his great debt, philosophically at least, to Dobrowolski, dean of cryologists. He would divide the subject into a scientific and a technical or applied branch.

Next there comes a 33-page summary of the general character of the north and south polar seas, a compilation of the essential facts of oceanography and meteorology of significance to the study of the ice. The rest of the book (pp. 113-177) is finally devoted to the types and distribution of the ice itself. Here the author attempts a comprehensive international terminology and classification by arranging the equivalents in different languages, a difficult job for one man and one on which the author does not dare to claim more than a beginning. He leans heavily on the work of others, especially on the sea-ice code evolved by ths Soviet Russian Ice Commission, which he believes to be most acceptable, but suggests the question should be exposed to further formal international discussion. In bare outline, the classification (only the English terms are reproduced here) runs as follows:

(A) Accretion (growth) types of sea-ice, formed during the origin and first growth of the ice--
  I. Types formed in the open sea: (1) Ice-crystals; (2) slush, grease; (3) ice-rind; (4) sludge; (5) sludge-lumps; (6) snow-sludge; (7) sludge-ice, sludge-floes (sometimes from anchor-ice or ground-ice); (8) slob-ice (gruel); (9) pancake-ice; (10) young ice, bay-ice, snow-ice; (11) level ice.
  II. Types forming on the shore (fast-ice): (1) Ice-extensions offshore, bay-ice; (2) fast ice, shore-ice; (3) ice-foot.
(B) Secondary types of sea-ice, formed by destruction or construction of primary forms--
  I. Due to destruction: (1) Ice-field; (2) ice-floes, floes including (a) glacons (small floes, growlers, and cakes) and (b) sailing ice; (3) small floes; (4) growlers; (5) cakes; (6) bits; (7) brash.
  II. Due to construction (heaping and piling): (1) Standing blocks; (2) hummock, pressure-ridges, muddy ice, rafted ice (tented ice), toros, floeberg, including (a) marginal versus complete crushing, (b) drifting versus grounded, (c) by age, height, and extent; (3) stranded hummocks (or toroses); (4) pack-ice, pack including (a) by age, (b) thickness (light, heavy, rafted, tinted, or telescoped), (c) level, hummocked, moutonnée, honeycombed, (d) close versus open, (e) thin, medium, thick, heavy, or huge, (f) shelf-ice.
  III. Other forms, differing from common types: (1) Sikozak, sikussak, sikkosak; (2) paleocrystic ice.
(C) Different phenomena and forms characteristic of the decline and disappearance of sea-ice--
  (1) Shore-clearing, shore-water; (2) connected shore-water; (3) snow-water, pool (snow-swamps?); (4) holes; (5) glimmer-ice, the second ice; (6) snow rind, crust of snow.

(D) Phenomena connected with ice and its environment--
  I. Non-navigable openings: (1) Ravines; (2) cracks (tidal versus offshore); (3) fissures.
  II. Navigable open waters: (1) Wake, clearing; (2) lead or lane, including (a) open leads, (b) shore-leads between floating ice and shore, between drift and fast ice, and between fast ice and shore; (3) polynya, stromhols (temporary versus persistent).
  III. Uncommon and miscellaneous: (1) Bergybit (with or without rams, or pads); (2) broken belt, broken ice; (3) ice-edge; (4) limits of ice, ice-limit, tongues; (5) bays (in the ice-edge or ice-limit); (6) ice-blink; (7) water-sky; (8) fog-smoke; (9) frost-smoke; (10) rossol, salt-snow; (11) drift-ice, the drift (loose versus compact, wind versus current-drift).
(E) Classification of open and closed ice for navigation-purposes--
  (1) Brash-ice; (2) scarce-ice, rarely becoming ice; (3) condensed ice; (4) uninterrupted ice; (5) compact ice; (6) rafted ice; (7) young ice; (8) old ice, ice many years old, ancient ice.

The Russian, German, and French equivalents are given for most of these terms.

Dr. Zukriegel has obviously gone to great labor to read the literature and compile a very convenient, if eclectic, account. The experts in the field will undoubtedly find much that is debatable and there are probably the inevitable omissions. The work as a whole suffers from the author's difficulty in writing in a language foreign to him (English) as there are many mistakes of form and grammar and awkward expressions, though the format is very fine. Also, one misses a discussion of the physics of ice in relation to its forms--the descriptive approach would seem to be inadequate in itself for the best treatment of such a subject. The student will, even so, find here a very handy entrée to a vast otherwise inaccessibly dispersed subject-matter, and thus deeply thank the author for lightening the road to much-needed ice-research.

Current publications

The list of current publications has been assembled by Carl Elges and Robert G. Stone with assistance of others who have generously provided articles and reports. A broad view has been taken of the subject-matter and a fairly complete cross-section of literature in all countries has been sought in order to maintain broad contacts. The entire field of snow and ice has been included together with related precipitation and storms to afford a complete picture of their evolution. Their human relationships also have been stressed. Thus the Committee seeks to perform the functions that have developed with its growth.

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Ministerio de Lavori Pubblici, Sulla ricerca dell precipitatzioni nell'alta montagna e sul funzionamento dei pluvionivometri totalizzatori nell'alto bacino del Po, Ufficio Idrografico del Po, Roma, 1936.

SNOW--Concluded

(2) Precipitation and evaporation--Concluded

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F. Steinhauser, Ueber den Schneeanteil am Gesamtniederschlag im Hochgebirge der Ostalpen, Beitr. Geophysik, Bd. 46, pp. 405-412, 1936; Forstliche Rundschau Bd. 9, p. 162, 1936.

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M. Visentini, Sul rendimento in acqua delle precipitazioni nevose, Ann. Lav. Pubbl., March 1937.

A tribute

Tribute must be paid to W. A. Lamb, member of this Committee and District Engineer, United States Geological Survey, Montana, for his epochal work in forecasting and distributing runoff on international streams. The cooperation amounting to comradeship between the representatives of the United States and Canada on St. Mary and Milk rivers is a token of what internationalism can become.

Mr. Lamb was just beginning the task of developing the Regional Committee of the Section of Hydrology for the North Continental Divide Area, and was Chairman of Cooperative Projects there. His death still seems unreal.

In this tribute should also be included Albert W. Preston, formerly Superintendent of Shops at the University of Nevada and later Assistant Professor in the Engineering Shops of the University of North Dakota, who aided in the formal development of the Mount Rose Snow Sampler.

Appreciation

Mention must again be made of the generous support of the Committee on Snow and the Section of Hydrology by the Nevada Agricultural Experiment Station and the United States Bureau of Agricultural Engineering, who have become steadfast patrons of research in snow and water. Without their assistance, much of the activity of the International Commission of Snow would likewise have been impossible.

Nevada Agricultural Experiment Station,
Reno, Nevada

REPORT OF COMMITTEE ON GLACIERS, 1937-38

François E. Matthes, Chairman

During the past year the Committee lost by resignation one member, R. H. Sargent, but gained another, Professor Wm. S. Cooper of the University of Minnesota, whose ecologic studies of the vegetation in Glacier Bay, southeastern Alaska, have shed new and valuable light on the past fluctuations of the vast glaciers of that unique area.

The Committee continues to be represented on the International Glacier Commission by the Chairman, who is titular member for the United States, and by Professor Wm. H. Hobbs, who is associate member. Both are also members of the International Commission of Snow, of which Professor J. E. Church is President.

Following is the Committee's report on the glaciers of the United States and Alaska for the 12-month period ending October, 1937.

Glaciers of Mount Baker

The Easton and Coleman glaciers on Mount Baker, noted for their unusually rapid recession during recent years, were again measured in 1937 by Herbert V. Strandberg and assisting members of the Mountaineers of Seattle, Washington. Both glaciers continue to recede but at a decidedly slower rate than previously.

Easton Glacier, situated on the south-southeast flank of the peak, receded 116 feet during the year ending September 26. This recession compares with 170 feet during the preceding year, and with 190 feet two years ago. The amount, moreover, falls considerably below the mean rate of 176 feet for the 28-year period ending 1936 (based upon the total recession from its position in 1908, which is shown on the topographic map of the United States Geological Survey). It is much less, also, than the mean rate of 160 feet for a period of 26 years, based upon a photograph taken in 1910.

Coleman Glacier, situated on the cooler northwest side of the peak, on October 3, 1937, had receded 52 feet, as compared with a mean rate of 86 feet for a period of 26 years, based on a photograph taken in 1910.

This appreciable slowing up of the recession of the two glaciers, moreover, is paralleled in a general way by the diminution in discharge of the Skagit River, which drains this portion of the Cascade Mountains. The gaging records at Diablo Dam show, for the year ending September 1934, a mean of 5,945 cubic feet per second (a high for the 26-year period); for the year ending September 1935 a mean of 4,653 cubic feet per second; and for the year ending September 1936 a mean of 3,450 cubic feet per second.

As Mr. Strandberg points out, the record of glacier-measurements on Mount Baker is as yet too short to afford a reliable basis for comparisons; nevertheless, the data obtained so far justify the belief that in the course of time the recession-record, taken in coordination with the data of precipitation and runoff that are available for the mountain district, will come to have real value as an index of the trend of climatic fluctuations.

Glaciers of Mount Rainier

Measurements made by C. F. Brockman, Park Naturalist of Mount Rainier National Park, seem to indicate a general slowing up of the recession of its glaciers in 1937. Five of the six glaciers measured had receded considerably less than the mean rate for several years past. The sixth glacier had receded an excessive amount, but this was due to local and temporary conditions not representative of the mountain area as a whole.

Nisqually Glacier, on the south side of Mount Rainier, showed a recession of 55 feet, as compared with a mean of 70 feet for the 19-year period since 1918. Whether this reduction has any significance as being indicative of a definite trend, remains to be seen, for 12 times during that period the glacier's recession has fallen below the mean, and five times it has been under 50 feet. The mean is brought up by several years of excessive recessions, when the amount ranged from 80 to 155 feet. Nor are the years of high and of low recession so distributed as to indicate any definite trend.

The complete table of recession for the Nisqually Glacier was published in last year's report and will not again be given here, but a recapitulation of the means for the successive periods, which presumably are indicative of climatic fluctuations, does seem appropriate.

From 1857 to 1885 the mean annual recession was 27 feet. From 1885 to 1892 it was 20 feet. From 1892 to 1918 it was 50 feet, and from 1918 to 1937 it has been 70 feet. The general trend, therefore, has been definitely upward since 1892, and has more than trebled during the last 45 years.

The total recession of the Nisqually Glacier since 1918, when annual measurements were begun, is now 1,338 feet. The total recession since 1857, when the glacier was first sighted, is 3,548 feet. The glacier is, however, still 4-1/2 miles long.

As was demonstrated by the two detailed topographic surveys of its lower half made in 1931 and 1936, and comparison of them with the small-scale topographic map of the Park made in 1910, this shortening of the Nisqually Glacier has been attended by a corresponding reduction in thickness. In 21 years, from 1910 to 1931, the surface of the glacier was lowered by melting between 80 and 120 feet over a stretch of approximately 4,000 feet; and from 1931 to 1936 it was lowered an additional 40 to 80 feet over a stretch of 3,500 feet.

As a result of this loss in thickness the lower half of the glacier now presents a very different appearance from the one it had in 1910. An island of rock protrudes through the middle of the glacier at the 5,500-foot contour-line, where formerly there was a tumultuous ice-cascade bristling with tall, leaning séracs. In 1931 this rock-island was 500 feet broad and had a sloping face about 180 feet high; by 1936 it had developed to a width of 650 feet and a height of 280 feet.

About 1,800 feet lower down on the glacier, at the 5,200-foot contour-line, another rock-mass has become uncovered. In 1931 this had the form of an island 700 feet long and 200 feet broad, situated not far from the eastern margin of the glacier, but by 1936 it had become a rocky platform with sloping front, projecting from the eastern wall of the canyon. Opposite it the glacier has lost nearly 700 feet in width and is now only 1,100 feet wide.

This marked reduction in thickness and in volume, however, appears to be strictly limited to the lower half of the Nisqually Glacier. From the 5,700-foot contour-line (at the head of the first island mentioned) upward the surface of the glacier is no lower than it was in 1931 and in 1910, but, if anything, somewhat higher.

The detailed surveys of the lower Nisqually, it should be repeated here, were made by the Water Resources Branch of the United States Geological Survey in cooperation with the Department of Public Utilities of the City of Tacoma, the National Park Service, and the Bureau of Public Roads. The plane-table maps were made on the scale of 1:4800, with 20-foot contour-intervals. The plan is to repeat these surveys every fifth year for the purpose of determining the loss in volume sustained by the glacier. The earlier topographic map of Mount Rainier National Park was made by the Topographic Branch of the Geological Survey in 1910 and 1911, on the scale of 1:62,500, with 100-foot contour-intervals.

Emmons Glacier, the largest glacier, situated on the northeast side of Mount Rainier, had receded only 10 feet in 1937. As usual, Mr. Brockman made his measurements to the ice at the point of effluence of the stream, adding, however, a supplementary measurement to the approximate point of effluence of the year before. The latter measurement indicated a recession of 16 feet. The comparable figures in 1936 were 193 feet and 100 feet; and the means of the two sets of measurements for the period 1930 to 1937 were 66 feet and 55 feet, respectively.

While the recession of the Emmons Glacier is never capable of accurate determination because of the vast accumulations of morainal material that mantle its terminus, there can be no doubt that the recession in 1937 was far less than in previous years.

Carbon Glacier, situated on the north-northwest side of the peak, ensconced in a steep-walled, shady canyon, was found to have receded 20 feet, as compared with 62 feet in 1936, and as compared with a mean annual recession of 29 feet for the period 1932 to 1937.

South Tahoma Glacier, situated on the southwest side of the peak and confluent with the southern lobe of the Tahoma Glacier, in 1937 had receded only 13 feet at point B, as compared with 37 feet in 1936. Point A, which receded 66 feet in 1937, was found completely obliterated. At point B the mean annual recession since 1934 is 31 feet; at point A the mean annual recession since 1931 is 50 feet.

Paradise and Stevens glaciers, heretofore listed separately, are really but two lobes of one and the same ice-mass. The Paradise Lobe has now receded so far that it is little more than the southern flank of the parent mass, while the Stevens Lobe remains prominent and now may be regarded as the real terminus. As a result of these changes the famous ice-cave under the Paradise Lobe, which was formerly visited by many tourists, has ceased to exist, and the Paradise River, and the Sluiskin Falls by which that stream descends into Paradise Valley, have suffered appreciable losses in volume.

Recession of the Paradise Lobe has usually been measured at three points, A, B, and C. Of these only B was identifiable in 1937, the other two remaining covered with snow. The recession at point B amounted to 28 feet, as compared with 60 feet in 1936, and with a mean of 30 feet for a 5-year period. The total recession at point B since 1932, when measurements were begun, is 124 feet.

The Stevens Lobe is wasting away at a much more rapid rate than the Paradise Lobe, chiefly owing to its narrow, tapering form, which has a large melting surface per unit of volume. The recession, measured in 1937, amounted to not less than 187 feet, as compared with 146 feet in 1936. As Mr. Brockman points out in his report, the rate of recession doubtless will come down to a more nearly normal figure when the narrow ice-tongue has melted away.

The Paradise and Stevens lobes are of peculiar interest because the ice-mass of which they form part is merely a small secondary glacier lying on a gently sloping platform between the deep canyons of two primary glaciers--the Nisqually and the Cowlitz. It is only 1-3/4 miles long, heads in névé fields at altitudes near 8,000 feet, fully 2,000 feet below the general level at which the main cirque glaciers originate, and has a southerly exposure. It is nearly stagnant, and as a consequence the losses by melting at its lower margin are not compensated by forward movement of the ice to the same degree as in the more mobile primary glaciers.

Glaciers of Mount Hood

The Research Committee of the Mazamas, of Portland, Oregon, last year, as usual, remeasured the more important glaciers on Mount Hood, and in addition extended its studies to some of the lesser ice-streams, principally to obtain comparisons with photographs taken in earlier years.

Eliot Glacier, the longest glacier on the peak, situated on the northeast side, was found to have receded 20 feet from September 20, 1936, to September 11, 1937. During a comparable period a year ago it receded 21 feet.

Comparison of a photograph of the terminus taken on September 11, 1937, with one published in 1912, in J. H. Williams' book, "The guardians of the Columbia," shows that during the past 25 years the glacier has receded several hundred feet and has lost, by estimation, fully 100 feet in thickness. Cryocinemeter-measurements at the terminus indicated a forward movement of the ice averaging 0.66 inch in 24 hours.

Coe Glacier, on the north side of the peak, had receded 29 feet during the 23-month period from October 20, 1935, to September 11, 1937. Cryocinemeter-measurements at the terminus indicated a forward movement of the ice averaging 1.09 inches in 24 hours. The rate of movement actually varies widely during different parts of the day, presumably in response to changes in temperature.

Newton Clark Glacier, on the east side of the peak, was rephotographed by A. J. Gilardi on September 12, 1937, from the identical point from which Dr. Harry Fielding Reid photographed it on July 25, 1901. Comparison of the photographs reveals an amazing reduction in both extent and thickness. The recession, which is difficult to estimate because of the irregular shape of the ice-body, amounts to about 500 feet, and the decrease in thickness averages fully 100 feet.

Palmer Glacier, situated on the south slope of Mount Hood, just west of the canyon of the White River Glacier, is the remnant of an ice-tongue that was formerly fed by overflow from that glacier. A recent experiment shows that it is now reduced to a stagnant ice-field. On October 11, 1936, a wire was stretched across one of the lower tongues of the ice-body, 75 feet in breadth. A trench was cut along the wire, filled with orange pigment and again filled with snow. On October 9, 1937, the line was reexamined and found still identifiable by means of the pigment. It showed no movement although the surface had been lowered several feet by ablation.

Langille Glacier is the name proposed for the ice-body that lies just west of Langille Crags, on the north slope of the peak. When mapped in 1907 it was a compact mass extending from an altitude of 7,500 feet down to an altitude of 6,500 feet. It is now greatly reduced in area and has become severed into two thin lobes each of which ends in a lakelet held by a terminal moraine.

Observations on these relatively insignificant ice-bodies may at first seem to have little value; nevertheless, it is hoped that they will be repeated from time to time, for these small residual ice-masses are on the verge of extinction, and unless there is a change in the trend of climatic conditions that will insure their early rehabilitation they will probably vanish entirely within the next ten or twenty years.

Aerial photographic surveys of the glaciers of the Cascade Mountains

On September 25, 1935, Kenneth N. Phillips, Chairman of the Research Committee of the Mazamas, accompanied by Adrian J. Gilardi, member of that Committee, and L. J. Baily, photographer, encircled Mount Hood in an airplane and secured a set of excellent oblique photographs of its entire glacier system.

So valuable did these aerial photographs turn out to be as a means of recording the extent, form, and characteristics of each glacier, and such accessory features as moraines, proglacial lakelets, the course of effluent streams, and the height of the timber-line near by, that it was decided to make a more extended flight the following year around some of the other glacier-bearing peaks of the Cascade Mountains. Accordingly, on October 6, 1936, a flight was made encircling Mount Adams, Mount Rainier, and Mount St. Helens, in the State of Washington. A set of photographs was taken, portraying the numerous glaciers on each of these three peaks, and again much valuable information was secured that could not possibly have been obtained by ground observations.

On September 24, 1937, a third flight was made embracing the glacier-bearing peaks of north-central Oregon--Mount Jefferson, Three-fingered Jack, Mount Washington, and the Three Sisters--and as a result there is now at hand, for the first time, a collection of aerial photographs depicting all of their relatively little-known glaciers.

These photographs have been mounted in two annotated albums that constitute, in effect, geographic reports in pictorial form. The first of these albums contains the views of Mount Adams, Mount Rainier, and Mount St. Helens; the second contains the views of Mount Hood, Mount Jefferson, Three-fingered Jack, Mount Washington, and the Three Sisters.

A copy of each of these albums has been generously presented by the Mazamas to the Committee on Glaciers. The Committee takes this opportunity publicly to express its high appreciation, and to congratulate the Research Committee of the Mazamas on the success of its aerial reconnaissances. Whatever time, effort, and expense have gone into them have been well spent, for these collections of aerial photographs will form a much-needed groundwork for future glaciologic research. It is to be hoped that similar collections will be made in the course of time, covering all of the more important glaciers of the western United States.

Glaciers of the Sierra Nevada

C. A. Harwell, Park Naturalist of Yosemite National Park, California, and his assistants last autumn revisited the five glaciers on the crest of the range to which measurements are annually made. Three of these--the East Lyell, West Lyell, and Maclure--are within the Park, the other two--the Dana and Conness--are just outside its eastern border. The results were as follows:

East Lyell Glacier on September 24, 1937, was found to have receded only six feet from its position in 1936.

West Lyell Glacier had receded 14 feet and had lost at least six feet in thickness at its lower end. Mr. Harwell's handsomely illustrated report contains an unusually instructive photograph showing clearly the system of concentrically outcropping ice-layers in the surface of this glacier, which was completely divested of recent snow, save at its extreme head. Superimposed unconformally on the broad main mass lies a long, narrow ice-tongue composed of a more recent series of layers. On the upper half of the glacier, again, the central tongue disappears beneath another, shorter tongue composed of a third unconformable and still more recent series of layers.

It is here suggested that each of the two superimposed series represents an accumulation made during a relatively recent period, or cycle, of glacier-growth, and that each of the two marked unconformities indicates strong ablation during a prolonged period, or cycle, of glacier-wastage. The youngest series of beds is perhaps to be correlated with the cycle of wet years that is definitely recorded in the varying levels of Mono Lake, at the eastern base of the Sierra Nevada, and that culminated during the years 1919 to 1922 [see S. T. Harding, Changes in lake levels in Great Basin Area, Civ. Eng., v. 5, No. 2, February, 1935, pp. 89-90].

Maclure Glacier, like West Lyell Glacier, was found stripped of its snow-cover except in the vicinity of the bergschrund. It had receded 13 feet since the autumn of 1936.

Dana Glacier, by contrast, was still partly covered with snow. Large masses of snow blanketed its lower margin, and as a consequence recession-measurements could be made only from two of the four stations that are regularly used. At station 3-A the ice had receded 10 feet, and at station 5-A it had receded 33 feet.

Conness Glacier, again, was largely stripped of its snow-cover, and appeared to have sustained greater losses from ablation during the past year than any of the other glaciers of the central Sierra Nevada. Its eastern lobe had receded no less than 90 feet and had become reduced to a relatively thin sheet.

Glaciers of Glacier National Park, Montana

Dr. George C. Ruhle, Park Naturalist of Glacier National Park, reports the following figures for the recession of three of the more accessible glaciers:

Siksikaikwan Glacier (formerly west lobe of Blackfoot Glacier) on September 20, 1937, had receded 28 feet from the position it occupied in September, 1936. Its total recession for the last two years is 132 feet, but the total for the last five years is only 70 feet, there having been advances in 1933 and 1935.

Sperry Glacier had receded 29 feet during the year ending September 17, 1937. It is gradually breaking up into several small ice-bodies and now falls far short of covering the broad shelf which it mantled almost completely in 1901, when the topographic map of the Chief Mountain Quadrangle was made by the United States Geological Survey.

Grinnell Glacier, visited on October 25, 1937, was found to have held its own during the past 12 months, having receded only four feet.

During the summer of 1937 a detailed topographic map was made of the Grinnell Glacier and its surrounding moraines on a scale of 1:2,400, and with 20-foot contour-intervals. This work marks the beginning of a mapping program which, it is hoped, will ultimately be extended to all of the larger glaciers of the Park, and will afford a splendid groundwork for future glaciologic studies.

Glaciers of Rocky Mountain National Park, Colorado

To H. R. Gregg, Park Naturalist of Rocky Mountain National Park, we are indebted for resumption, in 1937, of recession-measurements to the Andrews and Tyndall glaciers. Measurement of these glaciers was begun in 1932 but had lapsed in 1936 owing to changes in the personnel of the Park.

Both glaciers are small, measuring only half a mile or less in length, but they are representative of the type that prevails in the central portion of the Rocky Mountains, and annual observations on them are, therefore, of special importance.

Andrews Glacier on October 13, 1937, was found to have melted back 31 feet since it was visited in 1935. Its complete record is as follows: 1932-1933, -10 feet; 1933-1934, -80 feet; 1934-1935, +73 feet; 1935-1937, -31 feet; net change since 1932, -48 feet.

Mr. Gregg's report is illustrated with photographs showing successive stages in the recession of the small cirque glacier from September 1928, when it fronted broadly on the lakelet at its foot, to September 28, 1937, when it was separated from the lakelet by a delta 35 feet long.

Tyndall Glacier appeared to have receded no less than 163 feet in the two-year interval since 1935. Its complete record is as follows: 1932-1933, -33 feet; 1933-1934, -9 feet; 1934-1935, +34 feet; 1935-1937, -163 feet; net change since 1932, -171 feet.

Particularly impressive is the evidence of great reduction in thickness and in volume of the Tyndall Glacier from September, 1928, to the present time, that is afforded by the photographs included in the report.

Glaciers of Alaska

From Harry J. Liek, Superintendent of Mount McKinley National Park, word was received that he has at last succeeded in making initial measurements to the fronts of three ice streams in the Park--the East Fork Glacier, the Toklat Glacier, and the Sunset Glacier.

Bayne Beauchamp of Honolulu, Territory of Hawaii, who visited Valdez, Alaska, and several of the glaciers in its vicinity in 1936, has kindly transmitted to the Committee copies of two old records of the recession of Valdez Glacier that are in the possession of the Order of Moose at Valdez. One of these goes back to 1898, when the great "gold rush" to this part of Alaska took place. Though these two records do not agree throughout, they indicate the same total recession of 918 feet. Both are given here as transmitted:

Paced distances from a stone monument on a mound near the front of the glacier (location of monument not positively identified by Beauchamp)

Date	Steps	Feet	Recession in feet
1898-99	?	±150	
1901	70	210	60
1902	83	249	39
1904	138	414	165
1905	171	513	99
1908	70 steps + 583 feet	796	283
1909	?	840	44
1914	?	918	78

Dates	Recession in feet	Total recession since 1901
June 6, 1901 - June 18, 1902	39	39
June 18, 1902 - June 7, 1904	165	204
June 7, 1904 - June 16, 1905	99	303
June 16, 1905 - Oct. 13, 1908	283	586
Oct. 13, 1908 - June 21, 1909	44	630
June 21, 1909 - Aug. 10, 1914	288	918

Mean annual recession for the 13-year period is 70.6 feet. The second set of measurements is presumably the more reliable of the two. It was made by Dr. L. S. Camicia and Joseph A. Bourke (both deceased). Transmitted with these records is a sketch map of the front of Valdez Glacier, showing distances measured to its front by Beauchamp's party, and location of reference-points.

In addition, there were received from Beauchamp notes on three glaciers situated near the Richardson Highway leading from Valdez to Fairbanks. These are the Worthington Glacier, situated 29 miles from Valdez; the Cache Creek Glacier, situated 50 miles north of Valdez; and the Gakona Glacier, situated about midway between Valdez and Fairbanks. Accompanying the notes are photographs of the glaciers and sketch maps showing the location of rock-cairns built for future reference-points.

Black Rapids Glacier, whose phenomenal advance threatening the Richardson Highway, during the winter of 1936-37, gave it front-page notoriety as the "runaway glacier," has now lapsed back to the sluggish, almost imperceptible movement characteristic of most glaciers.

The most complete report on its phenomenal advance, and the probable causes thereof, has been presented by Professor James H. Hance of the University of Alaska in a paper entitled "The recent advance of Black Rapids Glacier, Alaska" (J. Geol., v. 45, No. 7, Oct.-Nov. 1937, pp. 775-783). A brief note by Fred. H. Moffit of the United States Geological Survey entitled "Runaway glacier caused by friction and skidding" was published in Science News Letter for January 15, 1938.

The advance was estimated at about three miles. The precise date of its beginning is unknown, but if it is assumed that it began late in September or early in October, 1936, the average forward movement of the ice-front per 24 hours would probably have been 115 feet. Between February and June, 1937, advances as rapid as 250 feet per day were reported by local observers, but Professor Hance believes that "crumbling and flattening of the steep front may account for some of these phenomenal figures."

Professor Hance is inclined to attribute the rapid advance primarily to excessive snow-precipitation in the mountains surrounding the broad gathering basin at the head of the glacier, during 1929 to 1932, which was followed by another period of somewhat more than normal precipitation during 1929 to 1936. (The only precipitation-data available are those from the Weather Bureau station at Fairbanks, about 140 miles to the northeast; these are probably not representative of the conditions that obtained in the mountain area.) The rapidity of the forward movement is attributed to the narrowness of the outflow canyon, as compared with the breadth of the basin drained. Terminal moraines situated west of the Delta River show that the glacier has made somewhat similar advances in fairly recent times.

Moffit does not regard excessive snowfall as the primary cause of the rapid advance of the Black Rapids Glacier. If excessive snowfall had been the cause, he argues, then every glacier in the same mountain area would have "run away." The fact that the Black Rapids Glacier was the only one that behaved so extraordinarily, he insists, points to a local cause. That cause is very probably to be found in the extreme narrowness of the outflow canyon as compared with the breadth of the capacious gathering basin above. The great frictional resistance of the canyon-walls must have caused the ice to accumulate in the basin for many years until at last there was sufficient pressure or "head" to force outflow. Probably the resistance was overcome rather suddenly, and the outflow therefore took place with unusual velocity.

It may be presumed that similar sudden and rapid forward surges have occurred periodically throughout the life of the glacier, and this presumption seems to be borne out by two concentric moraines situated about one mile and one-half mile from the present front of the glacier.

U. S. Geological Survey,
Washington, D. C.

REPORT OF COMMITTEE ON EVAPORATION FROM WATER-SURFACES, MARCH 1938

S. T. Harding, Chairman

The membership of the Committee has remained unchanged from the previous year. It is as follows:

Harry F. Blaney, 447 Pacific Electric Building, Los Angeles, California
N. W. Cummings, San Bernardino Valley Junior College, San Bernardino, California
Robert Follansbee, 403 New Post Office, Denver, Colorado
J. A. Folse, Museum of Science and Industry, 56th Street and Lake Michigan, Chicago, Illinois
Robert E. Horton, Voorheesville, New York
Ivan E. Houk, U.S. Bureau of Reclamation, Customhouse, Denver, Colorado
R. E. Kennedy, 1577 Jasmine Street, Denver, Colorado
G. F. McEwen, Scripps Institution of Oceanography, La Jolla, California
Carl Rohwer, Colorado State College, Fort Collins, Colorado
C. M. Saville, 53 North Beacon Street, Hartford, Connecticut
Thorndike Saville, New York University, University Heights, New York
S. T. Harding, 12 Engineering Building, Berkeley, California, Chairman

The Committee on Evaporation from Water-Surfaces has commented in its previous reports on the need for more complete investigations of the factors affecting evaporation and the equipment used for its measurement. Owing to the wide distribution of its membership the Committee has not been able to hold meetings as a whole. Fortunately in June 1937, six members were in Denver at the meeting of the Section of Hydrology and were able to hold a partial meeting. It was agreed by those present that the program of the Committee for 1937 should consist of an attempt to prepare a definite program for an adequate study of evaporation from water-surfaces. It is hoped that the presentation of such a program would assist in calling attention to the need for such work and in finding some means for undertaking it. The results of the Committee's work on this subject are attached and constitute the main results of its activity during 1937.

In 1936 the main work of the Committee was directed toward the preparation of a chapter on evaporation from water-surfaces for the volume on Hydrology being prepared under the direction of the Section of Hydrology. A draft of this chapter was completed in May 1937, under the authorship of the Chairman, with the assistance of the members of the Committee.

The Committee has not, as yet, given definite consideration to its program for 1938. It is hopeful that means may be found for initiating a program of investigation similar to that it is proposing. The Committee will be glad to assist in any available way in such a program.

A draft of the proposed program was sent to all members of the Committee on October 1, 1937. Replies approving or making suggestions were received from eight of the eleven members. The suggestions were incorporated in a revised draft which was sent to the members on February 10, 1938. Specific approval of this draft has been received from four members of the Committee. No direct objection to the report, of which the revised draft is given in full below, has been received from any member and, as far as the Chairman is advised, it has the approval of the members.

Proposed program of investigations on evaporation from water-surfaces

The following program of investigation is presented by the Committee to meet the inadequacy of present knowledge of the factors affecting evaporation. Evaporation from water-surfaces is an important item in many water-supply projects. It is generally recognized that there are deficiencies in both the methods used to measure evaporation and the extent and distribution of observations of its amount. This report deals with the investigations considered to be essential to the development of adequate methods of observation, and the application of their results to large water-surfaces. When such methods have been developed, the character and extent of the routine observations to determine local records of the amount of evaporation can be much more readily outlined.

There are few places in which evaporation from large water-areas can be measured directly. Such measurements require conditions where all other elements of supply and draft can be determined closely. The scarcity of direct records on large water-areas has required the use of indirect methods. These have been of two types. One may be termed the sampling method wherein the measured loss from small surfaces is applied to the large area. In earlier work direct application of such results without adjustment was used; later observations indicated the need for coefficients to correct for the differences between sample areas and large surfaces. The second method which has come into use more recently consists in measuring the factors affecting evaporation and computing their effect in terms of loss from a water-area.

Neither of these methods has been fully established for the complete range and details of their applications. There has been much observational work undertaken but no series of experiments has been sufficiently comprehensive to be conclusive.

As a result of work to date the following items can be listed as unsettled or involving uncertainties in their applications.

Evaporation pans--non-insulated types--(1) Is there a single factor by which the annual results from any given form of pan can be reduced to the loss from large water-areas or does such factor vary with local climatic or physiographic factors?

(2) If there is a uniform coefficient for annual records for any given pan, is such coefficient uniform for shorter periods, such as months, days, or hours?

(3) If coefficients for pans are not uniform, can an expression for such coefficients be derived which utilizes only some of the more readily observed climatic items, such as temperature of air and water, humidity, and wind-movement?

(4) If the relationship of the evaporation from a pan and a large water-area cannot be expressed by a single coefficient, what items are required to be observed to express such relationship and what is the form of expression required to enable pan-results to be applied to large areas?

(5) Can a form of pan be found which has a simple relationship to a large water-area or is the relationship so involved that close results require detailed observations of all elements affecting evaporation?

(6) What form of pan most nearly meets the requirements of a simple and direct relationship to a large water-area?

Direct measurements of items affecting evaporation--These methods include measurements of solar insolation less back-radiation to the sky by means of an insulated pan with temperature-observations of air and water at the pan and in the adjacent large water-area.

(1) Does evaporation from an insulated pan have a simple or direct relation to solar insolation less back-radiation?

(2) Can such a pan be used without adjustment of its results and, if not, what records are needed in order to make such adjustments?

(3) Can methods of direct measurement of back-radiation be found?

(4) Can this method be used without an extent and character of observation that is more technical than can be expected from usual observers?

The inadequacy of present results is not due to lack of thoroughness in the work that has been done in so far as its program has extended. Extensive work has not been undertaken where the results of experimental observations could be directly checked against large water-areas. Some work has had such partial checks. Sleight at Denver compared his pan-results with a lake one-half mile away. Rohwer at Fort Collins used an 85-foot diameter circular reservoir.

Further work based on comparing different types of observational equipment against each other cannot be conclusive as none of the equipment has its relationship to large areas fully determined. The first essential of any adequate program is a large water-area for which the evaporation can be measured directly. Such areas are difficult to find but do exist. Some western lakes have practically no seepage and no invisible inflow. Visible inflow and outflow can be measured. During much of the year there may be practically no inflow and the fluctuations of the lake may be due to evaporation alone. Such lakes should have a limited range of fluctuation to avoid bank-storage problems. The lake should have an area large enough to be representative of exposed water-surfaces without being so large that conditions in its different parts may vary too widely. An area of 3,000 to 10,000 acres should meet both of these standards.

Around such a lake two types of work could be undertaken. One would be experiments with all suggested types of equipment located at some favorable point adjacent to the lake. The other would be less extensive work with selected types of equipment at different points around the lake.

To be conclusive such work should be continued for from three to five years. Sufficient results might be secured in three years but a much better checking of the effect of the variation in climatic factors would be secured from five years' work. It is not considered advisable to equip such a program unless it can be maintained for a minimum of three years.

Such work should be under the direction of some Federal bureau, active in this field, with the cooperation of State and other local agencies. There are different Federal bureaus available. If such work is assigned to any Federal agency, some coordinating group, including representatives of the other Federal agencies active in this field, should be established.

Such a program should include all present types of pans and any suggested new ones that give promise of usefulness. Present pans include:

Land pans--

Buried pans: Square and circular of different sizes and depths.
Surface-pans: United States Weather Bureau Class A and others of different size, depth, and exposure.
Floating pans: Typical sizes and depths with different methods of baffling.
Insulated pans: Different types of insulation and different sizes.
Finish of pans: Different finish and paint to determine their effect.
Quality of water: Further work on effect of salt content on evaporation.

Temperature-records--

At least one recording thermometer, maximum and minimum thermometers for air- and water-readings, single reading thermometers for direct readings.
Records of air-temperatures adjacent to the pans and over the large water-area.
Records of the temperature of the water in the pans and in the lake including temperature-profiles of the lake and deeper pans (for the lake-temperature the Spilhaus thermograph could be used).

Humidity-records--

At least one recording psychrometer and sufficient wet and dry bulb thermometers for observations on the pans and over the lake.

Wind-records--

Anemometers over the pans and at different elevations above the ground with anemometers at selected locations around the lake.
Instruments for distinguishing between laminar and turbulent air-movement.

Rain-gages--

Recording rain-gage at site of principal work and 8-inch standard rain-gages at additional points around the lake.

Solar radiation--

One standard thermo-electric pyrheliometer together with other available types, such as Burt photo tube.

Barometers--

Evaporimeters--

One or more sensitive instruments for continuous measurement of evaporation or for short periods by direct reading.

Atmometers--

While not generally regarded as representative of water-surface evaporation, atmometer-records to be obtained in an effort to define their limits of usefulness, including the Piche and the Livingston black or white spherical porous cups.

Instrument-shelters as needed--

Ground-water observations if there is inflow or outflow from the lake.

In connection with such a program for field-investigations, research under controlled conditions could also be effectively undertaken. This would include efforts to segregate the effects of single factors such as temperature, wind-movement, and humidity. Wind-tunnels, constant humidity-rooms, etc., with their necessary instruments would be required.

The selection of the location for such investigations should be very carefully made. It is desirable that the mean temperature be fairly high in order that the amount of evaporation may be relatively large. There should be sufficient variation during the year in temperature, wind-movement, and humidity to bring out the effect of these factors. Some freezing temperature is desirable if obtainable without sacrificing high summer heat. Losses from ice and snow could well be made a separate study in an area suited to such work.

The principal factor in the selection of such a site will be the availability of a large water-area for which evaporation can be determined. Among western lakes that should be considered, Lake Elsinore in southern California and Walker Lake in Nevada meet the requirements listed. There are now 25 years' records of inflow and fluctuation available on Lake Elsinore that show a consistent annual evaporation. Its area varies from 2,500 to 5,000 acres and depth from 12 to 35 feet. It now contains sufficient water to insure continuation of its area for many years. Walker Lake was suggested as an ideal site for such work by Russell in 1882. It has a limited and measurable inflow. Its present area is about 50,000 acres and its depth about 200 feet. Ground-water inflow or outflow is negligible at both of these sites. Other suitable lakes are also probably available if a search should be made.

The estimated cost of this program is as follows:

Cost of installation for field-work.......	$ 3,000
Cost of operation per year for five years:	
Engineer in charge, $3,600 per year....	18,000
Supplies and miscellaneous expenses, $1,000 per year.........................	5,000
Assistance, $900 per year..............	4,500
Publication of results....................	2,500
Total	$33,000

If, in addition, laboratory-type of work was also undertaken, these estimates should be increased by $1,500 for equipment and by $2,000 per year for assistance.

The work outlined will be used mainly by engineers in connection with water-supply problems and an engineering point of view should be retained throughout the investigation. However, much of the observational work and the analysis of the results are within the field of the physicist, and training in physics is essential for the proper conduct of the work. Personnel should be selected with these two points of view in mind. Some advisory group representing both points of view should maintain close contact with the work. The Lake Elsinore site is particularly well suited to meet this, due to its closeness to research-institutions that have been active in this subject and the number of engineers who have been actively engaged in local water-supply projects in which evaporation is an important element. While results at any selected location can only be representative of its location conditions and further work will be required before such results can be generally applied, such a program should enable the observational methods best adapted for securing results in other areas to be defined and simplify the future work needed at other locations.

12 Engineering Building,
Berkeley, California

REPORT OF THE COMMITTEE ON RUNOFF, 1937-38

W. G. Hoyt, Chairman

The personnel of the Committee has remained unchanged as follows throughout the year:

H. K. Barrows
Merrill Bernard
E. S. Cullings
R. W. Davenport
R. S. Goodridge
R. E. Horton
I. E. Houk
W. G. Hoyt
Joseph Jacobs
F. T. Mavis
A. F. Meyer
C. R. Pettis
F. F. Snyder
H. C. Troxell
C. O. Wisler

Members of the Committee have been active throughout the year in matters relating to the work of the Section of Hydrology and all, through personal study and research, have advanced our knowledge of the phenomena of runoff and stream-flow. During the year various members have reviewed many technical papers and articles dealing with the hydrology of runoff and have made appropriate recommendations to the program committee regarding publication. Members have also assisted in the compilation of references for the Bibliography of Hydrology for 1935, 1936, and 1937. Credit for this compilation rests largely with Mr. Beij.

Members of the Committee were active in the formation of the project relating to flood-waves and five of the members are represented on the special flood-wave committee. One or more members are also represented on each of the other research-committees of the Section.

As individual member, L. K. Sherman, ex officio, prepared and issued a paper in mimeograph during the year entitled "Hydrographs of stream-flow" and has prepared a paper on "Determination of infiltration-rates" for presentation at this meeting.

H. K. Barrows and Joseph Jacobs were members of the special Committee on Hydrologic Data of the National Water Resources Committee.

Merrill Bernard presented papers relating to flood-forecasting activities of the Weather Bureau at both the Denver and Davis sectional meetings.

An article by R. W. Davenport on the Ohio-Mississippi Flood of 1937 was published in Nature, London, October 16, 1937.

R. E. Horton delivered a paper on "Hydrologic aspects of the problem of stabilizing streamflow" at the Denver sectional meeting; he took an active part in the discussions at the Davis meeting and in addition has prepared papers for the Flood-Wave Committee on "Definitions and classifications of flood-waves," "Rain wave-trains," and "The Seddon and Forcheimer equation for crest-velocity of river flood-waves." Dr. Horton also prepared a preliminary outline for a comprehensive research-program on runoff phenomena which will be the subject of a round-table discussion by the Runoff Committee and others on April 30, 1938. He will present also at the meeting a report of the Advisory Committee on Soil Conservation Research.

R. S. Goodridge presented a paper at the Denver meeting on "Some interrelationships of rainfall- and infiltration-intensities."

Professor Mavis has continued the analysis of hydrologic data for the Ralston Creek Area and progress is being made on the establishment of a study-area on Rapid Creek, an adjacent watershed.

C. R. Pettis presented in Civil Engineering for February 1938 an appraisal of the unit-graph method of flood-estimation in which are shown comparisons between crest-discharges as determined from the unit-hydrograph and as determined by the Pettis width-formula.

F. F. Snyder was co-author of "Flood-routing" published in Proceedings of the American Society of Civil Engineers for February 1938, in which are described flood-routing problems on the Tennessee River. He has also prepared a comprehensive paper on synthetic hydrographs for presentation at the 1938 meeting.

A study by H. C. Troxell of the meteorologic and hydrologic features and analysis of the mechanics of debris-movement during the New Year flood of January 1934 in southern California was published in Geological Survey Water-Supply Paper 796-c.

Professor Wisler has continued his research-problem relating to the development of a practical method for producing hydrographs of surface-runoff from rainfall-records based upon the Horton theory as expressed in Horton's publication No. 101. He has determined definitely that there is a clean-cut relationship between surface-detention and stream-flow, but calls attention to the well-known fly in the ointment in connection with the wide variation in infiltration-rates for different storms.

Your Chairman, associated with Walter Langbein and others, has studied the hydrologic and meteorologic conditions and made detailed rainfall- and runoff-analyses of the floods of March 1936 and of the Ohio-Mississippi Flood of January to February 1937. The results of the study of the March 1936 flood are published in Water-Supply Papers Nos. 798, 799, and 800, and results for the Ohio-Mississippi Flood are now being prepared for publication as Water-Supply Paper No. 838. Mr. Langbein will present, at this meeting, some of the problems relating to the determination of channel-storage and infiltration rising from those flood-studies.

All students of hydrology will agree that during the last several years there has been a decided upward trend in the curve of scientific knowledge of the phenomena of surface-runoff and stream-flow, resulting largely from the crystallization of the thoughts and concepts of the many who have spent their lifetime in studies of various phases of the subject. There is today almost complete unanimity of opinion as to how and why surface-runoff occurs and as to its behavior after it has reached stream-channels. Having established sound theories, our trend should be rapid in the field of quantitative determination and in the practical application of the theory to flood-control, flood-forecasting, and problems of land-use and management.

Details relating to additional research needed have been so fully covered in Dr. Horton's outline for a comprehensive research on runoff-phenomena, which will be the subject of a round-table discussion April 30, 1938, that the subject is not considered more fully in this report. It is sufficient to say that eventually the qualitative theories will be translated into quantitative values through the experimental studies as follows: The Soil Conservation Service on erosion-control practice, including observations of precipitation, runoff, silt-content of water, and underground water-storage; the Forest Service on the relations of vegetal cover to runoff based on observations of precipitation, water-losses, runoff, and underground water; the Bureau of Agricultural Engineering on both ground- and surface-water in the "Dust Bowl" and availability of surface and subsurface water for irrigation, snow-surveys for irrigation and water-power forecast, and the research-projects on flood-studies and rainfall-runoff relationships; ground-water and silt-suspension studies being carried on by the Geological Survey; the advanced studies for improved flood-forecasting by the Weather Bureau; and many other similar studies and experiments by States, municipalities, colleges, and individuals.

The Section of Hydrology, through the Runoff Committee, should be in a position to render constructive criticism, advice, and otherwise, to cooperate in the wide-spread and comprehensive hydrologic studies now in progress.

U. S. Geological Survey,
Washington, D. C.

REPORT OF THE COMMITTEE ON PHYSICS OF SOIL-MOISTURE, 1937-38

(Effect of organic matter on the infiltration of water into soils)

F. J. Veihmeyer, Chairman

The Committee has endeavored to review the present state of knowledge in this country relative to the effect of organic matter in the infiltration of water into soil. Rather than attempt to review the literature in the field, it was thought best to elicit replies from members of the Committee, and from a selected list of those the Committee thought may be interested in the subject.

In an effort to get information and, at the same time, break down the problem, the following questions were asked:

(1) Effect of incorporated organic matter on the rate of entry of water into soils
(2) Effect on rate of entry of water into soil due to the presence of organic matter on the soil-surface
(3) Effect of organic matter on the physical condition of the soil below it, in so far as it influences rate of entry of water into soils
(4) Effect of surface-litter on filtering of silty water and thereby influencing the rate of entry of water into soil
(5) Effect of the presence of organic matter in retarding runoff
(6) Effect of organic matter on the water-holding capacity of soils
(7) Change with time of the rate of entry of water into soil for measurements made while the rate was steady or unsteady

We have attempted to keep the wording substantially the same as it was submitted, but yet to condense the replies within the space available. The meaning, of course, has not been altered in any case. Numbers under each commentator's contribution are in answer to the items as numbered above.

J. T. AUTEN, Forest Service, United States Department of Agriculture--(2) Refers to some of his measurements [see 3 and 4 of "References" at end of report] from which he concludes that surface organic matter has a profound effect on rate of water-absorption. Surface organic matter protects and retains the porous upper-soil horizon. Without organic matter, impact of rainfall and surface-runoff quickly destroy porosity and remove the loose upper soil. Removal of porous soil increases percentage of runoff very greatly.

(3) Without surface organic matter, soil-porosity development and water absorption-rates are greatly diminished.

(4) There is some filtering effect, but the most important function is the prevention of water-silting by impact of rain on bare mineral soil.

(5) Refers to work [36] of Meginnis from which Auten concludes that the runoff was much greater on plots which were either barren or cultivated in rows than it was on plots covered with scrub-oak woodland, black locust, osage-orange plantation, broom-sedge, bermuda-grass, or oak-forest.

(7) Auten thinks that the rate of entry of water into soil changes with time, but it varies with the soil. In the absence of soil-puddling, he does not think there should be much change in rate until saturation is approached, when the rate of absorption will depend on removal of gravitational water.

L. D. BAVER, Ohio State University--(2,3,4) Baver states that the effect of surface organic matter on the rate of entry of water into the soil has many ramifications. There is the protective effect of this surface-cover against the impact of the raindrops. This minimizes dispersion of the soil. Consequently, clear water percolates into the soil. If the water becomes silty, the pores are clogged and the rate of entry is decreased. Wollny investigated this factor in 1874 and showed that the non-capillary porosity of the surface soil was decreased from one-third to one-half by the infiltration of turbid water. Lowdermilk has called attention to the same effects. Too much emphasis is being placed upon the filtering effects of organic matter on the surface. He doubts if this is the major effect. It seems that the prevention of dispersion is far more important.

Organic matter on the surface is responsible for much biotic activity which results in increased permeability arising from the greater number of large pores. This is especially true under forest-litter. He quotes Wollny who showed as early as 1890 that earthworms increase the non-capillary porosity of the soil, and thinks most soil-scientists have overlooked the hydrologic significance of these effects.

When organic matter is added to the soil via the growing plant, the root-effects definitely increase the rate of entry of water not only because of the root-channels but also due to the improvement of soil-structure which is accomplished by the organic matter. As a matter of fact, the formation of a stable soil-structure which will increase water-movement in soils can only rarely be accomplished by artificial additions of organic matter. The root-effect of growing organic matter is of paramount importance. Russian investigators have definitely shown that there is more difference between crops in their ability to improve soil-structure than there is between manure and no manure. Investigations on the genesis of soil-structure indicate that organic matter is the key to stable structure-formation and that vegetation (especially grasses) is the locksmith. He refers to his paper which was presented at the annual meeting of the American Society of Agronomy, 1937, under the title "The relation of vegetative cover to the physical properties of soils."

(5) Baver refers to investigations at the Missouri Agricultural Experiment Station [37] which show that the turning under of a clover-crop reduces runoff by about 2-1/2 times over no organic matter.

(6) He has no information on the effect of organic matter on the water-holding capacity of the soil as such. However, he has considerable data [7] on the plasticity of soils in relation to organic matter. In every case, the removal of organic matter caused a decrease in the limits of plasticity corresponding to decreases in moisture-holding capacity of from two to three parts of water per unit of organic matter.

H. F. BLANEY, Bureau of Agricultural Engineering, United States Department of Agriculture--(2) Blaney refers to the work of Mitchelson and Muckel [38] from which they concluded that, other factors being equal, the highest rates of percolation on their plots were obtained on land where the native vegetation and soil-covering were disturbed the least.

(7) He states that Mitchelson and Muckel [38] found that the rate of entry of water on plots from which the vegetation had been removed, decreased with time, and explained this effect as being due to resorting of the soil-particles and puddling of the soil-surface.

G. B. BODMAN, University of California--(6) Bodman reports an investigation on the effect of additions of organic matter to a Yolo fine sandy loam and a Yolo clay-loam upon the respective moisture-equivalents of these two soils. In order to obtain at least some degree of approximation to the kind of organic matter which might be expected to be present in cultivated soils, artificial manure was prepared by the addition of ammonium sulphate and calcium carbonate to unchopped wheat straw according to the general plan developed in the Adco process. Composting took place over a period of three months in galvanized tanks after sprinkling with water. At the end of this time, the compost was thoroughly leached with water to remove electrolytes. It was then dried and ground in a Wiley mill finer than one millimeter.

The moisture-equivalents were determined upon untreated samples of a Yolo fine sandy loam (M.E. = 18.72 per cent) and a Yolo clay-loam (M.E. = 31.09 per cent) and upon samples of the same soils to which the dried manure had been added in amounts equal to one, two, four, and eight per cent by weight. Determinations were made upon all of the samples by filling the centrifuge cups to a depth of one centimeter. The artificial manure (O.M.) alone had a moisture-equivalent of 138 per cent when determined in this fashion. The effect of the additions of organic matter was such that practically a linear relation was found to exist between moisture-equivalent per cent by weight and fractional composition by weight of the binary system, mineral-soil-artificial manure, although the slopes in the case of the two lots of mixtures were different. ΔM.E./ΔO.M. amounted to 1.52 for the Yolo clay-loam and 0.95 for the Yolo fine sandy loam. He sees no reason to suppose that the results are not typical of normal soils. They are exactly what might be expected from the work of Briggs and Shantz, but their practical significance with regard to soil-management recommendations concerning water-economy is greatly limited owing to: (1) The tremendous and uneconomical quantities of organic matter necessary; (2) the necessity and difficulty of deep and thorough incorporation of the organic matter in the soil; (3) the fact that only a fraction of the increased water-content is available to plants; and (4) the difficulty of maintaining a high organic-matter status under climatic conditions for which the natural soil-profile developed does not tend toward an organic-rich profile (for example, in California).

(7) Bodman reports having made a large number of rate-of-entry measurements with different homogeneous soil-columns in the laboratory, in which precautions were taken to avoid border effects. A few of them have been published [9]. In all cases, there was found to be a pronounced decrease in rate of entry with time where rate of entry is defined as the velocity of a water-stream past the surface-plane of the column . As a first approximation, it may be stated that the rate of entry-time curves were of the general form of hyperbolas with rectangular asymptotes. During all of these measurements the depth of water above the soil-surface was maintained constant, and it seems probable that for a given soil the difference in pressure-potential between soil-surface and wetted front was practically constant. In this case, the average potential-gradient would steadily decrease with time; and despite a certain amount of increase in water-conductivity of the soil during the infiltration-process, the reduced average gradient in pressure-potential was evidently the predominant influence in reducing the rate of entry. It should be pointed out that all measurements were made with vertical soil-columns having horizontal surface-planes and an impounded water-supply maintained at a constant level. The significant point here is that water-induced modifications in the soil-structure would be reduced to a minimum under such conditions. Such relative constancy in structural form at the soil-surface certainly does not always exist, during water-additions, and on sloping fields and watersheds pronounced changes in rate of entry may be brought about by structural alterations. He believes that artificial natural covers, such as a grass-mat or a layer of forest-floor, may do much to maintain the initial soil-structural arrangement on sloping surfaces. It also seems probable that the chief effect upon rate of entry of water into soil of a protective covering will be confined to deep soils.

C. W. BOTKIN, New Mexico College of Agriculture and Mechanic Arts--(1) Refers to measurements [10,11] which he thinks show an improvement where manure was cultivated into the soil both in the laboratory- and field-experiments.

(3) He feels there is no decided improvement in the permeability of soils below the depth to which the manure was incorporated.

(7) He refers to measurements [10] which indicate a decrease in the rate of entry of water with time.

T. F. BUEHRER and J. E. FLETCHER, University of Arizona--(1,2) Buehrer and Fletcher say they have observed that the infiltration-rate increases with the organic-matter content of an alkaline calcareous soil. Incorporation of organic matter with surface-soil, or the spreading of organic matter on the surface, has been found to increase markedly the rate of infiltration of rainfall. The pore-space, they say, invariably increases if organic matter is incorporated without puddling the soil or if it is used as a mulch. These observations are also in accord with those of Sprague and Marrero [56]. The nature of the organic matter added exerts a notable effect. Usually it has been assumed that the addition of organic matter to an alkaline calcareous soil would, as a result of bacterial action, lower the pH and bring about flocculation of the colloids. However, as Oberholzer [49] showed, the incorporation of one per cent of manure, alfalfa, and hegari, respectively, brought about practically no change in pH even over a period of 20 weeks. Even in the case of alfalfa, which gave the highest rate of decomposition, the pH change was only about 0.2 pH unit. Hence, it follows, they say, that organic matter has little or no effect upon the alkalinity or upon the dispersion of a soil. Nevertheless, in these experiments, the rate of percolation as well as infiltration of water was markedly increased. Whether this increase was due to a purely mechanical effect of the organic matter or to a change in size, number, or stability of the aggregates in the soil was not determined.

(6) They say that preliminary dilatometric measurements on alkaline calcareous soils in their laboratory have shown that in some soils which have a high organic-matter content, a smaller proportion of water seems to be bound than in alkaline calcareous soils with no organic matter. If the organic matter is highly hydrophilic, a considerable proportion of the water added may be in the form of a gel, with attendant swelling. Thus, while the water-holding capacity of such a soil is found to increase greatly due to addition of organic matter, the decrease in pore-space due to swelling tends to decrease the rate of infiltration.

(7) Studies by Fletcher have shown that the rate of infiltration changes with the time, even when the rate is steady. Initially the rate is very rapid, and gradually approaches a constant value. It is very difficult to obtain concordant results on infiltration-measurements on different points of a given plot. It is believed that the rate of infiltration is, in part, a function of the rate at which the air normally in the pore-spaces is displaced by the advancing water-column. Buehrer says he has shown that the rate is a function of the initial moisture-content already present in the soil. The infiltration-process is slowed up by reason of the air in the pores which, being insoluble in water, must be displaced. The initial moisture-content appears to determine the ease with which the moisture-film can displace the air film from the surface of the particle.

As a supplemental statement, Buehrer and Fletcher mention the investigation by Breazeale and McGeorge [12] on the effect of organic matter on the physical condition of the soil, who found that when a soil is puddled with organic matter at or near the moisture-equivalent, it is not restored to its normal structure by the organic matter. There is evidence to indicate that under such conditions the organic matter tends to stabilize the puddled state and keep the soil in a highly impervious condition. However, when worked with organic matter at moisture-contents above or below the moisture-equivalent, the structure of the soil is not impaired.

S. W. COSBY, Soil Conservation Service, United States Department of Agriculture--(1) Cosby states that frequent and general observations of the effectiveness of various soil-conservation measures, together with fragmental field-determinations, have convinced him that organic matter is highly effective in speeding the entry of water into soils. Certain soils on certain slopes, on which green manure-crops have been grown for the past several years, show far less surface-runoff and twice-as-deep wetted depth, than comparable soils and slopes continuously clean cultivated.

(2) Soils underlying patches of straw, litter, grass, and other vegetation, evidence less surface-runoff of water and a deeper wetted zone than do adjacent areas of similar soils which have no organic material on the surface.

(3) There is visible evidence of better structure and lower volume-weight (greater permeability) in soils recently brought into cultivation, or of those which have had organic content continuously maintained by cropping practices, than in the case of comparable soils which have had their organic matter reduced by continued clean cultivation.

(4) Cosby believes that water enters soils more rapidly when they have surface-litter, but there is no evidence that this is due to "filtering." It might well be a response to the cushioning effect of the litter intervening between the beat of rain, or velocity of runoff, and the wetted soil-granules. Without this litter, comparable soils do "melt" down to produce a thin surficial seal of fine particles.

(5) Plant fragments and other organic materials directly check the velocity and deflect the direction of surface-flow. The magnitude of this effect varies, being relatively greater in case of lighter rains on gentler slopes. Less clearly defined organic matter indirectly retards the lesser runoffs by increasing the rate of entry.

(6) Cosby believes that there is no clear evidence that organic matter increases the "water-holding capacity" of a soil. But it does induce more water to get into the soil in any given period of time.

(7) Rate-of-water entry varies widely with time for any given soil, under field-conditions; slowly at first, if soil is initially dry, more rapidly when original porosity still exists and particle-surfaces are fully wetted. Shortly, a more or less regular and intermediate rate appears to be established which continues as long as affecting circumstances remain unchanged. His observations have been made under field-conditions, however, and no such equilibrium has been long maintained.

N. E. EDLEFSEN, University of California--(2) Edlefsen says one direct effect of organic matter on the surface of a soil is to offer mechanical resistance to overland flow. The magnitude of this effect is probably overestimated and may not have any effect on the rate of entry.

There is an abundance of evidence that the rate of entry of water can sometimes be increased several times by growing a crop on the soil until the root-system is quite extensive, and then disking it down when the soil is fairly dry. This is probably due to root-channels developed in the soil.

An indirect effect of organic matter is sometimes thought to be the openings in the soil made by macroscopic organisms, the activities of which are encouraged by the presence of an abundance of organic matter. The relative magnitude of these effects have not, so far as he knows, been investigated.

(7) A large number of his measurements under field-conditions, the results of which are unpublished, show that the rate of entry of water into soils, under field-conditions, is always highest when water is first applied and gradually approaches a constant value. The shape of the curve varies with the moisture-content as well as other factors. The rate-time curve is, in general, different for every soil, and may vary considerably with the various conditions of a given soil.

I. C. FEUSTEL, Bureau of Chemistry and Soils, United States Department of Agriculture--(6) The effects of organic-matter additions to soil on the resulting moisture-holding capacity has been considerably overestimated. The fact that organic materials generally possess moisture-holding capacities much greater than those of mineral soil-materials has led to erroneous conclusions regarding the actual quantity of water held by mixtures of soil-organic matter as compared with the soil alone. Consideration must be given to the volume-weight relationships of organic matter to inorganic material in order to accurately evaluate the actual effects obtained from organic admixtures.

Other considerations that must be taken into account are the effects of added organic matter on the rate of moisture-loss by evaporation, the amount of unavailable water retained by the organic matter as compared with that held by the soil, and the resistance to wetting after having been allowed to become air-dry.

Experiments were conducted during 1935 to determine the effects of various types of peat on the moisture-holding capacity and on the moisture-retaining power over an extended period of drought with different soils [18]. The following conclusions were reached:

The moisture-holding capacity of each of the peats separately (ranging in type from a raw, fibrous moss-peat to a well-decomposed peat or muck) was approximately two to three times that of the soils used when expressed on a volume-basis. Moisture-percentages by weight are more or less meaningless when comparisons are made among materials differing widely in volume-weight. The maximum moisture-holding capacity of a peat may, for example, be as much as 20 or more times that of a soil, on a weight-basis, but such a comparison is actually misleading.

Mixtures of peat with soil in equal proportions by volume were capable of absorbing 40 to 50 per cent more moisture than untreated soil in the case of a clay-loam, and 80 per cent more in the case of pure quartz-sand with intermediate values for a loamy fine sandy soil. Mixtures of peat in lesser proportions had correspondingly lower moisture-holding capacities.

The results of evaporation-experiments conducted in a greenhouse showed that peats lost water more rapidly than clay-loam soil when each was initially saturated and the effect of mixing peat with this soil was to increase the rate of moisture-loss except during the early part of the evaporation-period when the rates for soil and peat were similar. A fibrous peat increased the rate of evaporation to the greatest extent and a well-decomposed variety to the least extent. Similar results were obtained with a sandy soil except that at first the soil alone lost moisture somewhat more rapidly than any of the peat- and soil-mixtures.

A tendency for peat- and soil-mixtures to retain more moisture relative to that held by the soil-checks was noted but the advantage became of less significance as the dry condition was approached during a period of drought. The more decomposed peats were more effective in this respect than fibrous peat but the former contained an appreciably greater content of water unavailable to plants. Moisture-contents of the peat-mixtures relative to that of the soil-checks after a dry period were greater on the sandy soil than on the clay-soil since the sandy-soil check approached its dry condition more rapidly than the clay-soil.

When the various materials had an initial moisture-content well below the saturation-capacity, but equal in each case, the effect of peat-admixture was generally to reduce the rate of moisture-loss, particularly on sand or sandy soil. In the case of the clay-loam soil, however, little effect was noted by incorporation of moss-peat but some reduction in the rate of moisture-loss was effected by a decomposed type of peat. The advantages of peat with respect to moisture-conservation were not as great, however, as these evaporation-rates appeared to indicate, even on the sandy soil, because of the increased content of unavailable moisture in what would otherwise have been a more favorable circumstance.

Observations in connection with golf-green field plots containing peat and clay soil-mixtures showed that the moisture-contents (after saturation from heavy rains) of the mixtures were very similar regardless of the type of peat used when expressed on a volume-basis. These values were approximately 50 per cent greater than those of the check-plot. The moisture-loss from these plots during a dry period was definitely greater from the peat-soil mixtures than from the check. The evaporation-rate was greatest in the case of the more fibrous peats. After a prolonged period the grass appeared to be suffering from drought on all of the plots. At this point the fibrous moss peat-soil mixture had a moisture-content slightly greater than the check-plot, whereas the muck-soil mixture had a moisture-content nearly twice that of the check-soil. The high permanent wilting-percentage of the latter mixture appeared to largely, if not entirely, offset the advantage of a higher residual moisture-content. Expressed in terms of pounds of water per cubic foot of dry peat, the unavailable moisture-content of moss peat was about five and that of decomposed reed peat about 15. The addition of peat to soil increased the unavailable moisture-content or wilting-percentage by an amount roughly proportional to the quantity of peat used and to the magnitude of the unavailable moisture held by the peat as compared with that of the soil before mixing.

He refers to work by Keen [24] in which manure plots at Rothamsted received 14 tons of manure annually contained on the average five per cent more moisture than the check-plot. An appreciable fraction of this increased moisture-content is admitted as probably being unavailable to plants.

Attention is called to some experiments [5] on the influence of humus-colloids on certain physical and chemical properties of clay-soils in which it was observed that there was an increase in the moisture-holding capacity resulting from the combination of clay- and humus-colloids which was greater than that attributed to a mere mechanical mixture. The result was explained on the basis of an increase in space between soil-grains or soil-porosity.

He also calls attention to the work of Musgrave and Norton [47] in which the addition of 16 tons of manure per acre did not increase the vapor-losses of fallow plots. The potential supply of moisture was increased on all plots to which manure was applied and was ascribed to the decrease in runoff on these plots. Soil-moisture determinations made at different depths on differently treated plots showed that the check-plot contained 25.29 per cent moisture in the surface to 6-inch depth as compared with a maximum of 28.38 per cent on a plot having 16 tons of manure per acre. Determinations at greater depths showed smaller differences resulting from organic matter.

An added comment by Feustel is that the results of evaporation-experiments conducted in greenhouse showed that peats lost water more rapidly than clay-loam soils when each was initially saturated and the effect of mixing peat with this soil was to increase the rate of moisture-loss. He states that the advantages of peat with reference to moisture-conservation were found not to be as great as evaporation-rates appeared to indicate, even on the sandy soil because of the increased content of the unavailable moisture. He feels that the use of peat as a soil-amendment for the sole purpose of conserving the moisture available to plants is not to be recommended, except possibly in the case of a decomposed type of peat with a sand or a very sandy soil. He calls attention to some data [60] which purport to indicate that peats supplied moisture to growing crops better than a sandy soil during a dry year.

He also refers to a study conducted by Wiggin [58] on the effects of various soil-moisture conditions on the growth of flowers in which peat- and soil-mixtures depressed the growth of zinnias but required less water as measured by an automatic irrigator. He also calls attention to a number of articles containing material on the effect of organic matter on the water-relations of soils.

Peat or other organic materials may render a heavy soil more open and pervious to percolating water and otherwise contribute to a favorable physical condition of such a soil.

H. J. HARPER, Oklahoma Agricultural and Mechanical College--As a general comment, Harper reports having made during the past year some studies on the rate of percolation of water into the soil by means of metal cylinders which were forced into the ground and a constant head of water maintained on the surface. Some unpublished data indicate very definitely that the rate of infiltration of the water decreases with time. The infiltration-rate of different types was quite variable. Where clay-pan types were encountered, the infiltration-rate of virgin soils is not any higher than cultivated soils after the surface-soils had reached equilibrium.

R. E. HORTON, Consulting Hydraulic Engineer, Voorheesville, New York--(1) Horton says that if the organic matter is in a sufficiently decomposed state so that it behaves like colloidal material, statements can be quite definitely made concerning the absorption of rain, since the presence of colloidal material tends, in general, to cut down the infiltration-capacity of the soil when the soil has remained well moistened until the colloids are fully swollen. On the other hand, colloidal material in excess tends, he believes, to produce a soil which sun-checks and has initially an abnormally high infiltration-capacity, unless recently cultivated. Whether organic material has the same effect or not, he says he does not know.

(2) The effect of organic matter on the soil-surface, such as dead grass or leaf-litter or humus in a forest, is certain to break the force of raindrops and cut down rain-packing of the soil-surface. This may be important in some soils but is relatively unimportant in sandy soils and probably not important for heavy clay-soils. The presence of a mulch or cover of organic material, leaves, grass, etc., has, however, at least two other important effects. In medium-textured soils containing colloids, the soil keeps its moisture better under the cover, and earthworms and burrowing insects, which work only in moist soils, will be present under such a cover and absent without it if the soil-surface becomes very dry. Consequently, there are likely to be many more large perforations in the soil-surface with cover than without. Studies of the hydraulics of infiltration indicate strongly, he says, that these large perforations are of great importance but more as a medium of permitting free exit of air when rain is entering the soil than as a medium of admitting water to the soil.

(4) He says that surface-litter unquestionably cuts down the velocity of overland flow, and consequently permits deposition of material in suspension when it might not otherwise occur. This, he believes, may result in filling up the larger openings into the soil-surface by inwashing of fine material and thereby cut down the infiltration-capacity. However, the effect unquestionably depends on rain-intensity and velocity of overland flow. His own studies, those of Dr. Lowdermilk, and some experiments with artificial rainfall at Colorado Springs the past summer leads him to the conclusion that for a fine-textured soil there is a critical velocity and depth of overland flow, such that for less depths and velocities of overland flow, inwashing will occur or material in suspension will be deposited and infiltration-capacity somewhat decreased; whereas for greater depths and higher velocities of overland flow, surface material will be picked up and carried along. He says that if there were initially a layer of very fine material on the soil-surface, this may be removed, exposing coarser material and somewhat increasing the infiltration-capacity of the soil.

(5) A second effect of organic cover is quite certain to greatly increase the frictional resistance to overland flow, thereby increasing the depth of surface-detention and also the duration of surface-detention during and after a rain. This permits infiltration to take place for a longer period [21].

(6) He believes that the presence of organic matter quite certainly increases the water-holding capacity of the soil.

(7) Horton says that analysis of many experiments at his laboratory and by the Soil Conservation Service show, in general, that most soils have what he terms a minimum infiltration-capacity which seems to be extremely stable and is more or less characteristic of the soil. He says that the sandy soils and pure sands are nearly always at their minimum infiltration-capacity; whereas the heavier types under particular conditions often have considerably higher values for a short time.

J. KITTREDGE, JR., University of California--(2) Kittredge says that he hesitates to hazard a reply to most of the questions if he were limited to direct effects, but if they are broadened to include the associated indirect effects, he concludes that organic matter on the soil-surface accelerates the entry of water into the soil, as indicated by his own investigations, and by the work of Auten [3] and Lowdermilk [28].

(3) He believes that organic matter, by favoring a more abundant or more active fauna, contributes macro-structure to the underlying soil, including animal-channels which may appreciably accelerate the rate of entry of water into the soil.

(4) The effect of surface-litter in filtering silty water was demonstrated by Lowdermilk [28] and from his own observations, when well-defined shoestring-gullies and mud-flows from partially denuded slopes disappeared and left deposits of the sediment lying on top of the litter material, where they passed down under the crowns of trees.

(5) The direct effect of organic matter, he believes, in retarding runoff, aside from the effect on infiltration mentioned above, would be a mechanical one due to the obstructions offered by the organic debris to the free movement of water over the surface of the soil.

(7) He has observed that the rate of entry of water into dry soil is faster during the first one to three minutes than it is during the subsequent seven to nine minutes. The measurements referred to covered a ten-minute period of application in each determination.

An added comment by Kittredge is that in a paper to be published in the near future, results of measurements on two areas approximately 50 feet apart will be reported. One is covered with pine forest, 25 to 30 years old; the other is in an area kept clear for a fire-break. He assumes that the conditions on the two areas before the pine was planted were as nearly identical as any two natural areas are likely to be. His volume-weight measurements of the upper two inches of mineral soils in the forest averaged 1.12 and in the fire-break, 1.38. Assuming a specific gravity of 2.6, he calculates the pore-space under the forest to be 57 per cent and in the open, 47 per cent. The forest-floor was scraped away before the measurements were made. The loss in ignition was 7.04 per cent in the forest and 7.13 per cent in the fire-break.. He found the moisture-content of this same layer to be seven to eight per cent in the forest, and from two to three per cent in the fire-break. Twenty-four hours after a heavy rain he found the average moisture-content under the forest to be 26.8 per cent and in the fire-break, 24.7 per cent. He found that about four times as much water entered the forest-soil as entered the fire-break soil in ten minutes.

M. R. LEWIS, Bureau of Agricultural Engineering, United States Department of Agriculture--(7) Lewis has found that in nearly all of his experiments the rate of entry of water into soil decreased with time, but approached a fairly constant value. The length of time before approaching the time of constant value varied with different soils.

A general comment by Lewis is that experiments [26] in which no attempt was made to differentiate between the effects of organic matter in the soil and growth of litter on the surface showed that the rate of entry of water on a soil that had been clean cultivated in an orchard was much less than on a nearby piece of virgin soil.

H. A. LUNT, Connecticut State College--(1) Lunt found that with shallow 4-inch tank lysimeters under red pine, the amount of water percolating through the soil during three years was nearly twice as much in the lysimeters where the natural litter was left on the top of the soil as was the case where the litter was removed, and that the presence of litter was decidedly beneficial in moisture-absorption and percolation, not only as a direct function, but also in reducing the period of freezing. The bare soil froze sooner and remained frozen longer than did the soil protected by litter [29,30].

(2) He refers to data [31] from which he concludes that there is a beneficial effect of forest-litter and a detrimental effect of its removal upon soil-structure; that where the soil has been kept bare of litter for a period of 3-1/2 years, the amount of aggregates in the first

inch has decreased about 40 per cent in comparison with soil retaining its natural litter-cover. Between the depths of the first and third inch the decrease was between 11 and 24 per cent. In pan-lysimeters in which soils were not disturbed in any way, removal of the litter over a 2-1/2 year period reduced the aggregate structure of the first inch by 36 per cent to 58 per cent, depending upon the method used, and in the depth one to three inches by 5 to 23 per cent. In cases where the natural soil-structure was less favorable, removal of the litter, he says, may or may not be detrimental to soil-structure depending upon the effect of such treatment upon ground-cover and lesser vegetation.

F. T. MAVIS, University of Iowa--(7) Mavis reports that the permeability of the sands with which he worked reduced progressively from day to day until the hydraulic gradient necessary to produce a given filter-velocity on the fifth day was about 60 per cent greater than that required to produce the same velocity on the first day of tests [33,34].

G. R. McDOLE, Soil Conservation Service, United States Department of Agriculture--(1) McDole concludes from his observations that incorporating organic matter does increase the rate of entry of water into soils.

(2) The presence of organic matter on the soil-surface retards runoff and, as a result, increases the amount of water added to a soil.

(3) He says he has no observations that would lead him to believe that organic matter, either on the surface or incorporated with the surface-layer of soil, has any appreciable effect on the physical condition of the soil below it.

(4) He has some evidence which indicates that the protective value of organic matter on surface serves as a buffer to the soil and prevents packing of the surface. He has also observed that, where sufficient crop-residues remain on the surface, they do serve as a check or filter and that the resulting runoff is usually clear, whereas bare soils lose considerable material in the runoff-water.

(5) The effect of the presence of organic matter in retarding runoff is greatest when used as a surface-mulch to the soil; however, observations show, he says, that the incorporation of a type of organic matter which decomposes quickly, such as sweet clover, does have a retarding effect on runoff. This is as great as where crop-residues are on the surface in sufficient amount to produce the effect of a mulch. To draw definite experimental conclusions relative to the effect of organic matter, it is necessary that the surface-conditions be the same. He feels that investigators should also distinguish between organic matter which has become a component part of the soil through decomposition. This type of organic matter usually has a carbon-nitrogen ratio approximating 12 to 1 and is to be regarded as part of the soil. Crop-residues, such as wheat stubble when partially incorporated in the surface-layer of the soil and partially left remaining on the surface, have a mechanical effect in retarding runoff and maintaining a more open and porous condition of the soil-surface which permits the water to remain until distributed through the subsoil.

(6) He refers to unpublished work in which he found that organic matter has a very high moisture-equivalent and that mixing organic matter with soil has the effect of increasing the moisture-equivalent. He observes some exceptions to this, but for most soils, which would be considered normal soils, that effect has been observed.

O. W. MONSON, Montana State College--(1) Monson says that incorporated organic matter definitely accelerates the rate of entry of water into the soil.

(2) The presence of organic matter, he says, on the soil-surface has comparatively little effect upon the rate of entry into the soil. Without organic matter on the surface, a heavy clay-soil will crack which leaves large openings which afford a rapid absorption for a short time until the cracks fill up.

(5) The presence of organic matter, either on the surface or incorporated in the soil, tends toward retarding surface-runoff, and thereby allows greater time for it to be absorbed by the soil.

(6) He says that organic matter in soil increases its water-holding capacity.

(7) The rate of entry of water into the soil changes with time. During the first half-hour or so the rate of absorption is much greater than after the rate has become steady. The rate of absorption becomes steady somewhere between one-half and one hour.

G. W. MUSGRAVE, Soil Conservation Service, United States Department of Agriculture--(1) Musgrave discusses the mechanical and physico-chemical effects of organic matter upon the infiltration of water into soils. He says such purely space-volume effects as result from the incorporation within the soil of coarse organic material thereby reduce the density of the soil-mass and add to the total amount of porosity as well as increase the average size of interstices [14,41,45,50,55]. Mavis and Wilsey [34] have shown, he says, that the permeability of sand to water varies with the total porosity as well as with the size of grain. Durante [17] showed a diminution of permeability of a clay-loam following its compression when wet. Many of these physico-chemical effects have not as yet been fully investigated and little is known in regard to some of them. For example, the effect of an organic colloid of specific character upon the permeability of a soil-complex is little known. In general, however, the effects of decomposed organic matter upon a soil include increased aggregation of soil-particles and significant increases in size classes of aggregates [8,14,32,50]. Such effects are accompanied by increases in the size of the individual orifices or pores of the soil with consequent more rapid infiltration.

(3) In a relatively impermeable soil within the surface of which coarse organic material was incorporated, water-penetration has been found to proceed even through lower lying untreated and comparatively dense horizons at magnitudes 100 per cent above that found in check-treatments [46]. Under these conditions it appears that water which once enters into the surface-horizon of a soil tends to gradually move downward through the soil-mass in what eventually is an appreciaole total amount. It is probable that the effect of the decomposition-products of organic matter extend to a much greater depth than that included within the immediate zone of incorporation. Such effects have been traced by differences of color to depths of 30 or more inches when various kinds and quantities of organic matter were incorporated within different plots to a depth of six inches only. The extent of physical improvement at various depths below the zone of incorporation should be systematically investigated. While these effects are those which have been commonly found thus far, it is possible, on the other hand, that entirely opposite effects may result from various decomposition-products. Thus, Lutz, in preliminary work, has raised a question as to whether the effect of the decomposition-products of organic matter upon a semi-lateric soil (Cecil clay-loam) may not be such as to make the soil more compact and dense. At least certain observations indicate a possibility of this kind and warrant investigation.

(4) Still another mechanical effect of coarse organic matter is that of reducing the turbidity of surface-runoff [28]. Protection from the impact of beating rains upon the soil-surface, such as may be provided by forest-litter (as well as vegetal cover), has been shown to greatly reduce the amount of suspended material in surface-runoff.

(5) The mechanical effect of organic matter in retarding runoff is exemplified by the surface-litter upon forested areas as well as by the effects of partially incorporated crop-residues on agricultural fields. It has been found at the Soil Conservation Service Experiment Station at Statesville, North Carolina, where comparisons have been made of various kinds of leaf-litter applied to the surface, that runoff has been reduced greatly below that of untreated check-areas [6]. The same effect is found on stony soils in contrast to soils from the surface of which stones have been removed. Retardation of the rate of overland flow is commonly accompanied by appreciable increases in the total amount of infiltration.

(6) The effect of organic matter on the water-holding capacity of soils, he says, might likewise be of diverse directions of trend. Upon the one hand, effects may be such as to eventually increase the permeability of a soil to water and permit its more rapid transmission to lower horizons. On the other hand, since the water-holding capacity of certain kinds of organic matter is greater than that of some soils, the water-holding capacity may be increased by its presence.

(7) The effect of long-continued time of application upon the rate of entry of water into a soil-profile is similar in all of the soil-profiles upon which we have made direct infiltration-measurements, including approximately 50 important soils of the eastern half of the Continent. At first, there is a rapid intake of water which within a short period, however, reaches a nearly straight-line relationship (declining almost imperceptibly with additional time-intervals).

O. R. NEAL, Soil Conservation Service, United States Department of Agriculture--(1) Neal says that the incorporation of organic matter into the six inches of surface-soil effected marked increases in the movement of water through a three-foot profile.

(2) Applications of organic matter, both as barnyard and green manure, to the surface-soil have been highly effective, he says, in reducing runoff from slopes of Marshall silt-loam. Dur-

ing the period from 1932 to 1935, under continuous corn, the runoff amounted to 9.26 [47] surface-inches from an untreated area. Where annual applications of 16 tons per acre of barnyard manure and green sweet clover were made, the runoff was reduced to 3.43 and 4.25 surface-inches, respectively. Under a fallow condition the same relative results were obtained, although the amounts of runoff were larger in each case. The total precipitation during this period was 96.54 surface-inches.

In lysimeters [46] containing soil of undisturbed field-structure, similar results from organic-matter treatments have been observed. The amount of precipitation passing through the soil-column and emerging as "percolate" has been significantly increased by additions of organic matter to the surface-soil. This effect has been observed on both the permeable Marshall silt-loam and the relatively impermeable Shelby silt-loam. Furthermore, it has been evident under both cropped and fallow conditions. With fallow untreated Marshall soil, during the period from 1933 to 1936, inclusive, the amount of precipitation passing through the 3-foot soil-column and emerging as percolate was 14.16 surface-inches. Annual applications, amounting to 16 tons per acre, of barnyard manure increased the percolate to 23.51 surface-inches during the same period. Under corn, the untreated units yielded 5.77 surface-inches while the treated units yielded 10.63 surface-inches of percolate. The total precipitation during this period was 102.41 surface-inches. On the relatively impermeable Shelby silt-loam, fallow untreated units yielded 2.20 inches of percolate while fallow units treated with organic matter yielded 5.46 surface-inches during the same period of time.

W. L. POWERS, Oregon State Agricultural College--(1) Powers reports infiltration-measurements under native sod and in old adjacent grain-land farmed for 60 years or more and found the infiltration from two to three times as rapid as in the native sod-land. He reports having three years' runoff-data from a set of 1/1000-acre erosion-plats that are in line with the above statement.

(2) He made applications of chopped alfalfa to the surface of erosion-plats and found that it decomposes and loses its effectiveness within less than a year and that it is fairly effective while it is in a fibrous condition.

(3) Organic matter has been found, he says, to improve the physical condition of soil below it partly because it attracts large numbers of earthworms and other like organisms [52].

(4) He mentions measurements of runoff from small plats with and without the forest-litter removed, and with cultivation. There was very little runoff with the natural forest-floor and it was colorless, whereas the denuded land yielded a heavy runoff of a coffee-brown color. He reports tests in which various kinds of cover on monoliths were brought into the laboratory in cylinders for use in percolation-tests. Quack grass-cover, including roots in the natural position, was very effective. Sand-cover was ineffective; in fact, it tended to seal up and check infiltration. He observed that blocking up of pores occurs at or near the surface. Near Yuma, Arizona, the silt from the Colorado is filtered out on the top of the sandy soil and scarcely penetrates the sand-soil below enough to stain it. The clay-colloid material accumulates on top until the depth is such that sand cannot be brought up with the ordinary plow in the course of one or two decades.

(5) He gives a summary of three years' data from his erosion-plats of 1/1000-acre and reports having had negligible loss of soil except on cultivated plots. The slope is about five per cent, and the soil is Willamette silty clay-loam. A striking example of reduction in runoff and washoff can be found at the Moro Field Station where a steep slope below the farmed area in plats is maintained in crested wheat-grass, and it filters out much of the silt that washes from the plats above.

(6) He reports that at the New Mexico Experiment Station he treated sandy mesa-soil with pulverized sheep-guano in amounts from one up to six per cent by spading it into the top part of water-capacity filters, which was found to increase the total water-retaining capacity against gravity by 0.8 per cent for each per cent of organic matter added.

(7) He says that infiltration is difficult to segregate and study entirely separately from percolation. He scratched the surface of the soil with the point of a paper-clip which markedly increased the amount of water going in for the time being [51]. He has found very generally in percolation-tests that the rate slows up with time and especially if the start is made with rather dry soil. Even in studies with bank-run gravel with well-strainers he has found this tendency to slow up with time. In the experiments with which he has been connected, the infiltration- or percolation-study has seldom been continued long enough to see what a constant would be.

C. F. SHAW, University of California--(2,4) Shaw reports that laboratory-experiments conducted at Berkeley and field-experiments conducted by Lowdermilk and others show that the presence of organic matter on the soil-surface prevented the destruction of the soil-granules and consequent silting up of soil-surface pores and gave a rate of percolation of water through the soil that in the laboratory was more than twice that of bare surfaces and in the field was always higher, but varied materially. This was not so much the effect of filtering of silty water as it was in preventing the silting by impact of raindrops or movement of water over the bare soil.

(5) The presence of organic matter very strikingly retards runoff, he says, as shown by experiments carried on by the Forest Service in their various experimental plots.

(6) It was found in a series of experiments carried on in Berkeley that the curve of moisture-equivalent corresponded very closely to the curve of organic-matter content. In one set of these experiments a very large quantity of fresh-ground alfalfa was added to a soil that was kept moist. As this alfalfa decomposed, the organic-matter content constantly decreased, but the moisture-equivalent curve corresponded very closely to that of the content of organic matter.

(7) He says that the rate of passage of water through soil changes with time. His experiments were made with a steadily maintained head of water noting the change in quantity passing through the soil, but there were no attempts to so vary the head or pressure as to maintain a steady rate of passage.

L. K. SHERMAN, Chairman, Section of Hydrology, Consulting Engineer, Chicago, Illinois--(7) Sherman reports having made some studies of the rates of infiltration during storms or successive storms, in which he finds that the rate of entry of water into soil decreases as the moisture-content of the soils increases. His investigations, he says, have been confined to applications of the investigations on rainfall and runoff made by Horner in St. Louis [20] and also the rainfall- and runoff-investigations on the basin of Ralston Creek, Iowa, made by Mavis [33]. [For Sherman's work, see 53.]

F. B. SMITH, University of Florida--(1) Smith refers to experiments which he believed show that incorporated manure significantly increased the rate of entry of water into soil [55].

(3) Experiments with Clarion loam and Marshall silt-loam, he says, show that the addition of farm manure or green sweet clover brought about an increase in the percentage of the larger sized water-stable aggregates and a decrease in dispersion, and, also, that the percentage of moisture in the soil at 24- to 36-inch depths during the summer of 1936 was greater on soils that had received applications of organic matter to the surface than on the check-soil. He feels that these data show that organic matter affected the moisture-movement and water-holding capacity of the soil at depths greater than the zone in which it was incorporated.

(7) He found that the rate of entry approached a constant value after about 40 minutes.

R. E. STEPHENSON, Oregon State Agricultural College--(1) Stephenson says his answers to the questionnaire are based principally upon data by other workers. He believes that incorporated organic matter increases rate of entry of water into the soil because the organic matter improves soil-structure and reduces dispersion [43].

(2) Organic matter on the surface, he says, increases the rate of entry of water into the soil by preventing soil-dispersion and helping to hold back runoff and to give more time for infiltration.

(3) He says that organic matter on the surface keeps the soil granular and open like a sponge. This is in part due to the protection against soil-dispersion by the rain, and in part to the channeling of insects which are active in breaking down the organic matter. A surface-cover of organic matter protects and encourages many insects, larvae, and worms that are important in channeling and opening the soil, particularly at the surface [40,22].

(4) Organic matter on the surface, he believes, prevents plugging soil-pores with muddy water, thus helping to maintain a high infiltration-capacity [1,54].

(5) Organic matter on the surface by affording millions of miniature dams tends to retard runoff and thus increases infiltration [44].

(6) He says that organic matter tends to increase water-holding capacity of soils (a) by promoting a better soil-structure that is capable of absorbing and holding more usable water and (b) the increased content of organic matter results in some increased water-holding capacity, but this is probably of minor importance over ranges through which it is practical to control the organic matter in soils [7,23].

(7) He feels that the rate of entry of water into the soil decreases with time (a) as voids become filled with water and (b) as the pores through which water enters the soil become obstructed with clay or other matter [42].

C. A. TAYLOR, Bureau of Agricultural Engineering, United States Department of Agriculture--(2) Taylor reports a test in which he established steady flow in a furrow 300 feet long and then filled the furrow with alfalfa-hay without making any measurable change in absorption.

S. C. VANDECAVEYE, State College of Washington--(1) Vandecaveye reports that results obtained by Horner at Pullman of runoff from two fallow plots--one located on land cultivated for 40 to 50 years and the other on adjacent land not cultivated until 1934--show the influence of organic matter on the entry of water into the soil. The higher content of organic matter in the latter soil is reflected in the lower water-losses as compared to the depleted soil. During the season of 1937, the old cultivated plot lost 0.574 of a surface-inch of water, whereas the other lost only 0.006 of an inch.

(2) He reports that, according to Horner, Four years' results on the comparison of runoff from wheat-stubble land with that of fallowed land show that the stubble-condition lost 5.91 per cent of the precipitation, whereas the summer-fallowed land lost 12.30 per cent. This difference is probably due, among other factors, to the presence of the straw on the surface and also to the root-system of the old wheat-plants. Field-observations have shown also that wheat-straw left on or mixed with the surface-soil results in decreased runoff and, therefore, in greater entry of water into the soil as compared to a condition where the straw or other crop-residues are burned.

(5) Annual field-observations were made, though no measurements, on the runoff of water from a series of field-plots started in 1920. Some of these plots have received regular applications of organic matter in the form of green manure whereas others have not. The loss of water by runoff, he says, has been much less from the former than from the latter plots. Consequently, the entry of water into the soil of the former was greater than in the soil of the latter.

(7) His observation is that the entry of water into the Palouse soils appears to be slower during the latter part of the rainy season than during the early part. He has no measurements to substantiate this observation, however, and therefore it expresses merely an opinion which may not be very significant.

F. J. VEIHMEYER and A. H. HENDRICKSON, University of California--(6) Veihmeyer and Hendrickson refer to unpublished data (paper presented at meeting of American Society of Plant Physiology, December 1932, The effect of manure on the water-holding capacity of soils) wherein organic matter in the form of well-rotted barnyard manure was added to soils varying in texture from a sand to clay. Applications were made on the basis of tons of organic matter to the acre. The increases in water-holding capacities as measured by moisture-equivalents, even with such tremendous applications of 200 tons to the acre-foot of soil, are surprisingly small, being only 0.2 and 0.3 of an acre-inch to the acre-foot of soil with the sandy loam, silt-loam, loams, and clays, and 0.4 of an acre-inch in the case of the fine sands. The maximum amount of readily available moisture, the difference between the moisture-equivalent and the permanent wilting-percentage which can be stored in the soil, showed no significant changes even with applications of organic matter far beyond anything practical in the field.

They refer to paper in press (Effect of cover-crops on nitrogen- and field-capacities of an orchard-soil, by E. L. Proebsting, to be published in Proc. Amer. Soc. Hort. Sci., v. 35, 1937). Proebsting found after 13 years in cover-cropped and clean-cultivated portions of an orchard of deciduous trees of various sorts that there has been no significant increase in the field-capacity as measured by the moisture-equivalent of the soil due to cover-cropping. However, Proebsting finds that there is a greater rate of infiltration into the soil in the cover-cropped plots than in the clean-cultivated ones.

As a general comment, they point out that the mechanical manipulations of the samples in making the determinations may markedly influence the results. An example of this effect is cited

[57] in using the moisture-equivalent to measure the effect of the replacement of other cations by sodium on the dispersion of soils.

E. A. WILLIS, Bureau of Public Roads, United States Department of Agriculture--As a general comment, Willis states that the Bureau of Public Roads is particularly concerned with the effect of contained organic matter on the physical properties of soils, and gives its conclusions on this subject. The Bureau recognizes organic matter as one of the primary constituents of soils. The organic matter may be present either in the fibrous form, accumulations of which the Bureau calls peat, or in the partially decomposed form which, when mixed with fine sand, silt, and clay, is referred to as muck. Soils high in organic content are characterized by their elasticity, high water-holding properties, and their general instability. An otherwise compressible soil may be rendered elastic by the presence of organic matter. Willis presents data on two soils and on mixtures consisting of 85 per cent of soil and 15 per cent of peat by weight. In both cases, the peat served to materially increase the elasticity as evidenced by the larger limit of shrinkage and field moisture-equivalent values. The water-holding capacity as measured by the centrifuge moisture-equivalent was also increased in both soils by the addition of peat. He also refers to data on the range in test constants of representatives of the soft peats and mucks. Sample 1 was obtained from Potomac River bottom-muck which failed to support hydraulic fills without displacing laterally. Sample 2 was obtained from a Minnesota muck which also has a tendency to displace laterally when supporting fills. Sample 3 from Virginia and sample 4 from Minnesota were composed of peat which failed to support dry-land fills. Samples 3 and 4 illustrate the very high water-absorptive and water-retentive properties characteristic of the peat-soils which have not yet reached the colloidal state by decomposition. That these two soils have not yet reached this state is indicated by the fact that their plasticity-indexes are zero and by the absence of water-logging in the centrifuge moisture-equivalent test.

Summary

It would be futile to attempt a comprehensive summary of the answers to the questions asked by the Committee since, in fact, the comments of each contributor are in themselves summaries. However, a few generalizations which can be drawn from the contents of the report seem to be justified.

Those who contributed information on question No. 1 were of the opinion that incorporated organic matter increased the rate of entry of water into soils. In some cases references were made to data upon which opinions were based. However, plowing with or without the addition of organic matter may, for a time at least, result in less runoff than from unplowed plots. This may give the appearance of greater rate of entry of water when, in fact, it may be only temporary storage of water in the loosened portion of the soil-profile. It is difficult in many cases to distinguish between high rate of entry and storage of water.

The effectiveness of either organic matter or plowing in storing water in the soil may be so limited in case of heavy rains as to be negligible in controlling floods, but may be sufficient to make a measurable difference in runoff in the case of light rains.

Furthermore, the roughening of the surface of the soil by plowing may decrease the rate of overland flow and the organic matter itself may play a minor rôle.

The concensus of opinion on question No. 2 is that the presence of organic matter on the soil-surface increased the rate of entry of water. As in the answers to question No. 1, the distinction between rate of entry and temporary storage is not clear.

There is little direct information in answer to question No. 3 and there is a conflict of opinions. The indirect effect of surface organic matter is believed by some of the commentators to be important because of the maintenance of favorable conditions for biotic activities which tend to open channels in the soil.

Some answers to question No. 4 were that the surface-litter filtered silty water and thereby increased the rate of entry of water. Others indicated that this probably is not the case and attributed the increase in rate of entry to the prevention of puddling of the soil-surface by raindrops.

There is agreement in the answers to question No. 5 that organic matter is effective in retarding runoff.

While every one agrees in answer to question No. 6 that organic matter increases the water-holding capacity of soils, it was pointed out by some of the commentators that the magnitude of these increases, which may be obtained from the addition of any amount within practical limits, is almost negligible. Attention was called to the fact, furthermore, that although the field-capacity is increased slightly by organic-matter additions to the soil, the amount of readily available water may not be increased.

There seemed to be unanimity of opinion in answer to question No. 7 that the rate of entry of water was greatest at the beginning of the application of water and decreased until approximately a steady rate was reached.

As a general comment, attention is called to the fact that the water transpired by plants on some of the experimental plots may not have been taken into account in the experiments designed to investigate the effect of organic matter on the water-relations of soils. The carbon-nitrogen ratios of the soil may be changed to such an extent that the plants on the organic-matter treated plots are reduced in size and therefore use less water.

There is a notable lack of agreement among investigators on some of the questions concerning the effects of organic matter on the water-relations of soils. Probably, before many generalizations can be made, the problem must be broken down into its basic components and the variables involved must be further differentiated. The questions asked by the Committee are an attempt in this direction.

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REPORT OF THE COMMITTEE ON UNDERGROUND WATERS, 1937-38

D. G. Thompson, Chairman

It is with regret that we record the loss of one member of our Committee, Professor Howard E. Simpson, who died January 31, 1938. Professor Simpson had been a member of this Committee since its organization in 1931. He had been engaged in ground-water studies for many years, beginning with investigations for the United States Geological Survey in Iowa in 1905. He was Assistant State Geologist of North Dakota from 1909 to 1919, State Water Geologist of North Dakota 1919 to 1933, and State Geologist since 1933 until the time of his death. In these positions he did much to reduce waste of water in that State from wells tapping the great Dakota formation that flowed under strong artesian pressure. His beneficial results were obtained largely through a campaign of education of well-owners rather than through a strict enforcement of State laws to prohibit waste. In 1915 Professor Simpson was responsible for the bringing together of a group of well-drillers in North Dakota who formed the first well-drillers association in the country. The formation of this association was the beginning of a movement that, after a slow start, has continued more rapidly in recent years, until now there are about 20 State or regional well-drillers associations, most of which are affiliated with the American Association of Water Well Drillers. Professor Simpson justly deserves the credit for starting this movement. In 1934 Professor Simpson was consultant for the Water Planning Committee of the National Resources Board, in charge of the preparation of a general report on ground-water conditions in the Mississippi Valley.

Since the last meeting of the Section of Hydrology there have been compiled abstracts of articles on ground-water hydrology for two separate issues of the contribution of the American Section of Hydrology to the International Bibliography of Hydrology under the auspices of the International Association of Scientific Hydrology. The first issue, covering articles published in 1935 and 1936, containing 140 abstracts, was distributed in the fall of 1937 with the Transactions of the American Geophysical Union. The abstracts--some 90 in number--covering articles published in 1937 were completed in March 1938, and presumably are available for distribution to members of the Section in published form. For this reason it is not necessary to make reference in this report to many articles that would otherwise deserve mention.

In this connection it is perhaps proper for the writer to record his admiration of the work of the Secretary of the Section of Hydrology in the compilation and publication of the first American contribution to the International Bibliography. It also seems proper to recognize the activities of the General Secretary of the Union and his assistants for their efforts in producing so pleasing a volume as the 1937 Transactions of the Union. Early a pioneer in the use of the "offset" method of printing of technical reports, the Transactions of last year attained a high quality and a promptness of publication of which the membership may well be proud. So far as the Section of Hydrology is concerned, and especially in regard to ground-water hydrology, it is apparent that the Transactions are filling a long existent gap in keeping hydrologists informed on current research.

The papers presented at meetings of the Section of Hydrology and those listed in the International Bibliography indicate the numerous phases of ground-water hydrology that are considered worthy of investigation. In this connection it may be stated that the subdivisions used in the International Bibliography, which follow the outline set up by the International Association, do not adequately cover the subject of ground-water hydrology, and it has been necessary to assign some papers arbitrarily to groups with which they have little logical relation. It is to be hoped that some revision of the topical outline may be made.

Late in 1937 there were published two valuable general contributions to ground-water hydrology. One is a textbook by Professor C. F. Tolman, "Ground-water" (McGraw-Hill Book Co., XVII - 593 pp., 189 figs.). This book is notable because it is the first comprehensive textbook on ground-water in the English language. Besides bringing together material from many scattered sources it contains the results of original studies by Professor Tolman and his associates. The book should prove to be not only a valuable textbook, but also a useful reference-book for hydrologists (see review by O. E. Meinzer, Bull. Amer. Ass. Petroleum Geologists, v. 22, pp. 490-494, April 1938).

The second important general publication is a volume by Dr. Morris Muskat entitled "The flow of homogeneous fluids through porous media" (McGraw-Hill Book Co., 763 pp., 284 figs., 1937). In this work Dr. Muskat, who for several years was a member of the Committee on Underground Waters, presents the results of original research by himself and associates at the Gulf Research Laboratories, with an exhaustive development of the mathematical concepts of flow of fluids through homogeneous porous materials.

Special attention may be called to three other reports. United States Geological Survey Water-Supply Paper 679-B, by Norah D. Stearns, H. T. Stearns, and G. A. Waring entitled "Thermal springs in the United States," is a compilation of information in regard to more than 1,000 springs whose temperature exceeds the mean annual temperature of the locality where they issue by more than 10°. Another Geological Survey report, just issued, is Water-Supply Paper 779, "Geology and ground-water resources of south-central Nebraska," by A. L. Lugn and L. K. Wenzel. This report is largely a description of areal conditions, but will be of interest to hydrologists because of many data it contains on extensive tests involving the use of the Thiem method of determining permeability made under the direction of Mr. Wenzel; and graphs of fluctuations of ground-water level in typical wells from the fall of 1930 to the end of 1934, during which period the precipitation was considerably below normal. A paper by A. T. Michelson and Dean C. Muckel, "Spreading water for storage underground," published as United States Department of Agriculture Technical Bulletin 578, presents the results of extensive studies of methods used in western States to increase ground-water recharge. H. F. Blaney presents a brief summary of the findings of this report (Appendix A).

In response to a request from the Water Resources Committee of the National Resources Committee, your Chairman has submitted comments on a statement of proposed projects for hydrologic research. Anticipating that requests may be made from time to time in the future for suggestions for other worthy projects, members of the Committee on Underground Waters were asked to suggest lines of investigation for subsequent consideration, with the following results:

Professor Gordon M. Fair has suggested a study of the determination of the surface-area of sand-particles. He points out the fact that this is an important factor in the movement of ground-water and infiltration, but one that has so far not been satisfactorily solved. This presumably would be a problem that might well be attacked in the laboratory of some university.

Dr. M. M. Leighton has called attention to two groups of problems that need investigation. One group includes problems arising from the use of ground-water for air-conditioning which, in some areas, already has become so great as to indicate a need of taking immediate steps to conserve the supply. There has been some tendency to minimize the danger--in part on the assumption that consumption of water can be reduced by using other substances as the cooling agency and in part on the assumption that the supply can be conserved by returning it to the ground. As to the first assumption it may be said that ground-water is likely to be used in abundance for air-conditioning as long as and wherever it can be used more cheaply than other methods of refrigeration unless there is legislation to prevent. A paper by M. L. Brashears, Jr., on the present program shows the results of an extensive study of the use of so-called "diffusion-wells" through which water from air-conditioning plants on Long Island is returned to the ground. In that area since 1933 more than 200 return or "diffusion-wells" have been drilled in compliance with requirements of the New York Water Power and Control Commission that the water be returned to the ground. The quantity of water involved, according to Russel Suter, Executive Engineer of the Commission, is estimated to be about 2,500 millions of gallons a year (averaging seven million gallons a day, but presumably much greater in summer than in winter). The result presented by Mr. Brashears shows that the problems of pouring the water back into the ground are not yet solved. In this connection attention may be called to an article by A. R. Tofte in the April 1938 issue of "The Driller" (Excavation Engineer Publishing Co., Milwaukee) quoting replies to a questionnaire sent to State geologists in regard to the use of diffusion-wells to return back to the ground. The problems of returning water to the ground obviously must be studied in the field, and it is hoped advantage will be taken of every opportunity to make observations on the effects of such returning.

Dr. Leighton's second suggestion relates to the problem of contamination of one artesian water horizon by water of another horizon moving through leaky casings, uncased holes through both formations. He states that in certain well-populated districts--such as in the vicinity of Chicago, Ottawa, and Rock Island, Illinois--the contamination of good water by salt water from lower formations is taking place and urges the use of deep-well current-meters, water-sampling devices, and electrical resistivity-surveys to investigate these conditions. In this connection attention may be called to a recently issued report of the Federal Geological Survey describing methods used to determine location of salt-water leaks entitled "Methods of locating salt-water leaks in water wells," by Penn P. Livingston and W. A. Lynch, Geological Survey Water-Supply Paper 796-A.

Appended to this report of your Chairman are statements submitted by members of the Committee, namely, Frederic H. Lahee, Harry F. Blaney, and A. C. Swinnerton, in regard to certain problems of ground-water hydrology.

In connection with the notes by Dr. Lahee, attention may also be called to the very grave problem involved in the production of salt water along with petroleum in many oil-fields. For many years the salt water was permitted to waste freely onto the ground whence in some places it reached the streams; in others it percolated downward to contaminate the fresh-water supplies. The problem of contamination has become so serious that much study and experimentation has been devoted to it. Several publications have been issued by the Bureau of Mines, United States Department of the Interior, in cooperation with the Kansas State Board of Health, and agencies of other States have studied the problem. In some oil-fields the salt water is being returned through wells either to its source-formation or other formations where it is supposed that fresh water-supplies will not be harmed. However, in some of the return-wells, for example, in certain fields in Kansas, after water has been taken in freely for a time, the pressure has increased greatly, and the return flow decreased or ceased entirely, so that it is obvious that the problem of disposal of salt water is thus not easily solved. In contrast, a salt-water disposal-well, drilled for that purpose to a depth of 1,090 feet in the Lake Barre oil-field in Louisiana, has been returning to the ground an average of 5,000 barrels of salt water a day (about 150 gallons a minute) since November 1933 (see Donald E. Fuelhart, "Subsurface disposal of oil field brines in southern Louisiana," Louisiana Dept. Conser. Inform. Circ. No. 2, pp. 26-28, November 1, 1937). The problem of disposal of salt water in oil-fields, and especially of its perhaps slowly felt but far-reaching effect upon bodies of fresh ground-water, raises many questions that deserve careful study. A recently issued report by Ogden S. Jones (Chloride survey in the north portion of Wichita, Kansas," Kansas State Board Health, 7 pp., March 1938) describes the slow movement toward the main part of Wichita--at a rate estimated to be a little more than 750 feet a year--of a body of salty water that entered the ground as waste from an oil-field above Wichita several years ago. Since the contaminating source has been diverted by pipe-line to a point beyond the City, it is expected that in course of time the chloride content of the water will decrease as the most concentrated part of the salty "slug" passes down the Arkansas Valley.

As in past years, a large number of investigations on different phases of ground-water hydrology are being conducted by the Division of Ground-Water of the Federal Geological Survey. During the current fiscal year, ending June 30, 1938, more or less detailed work has been done in some 28 States and the Territory of Hawaii, mostly in cooperation with Federal, State, or other governmental bureaus. It is anticipated that during the coming fiscal year investigations may be inaugurated in five or more additional States. Attention may be called to certain of the problems that are being studied.

The program of observations on fluctuations of water-levels and artesian pressure was continued, with records for several thousand wells in 28 States and the Territory of Hawaii for 1936 being published in Water-Supply Paper 817.

In connection with studies in New York, R. M. Leggette and C. E. Jacob have experimented in modifying water-stage records to adapt them to special phases of ground-water studies in such a way as to obtain much smaller time-scales for use where there is not much fluctuation, and to obtain much larger time and vertical scales for use where it is desired to study minute details of fluctuations of water-level. For example, with a mechanism which provides for a record covering a fraction of a minute spread over a linear distance corresponding to a week on the usual weekly recorder-chart they have obtained highly instructive graphs of small changes in water-level in a well as a railroad train has passed it.

Incidental to other studies, Thomas W. Robinson discovered on charts from a recorder on a well in New Mexico what seem to be unmistakably tidal fluctuations. The following brief statement of this most interesting phenomenon has been furnished by Mr. Robinson:

The well is in the SW 1/4 NE 1/4 sec. 17, T. 23 S., R. 29 E., on a peninsula in a large and shallow depression called the "Laguna Grande del Sal," which is partially filled with salty water. The aquifer tapped lies between 242 and 273 feet below the surface and contains a practically saturated brine of specific gravity 1.20. The brine rises to a level 46 feet below the land-surface. The well was drilled by the United States Potash Company as a test-well, under the close observation of Thomas W. Robinson and W. B. Lang of the Geological Survey. There is no pumping from the aquifer and hence no possible fluctuation of water-level from this cause.

An automatic record of water-level fluctuations is at present available from March 10 to April 19, 1938. This record clearly shows barometric effects. A mercury barometer is read every hour, day and night, at the refinery of the United States Potash Company, 1.75 miles west of the well. Comparison shows that the fluctuations of the water-level in the well amount to 70 per cent of the fluctuation of a barometer using a fluid of density 1.20. If the hydrograph of the well is corrected for the barometric changes, a residual curve is obtained showing the effects of other causes.

This residual curve shows definite approximately semi-diurnal fluctuations at times of full Moon, when the semi-diurnal fluctuations are about 0.05 foot, and new Moon, when the fluctuations are apparently somewhat larger. At the quarters of the Moon the fluctuations are indistinct, due possibly to slight errors in reading the barometer. The low-water stage occurs at times of the transit of the Moon within the probable error resulting from the nature of the barometric data and possible lag in the barometric response of the well. The water-level in the lake shows no similar fluctuations.

In this connection it may be worth while to call attention to a paper by Dr. Andrew Young entitled "Tidal phenomena at inland boreholes near Craddock (South Africa)" (Trans. R. Soc. South Africa, v. 3, pp. 61-106, 1913). This paper describes fluctuations that appear to have a typical tidal period, in a well that is about 100 miles inland from the coast of southeastern South Africa, and at an altitude of about 2,700 feet above sea-level. The New Mexico locality described by Robinson is, of course, much farther inland, and is probably at a somewhat higher altitude. The observations of fluctuation of water-level and of the barometer in this locality are being continued and the problem of the cause of fluctuations will be studied further.

Dr. C. V. Theis has devoted some time to his study of the mathematical approach to quantitative ground-water problems, including the development of formula to compute--from observations on the discharge, draw-down, and recovery of wells--the quantity of water removed from storage by pumping or artesian flow, the quantity returned to storage after artificial discharge ceases, and the safe perennial yield of the beds. L. K. Wenzel is preparing a paper on methods of determining permeability of water-bearing beds with special reference to pumping tests.

The Division of Ground-Water is continuing its study of the channel-storage method of determining effluent seepage in the drainage-basin of Difficult Run, Virginia, a tributary of Potomac River.

Attention may be called to one paper which does not in itself deal especially with problems of ground-water hydrology. This is a paper by Dr. M. King Hubbert, "Theory of scale models as applied to the study of geologic structures" (Bull. Geol. Soc. Amer., v. 48, pp. 1460-1519). In this paper the author derives the general theory of the similarity between a model and its original for purely mechanical systems and applies it in certain geologic problems to determine what the properties of the experiment of models should be. Possibly some of the same factors should be considered in laboratory study of some ground-water problems.

U. S. Geological Survey,
Washington, D. C.

APPENDIX A--GROUND-WATER INVESTIGATIONS IN CALIFORNIA

H. F. Blaney

As a part of the preliminary flood-control surveys which the United States Department of Agriculture is making, the Bureau of Agricultural Engineering is investigating ground-water conditions in California. Information has been collected on variations in water-table level, ground-water replenishment, overdraft of underground water-supply, etc., in the Merced Group, Kings River, and Ventura River areas. More detailed surveys are contemplated in the Los Angeles, San Gabriel, and Santa Ana River watersheds.

In the Kings River Area in central California, high ground-water resulting from the floods of 1937 and 1938 has seriously damaged agricultural crops such as deciduous fruits. Water in excess of irrigation-requirements is permitted, as far as possible, to seep underground to replenish ground-water supplies. Such practice, however, while providing for low pumping lifts, is likely, in periods of excess flow, to be detrimental to both soil and crops. Water-levels allowed to invade the root-zones of crops not only damage the roots by submergence, but often cause concentration of alkali salts in toxic quantities. Thus observation of water as now practiced may, and sometimes does, result in destruction of crops. Where these crops are fruit trees requiring years to bring to maturity, the situation becomes serious.

The Bureau of Agricultural Engineering, in cooperation with the Bureau of Plant Industry, the United States Bureau of Reclamation, the United States Indian Irrigation Service, and the Imperial Irrigation District, has been observing ground-water conditions in the Bard-Yuma Area between the Colorado River and the All-American Canal. This work is in charge of Dean W. Bloodgood, Associate Irrigation Engineer of the Bureau of Agricultural Engineering. Water-levels in 150 observation-wells are being recorded once a month. Ten recorder-wells equipped with weekly water-stage recorders have been located on 16,000 acres of land so that they represent various topographical conditions which prevail throughout the area. In addition to this, nine staff-gages on the Colorado River, Main Highline Canal, Haughteline Lake, and drainage-ditches are observed twice a week. Water-samples for salinity-determinations are being obtained at monthly intervals from all of the observation-wells, some of the drain- and irrigation-ditches, and the Colorado River. Data gathered during the past two years indicate a varying ground-water behavior which is influenced in some localities by deep percolation-losses from irrigation and underground water from the Colorado River. Some apprehension is felt over what ground-water conditions will be when water is turned into the All-American Canal.

The flood which occurred in southern California March 2, 1938, affords an excellent opportunity to observe the rise in ground-water due to excessive rainfall and runoff. Several weeks after the storm it was reported that in some areas no change in the water-table had occurred, while in other small basins the ground-water level had risen as much as 100 feet. The most reliable record available is that kept by the United States Geological Survey in cooperation with the Los Angeles County Flood-Control District with a continuous water-stage recorder on a well at Baldwin Park in the San Gabriel Valley. Here the average daily rise prior to the flood of March 2 was less than one-half inch, while the average rise for a period of 25 days following the flood was approximately seven inches per day. The rise for the latter part of March was about four inches per day. The depth to ground-water was approximately 100 feet on March 2, and 85 feet on March 27. The rainfall at Baldwin Park for February amounted to 8.67 inches and 10.44 inches for the first 27 days in March.

The Division of Water-Resources, Department of Public Works, State of California, is continuing the compilation of data on ground-water levels as reported in California State Bulletin No. 29, "San Joaquin River Basin" (1931). Maps are prepared for each year showing "Lines of equal elevation of ground-water table." In southern California the Division is measuring depth to water-level in typical wells, and taking water-samples for chemical analysis.

Spreading water for storage underground--Technical Bulletin No. 578, "Spreading water for stage underground," by A. T. Mitchelson and Dean C. Muckel, has been recently issued by the Bureau of Agricultural Engineering, United States Department of Agriculture. This publication describes the methods by which water which otherwise would waste to the sea is diverted into natural underground storage. It discusses the geological formation of absorptive areas on which these salvaged waters can be spread. Particularly described are the several basins of the South Coastal and Santa Clara River areas of southern California, and the Santa Clara Valley Area of the Central California Coastal Area. All the existing water-spreading systems in the State are discussed in detail, and diagrams and plates illustrate layouts and structures of the various systems.

Experimental work of the Bureau in selected areas is cited as a guide to methods which should be adopted under given conditions. The results of experiments conducted by the Bureau from February 1930 to 1936 are described.

These experimental projects have been in operation since February 1930 and cover tests on vegetated and denuded plots in order to determine the relative percolative-rate. Also used were plots simulating the various methods of spreading water, that is, the furrow-method, the basin-method, the ditch method, and sinking direct to the underground basin by way of shafts and wells. The effects of temperature, silty water, growth of algae, and fluctuating water-tables were also studied.

Some of the conclusions of the authors are:

Other factors being equal, the highest percolation-rates are obtained on land with the native vegetation and soil-covering the least disturbed.

The consumptive use by vegetation is negligible in comparison with its beneficial effect on the percolation-rate.

Although temperature has some effect on the rate of percolation, the change in temperature of the water during the spreading season is usually so small that its effect is overshadowed by other more important factors, and, furthermore, it is an uncontrollable factor in the spreading of flood-waters.

The ground-water has no marked effect on the percolation-rate until it comes in actual contact with the saturated soil extending a few inches below the ground-surface. When this contact is made, a very sharp decrease in the rate of percolation takes place. Maximum percolation-rates are obtained during the period the ground-water level drops after it has made contact with the ground-surface. The reason for the maximum rates at this time is not definitely known, but is probably due, in part, to the partial vacuum formed by the water which is then drawn underground as the vacuum is created. The effect of the dropping water-table on the percolation occurs only during the first few feet of drop.

Percolation-rates in basins or ponds, with the vegetation removed, decrease with the continued application of water. This decrease is due to re-sorting of the soil-particles and puddling of the soil-surface.

Higher rates of percolation in basins may be maintained by frequent harrowing or raking of the ground-surface.

Because of the high cost of sinking shafts or pits, their use is not economical if surface-spreading can be employed.

On streams subject to large floods, adequate works should be provided to completely control the amount of water diverted at all times.

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APPENDIX B--GROUND-WATER PROBLEMS RELATED TO PRODUCTION OF OIL

Frederic H. Lahee

In the various oil-fields very careful records are kept from day to day and, at regular intervals, systematic studies are made of the encroachment of water and of its relation to changes in average pressures and pressure-gradients in the fields which are under observation. The data are largely confidential.

During the past year it would seem that systematic handling of the water-problem has been satisfactory. In the East Texas Field, the problem of disposal of large quantities of water which may eventually be produced has not become immediately urgent, although it is receiving attention as something which must be cared for in the future. Another problem which is being studied by petroleum engineers relates to the amount of water which must be produced by edge-wells in a pool in order to secure the daily oil allowable. As the water encroaches and the percentage of water increases in the total volume of fluid produced (oil plus water), a small daily allowable of oil can be secured only by producing a much larger quantity of water. It is easy to see that, where there are more and more wells on the margin of a pool, producing a larger and larger percentage of water as time goes on, the problem of disposing of this water becomes increasingly urgent, and also the reduction of the energy, which may be contributed by the water-drive in the pool, becomes more and more significant. These two problems of water-disposal and of loss of energy by the production of water will undoubtedly need a great deal of careful study in the future.

It is generally understood now, but it may be worth repeating, that the water-drive in oil-fields is due primarily to expansion of the water within the pores of the sand rather than to actual mass-movement of the water through the sand. In this respect, then, the water-drive up-dip in the case of an oil-pool in the up-dip edge of a lensing sand is very similar to the water-drive in case an oil-pool is located on a closed domal structure or, as a matter of fact, on any kind of structure.

A subject which has been brought into wide recognition during the past year relates to the presence of water in the pore-spaces which also contain the oil in an oil-sand. It has been found, by securing bottom-hole samples of such sands under the conditions existing within the reservoir, that in most oil-producing sands so far examined there is a minimum of ten per cent, approximately, and often considerably more than ten per cent, of the pore-space occupied by connate water, which is apparently held in the finer interstices of the sand-body. The oil is supposed to occupy the more central parts of the pore-spaces. An excellent article on this subject has been read in several cities, before as many organizations, by Ralph J. Schilthuis of the Humble Oil Company. His paper was presented at the Oklahoma City meeting of the American Institute of Mining and Metallurgical Engineers in October 1937 (Tech. Pub. 869, Amer. Inst. Mining and Metal. Eng.). This discovery of the presence of connate water in oil-producing sands helps to account for the early yield of water with oil in some pools, and also helps to account for the relatively low resistivity indicated for certain oil-sands by the method of electrical logging. Under ordinary conditions, a sand, the interstices of which are filled with oil, should reveal high resistivity. Knowledge of this occurrence of connate water is leading to some changes in the ideas of production-methods and in estimates of oil-reserves.

Another paper which may be of interest is Report of Investigations 3392, published in March 1938 by the United States Bureau of Mines. This is entitled "Résumé of problems relating to edgewater encroachment in oil sands," by F. G. Miller and H. C. Miller. This paper was presented before the American Association of Petroleum Engineers at New Orleans in March 1938.

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APPENDIX C-- NOTES ON THE HYDROLOGY OF LIMESTONE TERRANES

A. C. Swinnerton

There is a considerable revival of interest in matters relating to ground-water behavior in limestone. Solubility of limestone and the behavior of calcium-carbonate solutions have been discussed in three papers which have come to my attention, and these are:

C. S. Adams and A. C. Swinnerton, Solubility of limestone, Trans. Amer. Geophys. Union, 18th Annual Meeting, pp. 504-508, 1937.
Edward Steidtmann, Humidity and waters of a limestone cavern near Lexington, Virginia, Virginia Geol. Sur. Bull. 46, pp. 35-47, 1936--not distributed until 1937.
Andrew Van Hook, The rate of adjustment of calcium bicarbonate solutions, J. Geol., v. 45, pp. 784-789, 1937.

Also three papers dealing with the physiography of limestone terranes have appeared as follows:

Robert A. Lawrence, Sink-holes of the Cumberland Plateau, J. Geol., v. 45, pp. 214-215, 1937.
A. C. Swinnerton, Structural control of the form and distribution of sink-holes, Science, v. 85, pp. 218-219, 1937.
Frank J. Wright, Natural Bridge of Virginia, Virginia Geol. Sur. Bull. 46, pp. 52-79, 1937 (reprinted with slight revisions from Denison Univ. Bull., J. Sci. Laboratories, v. 29, pp. 73-101, 1934).

At the meeting of Section E of the American Association for the Advancement of Science at Indianapolis there was a short symposium on limestone terranes with three papers (see abstracts Washington meeting, Geol. Soc. Amer., 1937), namely:

J. Harlan Bretz, Caves in the Galena formation.
Clyde A. Malott, Invasion theory of cave development.
Howard A. Meyerhoff, Textural contrasts in the karst topography of western Cuba and northern Puerto Rico.

In a paper by D. G. Thompson, O. E. Meinzer, and V. T. Stringfield entitled "Effect of a sea-level canal on the ground-water level of Florida" (Econ. Geol., v. 33, pp. 87-107, 1938), attention is called to the presence of openings presumed to be solution-cavities in limestone at depths of 34 to 42 feet below sea-level in test-holes drilled by the Army Engineers about 12 miles offshore in the Gulf of Mexico, and of similar solution-cavities in the Ocala limestone at depth of as much as 700 feet below sea-level at a number of places on the Florida peninsula.

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REPORT OF COMMITTEE ON DYNAMICS OF STREAMS, 1937-38

Lorenz G. Straub, Chairman

Although the field of activity assigned to the Committee on Dynamics of Streams has been confined largely to potomology, the work is so broad in scope that the activities of many other committees in the fields of hydraulics and geophysics border closely on those of this Committee. Much interest has developed in stream-dynamics and related fields since the time of the original formation of the Committee on Dynamics of Streams in the spring of 1932.

In presenting its annual reports this Committee has followed the custom of presenting a fairly comprehensive review of the literature pertaining to stream-dynamics in alternate years, although it is not intended that this precedent should be closely adhered to. It happens that at this Convention reports are being presented of other committees whose activities largely come within the scope of or are closely related to those of the Committee on Dynamics of Streams. These include particularly the report of the Special Committee on Flood-Waves, the report of the Advisory Committee on Soil-Conservation Research, and the progress-report of the National Research Council's Interdivisional Committee on Density-Currents. These three investigational groups are confining their attention to more narrowly defined fields. They have shown evidence of considerable progress and have stimulated a great amount of interest in their respective fields.

During the past year the International Association for Hydraulic Structures Research at its Berlin Convention stressed a field of potomology, namely, the study of the bed-load and suspended load of rivers including measurement and analysis. Certain research-projects being pursued under the sponsorship of the Special Committee on Hydraulic Research of the American Society of Civil Engineers are also quite definitely stream-dynamics problems.

The membership of the Committee remains the same as last year [see Trans. Amer. Geophys. Union, 18th Annual Meeting, 1937, p. 333]. With isolated exceptions, all of the members have been continuously engaged in work related to the various practical solutions of problems and research in stream-dynamics. Papers have been solicited and prepared for presentation at the meeting of the Section of Hydrology. Various translations have been prepared of foreign articles in river-hydraulics, particularly from the Russian, French, and German. Many of these have been listed in the Hydraulic Laboratory Bulletin of the National Bureau of Standards.

During 1939 especially great activity is contemplated in dynamics of streams because of the Assembly in Washington of the International Association of Scientific Hydrology at which the subject of potomology is a major division of the program.

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REPORT OF THE COMMITTEE ON CHEMISTRY OF NATURAL WATERS, 1937-38

C. S. Howard, Chairman

The membership of the Committee has remained unchanged [see Trans. Amer. Geophys. Union, 18th Annual Meeting, 1937, p. 342]. No special subject was considered by the Committee during this year and the year's report will consist of references to papers or reports on current work.

Mr. Scofield's paper entitled "Recent studies on quality of water for irrigation," presented at the regional meeting of the Section of Hydrology at Davis, California, January 7 and 8, 1938, gives a comprehensive summary of work in the field covered by the title. The author points out some of the deficiencies in the available material and suggests desirable standards for sampling and analysis.

A review of the chemical character of ground-water in the South Atlantic coastal plain by Margaret D. Foster was published in the Journal of the Washington Academy of Sciences [v. 27, pp. 405-412, 1937].

Considerable analytical work was done this year in studies of the chemical composition of the water in Lake Mead. A summary of the results is given by Mr. Eaton, Chairman of the Interdivisional Committee on Density-Currents.

During the year studies have been made of the composition of the water of the Pecos River under a cooperative agreement between the State of New Mexico and the Geological Survey of the United States Department of the Interior. Analytical results are available for inspection through application to W. F. White, Jr., New Mexico Military Institute, Roswell, New Mexico.

Current analytical data for the Colorado River at Grand Canyon, Arizona, have been kept on file and are available for public inspection at the offices of the United States Geological Survey in and near the Colorado River Basin.

Cooperative studies of the quality of water in Georgia are being made by the Georgia Division of Mines, Mining, and Geology and the Geological Survey of the United States Department of the Interior. The work involves analysis of composites of daily samples and of single samples from streams, analyses of some of the larger public water-supplies, and a survey of salinity in the Savannah River.

U. S. Geological Survey,
Washington, D. C.

REPORT OF THE COMMITTEE ON RAINFALL, 1937-38

Merrill Bernard, Chairman

The activities of the Committee on Rainfall for the past year are reported upon under the following headings:

(1) International bibliography--American literature on rainfall for 1937 was carefully reviewed and appropriate items listed and abstracted. This work was done largely by Miss Mary O. Soroka, member of the Committee, for which the Committee expresses its appreciation.

(2) A project to make available daily rainfall-records by stations--For the past two years there has been in progress in New Orleans a Works Progress Administration project, sponsored by the Weather Bureau and employing about 200 persons. The objective of the project primarily was the preparation of a marine atlas. In addition, the project undertook to assemble for statistical purposes nation-wide daily rainfall into weekly amounts. The results, in manuscript, give daily rainfall for the entire period of the record at each Weather Bureau station. Some idea of the extent of the project may be gained from the fact that there are 5,000 stations having an average record length of 30 years, involving in all 150,000 data-sheets. It seemed to the Chairman of the Committee that the advantages to hydrologic studies in daily rainfall assembled by stations might warrant the interest of the Committee on Rainfall in making such data generally available. Accordingly, the members of the Committee were circularized.

All agreed that the weekly totals would be of little interest to the hydrologist. Several felt that the cost of publishing would not be justified in view of the general availability of the Climatological Data Bulletin published by the Weather Bureau. Others felt that the value of individual station-records would be of considerable value to the hydrologic investigator and would fully justify the cost of making them available, either on loan at various regional centers or by purchase at a nominal cost per sheet. A particularly helpful suggestion was made by Professor E. W. Lane of the University of Iowa: "These data could be typed or written on form-sheets in such a way that they could be blue-printed at about five cents per sheet. Complete sets of the original prints or Van Dyke negatives could be filed in Washington or at several central locations throughout the country, and prints secured by any one desiring them. Each sheet could contain, say, three years of record at any given station, making the cost of the data about 1-2/3 cents per station-year."

The advantages of the availability of daily rainfall by stations will accrue largely to the investigator working at points where complete climatological data are not available. By utilizing the work of the New Orleans project it should be possible to make available at reasonable cost the full-length record at the comparatively few stations necessary to the study of any particular basin or region.

The Committee will continue to consider ways and means of publishing for distribution or sale rainfall-data in this form.

(3) The hydro-meteorological research-section of the Weather Bureau--Newly developed methods and techniques in the determination of stream-flow from rainfall have stimulated an interest in storm transposition as a means of estimating flood-flow and dam spillway-capacity. The Corps of Engineers, particularly, are actively interested and are cooperating with the Weather Bureau in a research-project, the objective of which is to develop limiting storm-potentialities by regions throughout the country. Air-mass analysis has an important part in the studies.

(4) A nation-wide hydrologic data-base--The recommendation of the Water Resources Committee of the National Resources Board to the President, dated February 4, 1938, provides for the establishment of hydrologic stations throughout the country to be accomplished within a 6-year program of expansion. These stations, equipped with recording precipitation-gages, will provide a network that can be expected to overcome a critical deficiency in rainfall-data for durations less than 24 hours.

(5) Other activities--The Chairman of the Committee has served actively on the subcommittee of the Water Resources Committee now engaged in the preparation of a report on the deficiencies in hydrologic research. The section on precipitation was reviewed by available members of the Committee.

Dr. George F. McEwen of the University of California at La Jolla, California, became a member of the Committee on April 6, 1937.

Six papers were submitted through the Committee for presentation at the annual meeting of the Union. The Committee was represented at the regional meetings held at Denver, Colorado, and at Davis, California.

U. S. Weather Bureau,
Washington, D. C.

SPECIAL COMMITTEES

REPORT OF SOIL CONSERVATION SERVICE SPECIAL ADVISORY COMMITTEE

Robert E. Horton, Chairman

Pursuant to a request from Dr. W. C. Lowdermilk, Director, Division of Research, Soil Conservation Service, United States Department of Agriculture, a special committee was appointed by the Chairman of the Section of Hydrology in August 1937 to act in an advisory capacity to the Division of Research of the Soil Conservation Service.

The present membership of this Committee is as follows: J. E. Church, W. W. Horner, A. F. Meyer, C. M. Saville, L. K. Sherman, Samuel Shulits, R. N. Towl, F. J. Veihmeyer, and Robert E. Horton, Chairman.

Two members of this Committee--L. K. Sherman and the Chairman--have also been appointed collaborators of the Soil Conservation Service in order to facilitate their work in connection with that of the special committee. The work of a collaborator of the Soil Conservation Service is also advisory and it is difficult to separate this work from that of the special Advisory Committee. Accordingly, both will be considered as appertaining to the latter.

One of the first objectives of the Soil Conservation Service Advisory Committee has been to familiarize its members with the work of the Soil Conservation Service in relation to hydrologic research.

The appointment of membership of the Committee was purposely placed on a geographic basis so that the Soil Conservation Service could avail itself of intimate local and regional knowledge possessed by different members of the Committee.

The appointment of the Soil Conservation Service Advisory Committee was decided upon at the Denver meeting, and, although this meeting was held in advance of the actual appointment of the members, the work of the Committee will be considered as dating from that meeting.

At the Denver meeting, a party inspected runoff-plat experiments, with artificial rain, which were being carried out at Colorado Springs. The inspection party included, among others, President Heck, W. C. Lowdermilk, R. S. Goodridge, and the Chairman. Following this meeting and utilizing the results of some of the experiments, a paper was prepared by the Chairman of the Committee on "Analysis of runoff-plat experiments," which will be published in mimeographed form by the Soil Conservation Service for the use of its research-workers.

A second inspection trip in September 1937 included a visit to the San Dimas Forest Experiment Station and the Wasatch Forest Experiment Station.

In December 1937, an inspection was made of the Coshocton runoff experimental plat, under the direction of W. D. Ellison, and the Muskingum Drainage-Basin climatic investigations, under the direction of W. C. Lowdermilk. Particular attention was given to certain special problems of ground-water phenomena and to the rainfall-studies in the Muskingum Basin. The matter of publication of the rainfall-maps was taken up and a program for such publication developed which is now in effect.

A paper on the Muskingum rainfall-maps was prepared by the Chairman of the Committee and published in planographed form by the Soil Conservation Service of the United States Department of Agriculture.

In January 1938 the Chairman attended the western meeting of the Section of Hydrology at Davis, California, and presented a brief outline of the research-problems of the Soil Conservation Service and their relation to the work of the Committee and of the Section of Hydrology. On return, the runoff research-work at Waco, Texas, was inspected.

In addition, in connection with the Section of Watershed and Hydrologic Studies, of which C. E. Ramser is in charge, the preparation of a regional hydrologic map of the United States has been undertaken, with special reference to problems of soil-conservation and flood-control. Several meetings have been attended by the Chairman at Washington in connection with this and other matters. Owing to the impracticability of other members of the Committee attending these meetings or making the field inspection-trips described, the members of the Committee have been kept more or less informed by correspondence of the work in progress and the Chairman has received many helpful and useful suggestions from them. Various members of the Committee have undertaken to inspect research-work in the field of the Soil Conservation Service in their vicinity.

In this connection a paper entitled "Preliminary outline for a comprehensive research on runoff-phenomena" was prepared by the Chairman for the Water Resources Committee of the National Resources Committee. Since the subject-matter of this paper falls exactly in line with the work of the Soil Conservation Service Advisory Committee, all members of that Committee were requested to submit suggestions and comments. Most of them have done so and this new material will in due course be digested and, it is hoped, incorporated in a revised draft of this program.

While the Advisory Committee as appointed is primarily a committee advisory to the Soil Conservation Service, Dr. Lowdermilk has taken the broad viewpoint, which is concurred in by all members of the Committee, that research in relation to runoff and conservation should not be carried out on a close communion basis by any one governmental bureau or agency but, rather, should be coordinated and conducted in the light of the broader needs of the nation as a whole, and with a view to the development of a sound hydrologic basis for conservation-measures by whatever Bureau they may be actually carried out. Accordingly, and at the request of Dr. Lowdermilk, considerable attention has been given to the problem of coordination of governmental research in hydrologic matters, and it is in part with a view to such coordination that the preparation of a regional hydrologic map of the United States has been undertaken. It is hoped that such a map, and the studies on which it is based, will assist in several ways:

(1) In allocation of runoff and hydrologic research in relation to conservation in a given region or locality to the Bureau or agency most directly interested in the conditions there pertaining, with, however, cooperation and a cordial, mutual intent and exchange of data between different bureaus engaged in different researches in different localities.

(2) Another objective of coordination in hydrologic research by governmental bureaus is to avoid duplication and overlapping and unnecessary work, and at the same time to prevent the ex-

istence of gaps certain to occur where one Bureau works independently of another on related problems. The Chairman ventures to give a single example of the desirability of close coordination and mutual helpfulness between workers in different bureaus in hydrologic matters. It appears quite certain that considerable hydrologic importance attaches to the character of wind-movement over an evaporation-pan, lake, or other evaporating surface, that is, whether the wind-movement is laminar or turbulent. Standard apparatus or equipment for determining gustiness of wind-movement is relatively costly and, to avoid this, Dr. Thornthwaite utilized a Fergusson recording rain-gage in the Muskingum Basin, substituting an evaporation-pan for the rain-gage funnel. It turns out that this equipment apparently records accurately the gustiness in the wind-movement as well as the evaporation but, down to date, sufficient data have not been collected to provide an adequate basis for certain determination of the validity of the results. If the method proves satisfactory, its extension to other stations is a matter of great immediate improtance. In connection with the Chairman's inspection of the San Dimas Forest Experiment work he learned that the same equipment had been developed there and that records covering about two years in a high mountain region were already available. Neither of the two agencies engaged in this work knew of the work of the other, but since one of the most important problems is the determination of the relative amounts of laminar flow in wind-movement in different latitudes and elevations, each could have profited greatly by a knowledge of the work which the other was carrying on.

It happens, in connection with the matter of coordination, that the objectives of the Soil Conservation Service are closely similar to those of the National Resources Committee in its efforts to secure interbureau coordination of hydrologic research-work. The Chairman of the Committee takes this occasion to call attention to this particular instance of cordial cooperation between two governmental bureaus working on related matters and to express the hope that such coordination of work may not only continue but be greatly extended in the future.

One thing which has become emphatically prominent in connection with the work of the Advisory Committee is the need for thorough study and further development of the methodology of hydrologic research. While not prepared specifically with reference to the Committee, attention is called to a paper by the Chairman, "Hydrologic research" [Science, pp. 527-530, December 10, 1937].

The work of the Committee, like that of the Soil Conservation Service, covers a broad field. The main function of the Committee may be briefly stated to be that of aiding the Soil Conservation Service in securing the best possible results from their experimental researches in hydrology, including research in the field and laboratory and correlative research. The work includes all the main phenomena of the surface-phase of the hydrologic cycle--surface-runoff, infiltration, soil-moisture, and ground-water--but it also includes many special hydrologic problems, such as flood-wave movement in stream-channels, and all of the phenomena of soil-erosion, both on the ground-surface and in stream-channels. In relation to the latter, an excellent and timely review of previous work has been prepared by Dr. Samuel Shulits, presented at the Denver 1937 meeting of the Section of Hydrology, and published in the present Transactions.

Voorheesville, New York

REPORT OF SPECIAL COMMITTEE ON FLOOD-WAVES, 1937-38

Lorenz G. Straub, Chairman

The Special Committee on Flood-Waves was established by the Executive Committee of the Section of Hydrology at a meeting held June 23, 1937, at Denver, Colorado. The setting up of this Committee was a culmination of previous informal conferences in Washington of persons concerned with hydrologic investigations for the Federal Government. It seemed to be the concensus of opinion of these meetings that, in view of the large number of governmental agencies interested in the genesis, propagation, and dispersion of flood-waves and hydrodynamics related thereto, a correlated research program of the various agencies might be carried on under some central research organization such as the National Hydraulic Laboratory of the Bureau of Standards. Although the procedure never had been definitely crystallized, it was suggested at these preliminary meetings that funds from various governmental agencies concerned with flood-wave phenomena might well be combined to foster a research-program.

It has been pointed out that the data and analytical studies related to various flood-wave occurrences are exceedingly fragmentary. The results of isolated observations for the most part have not been subjected to satisfactory rational analysis, and the development of mathematical theories of the various types of wave-propagation in open channels has lagged far behind the needs in connection with current developments in a national program for utilization of the natural water-resources of the country. This general statement is made notwithstanding the fact that several isolated studies of merit, both experimental and analytical, have been contributed to the literature both in this country and abroad.

Pursuant to carrying out the action of the Executive Committee of the Section of Hydrology at the Denver meeting, L. K. Sherman, Chairman of the Section, appointed the following Special Committee on Flood-Waves: Merrill Bernard, United States Weather Bureau; H. N. Eaton, National Hydraulic Laboratory; R. S. Goodridge, Soil Conservation Service; Robert E. Horton, Consulting Hydraulic Engineer; Ivan E. Houk, United States Bureau of Reclamation; W. G. Hoyt, United States Geological Survey; Hans Kramer, United States Engineer Corps; F. T. Mavis, State University of Iowa; M. P. O'Brien, University of California; Lorenz G. Straub, Chairman, University of Minnesota; H. A. Thomas, Carnegie Institute of Technology. All of the members of the Committee are closely associated in activities related to the dynamics of streams and flood-wave phenomena, and are for the most part concerned with the development of water-resources of the country.

The initial program proposal for fundamental research and investigations of flood-flow hydrodynamics as outlined by Mr. Goodridge, a member of the Committee, included the following phenomena:

(1) Propagation and change in configuration of flood-waves in a channel without tributaries.
(2) Continuation of item 1 for channel with tributaries, including the confluence of flood-waves and their subsequent propagation.
(3) Genesis, propagation, and dispersion of travel-waves.
(4) In the light of the findings of items 1, 2, and 3, to test methods of flow-assembly and routing for watersheds with a view to developing an adequate method.
(5) A study of the hydrodynamics of the flow of plastic mixtures with varying water-content and its analogy to the flow of debris from burned and eroded areas.
(6) Study of the entrainment of air by flow in chutes and torrents; the physical phenomena that lead to its occurrence; the conditions under which it may occur; the degree to which it may bulk the flow; and the rate at which the air is released.

Based upon the foregoing proposals and with consideration of a report by Mr. Goodridge for the Soil Conservation Service regarding deficiencies in basic meteorological data and other subjects leading up to the establishment of the Special Committee on Flood-Waves, a preliminary program was set up for the Committee by the crystallization of certain objectives with a view to preparing a fairly comprehensive preliminary report in time for this meeting of the American Geophysical Union.

Objectives--At the outset there seemed to be three fairly well-defined major objectives, namely: (1) A presentation of the status of the present knowledge of flood-wave phenomena; (2) a review of the activities in this country on practical hydrologic projects which involve a knowledge of flood-wave hydrodynamics (the various Federal, State, and municipal agencies concerned with water-conservation and stream-regulation probably more than other agencies are concerned with investigations and design of hydraulic structures having to do with flood-wave phenomena, although private enterprises are also interested); (3) the crystallization of lines of experimental research needed to fill the gaps in our present knowledge of flood-wave phenomena as related to natural and artificial channels. Steps were taken to pursue the first two objectives immediately. It was recognized that the third objective must await comprehensive study of the first two, although certain members of the Committee had studied the situation enough to have quite well-defined ideas of the needs thereunder.

Subcommittees--With a view to carrying out the objectives as systematically as possible, there were formed within the Committee on Flood-Waves the following subcommittees:

(I) Governmental activities--The function of this Subcommittee is to prepare a report which will give the extent of the work of various governmental agencies dealing with flood-wave phenomena. Here there are involved such questions as flood-routing, forecasting, flood-control, erosion, power-waves, etc. The members are W. G. Hoyt, Chairman, Merrill Bernard, R. S. Goodridge, Ivan E. Houk, Hans Kramer.

(II) Bibliography--An annotated bibliography of classical and recent works on flood-waves is being prepared, giving consideration to unanalyzed experimental data as well as to mathematical treatments. The members are F. T. Mavis, Chairman, H. N. Eaton, R. E. Horton, R. S. Goodridge, M. P. O'Brien.

(III) Current investigations--In view of the fact that there is at the present time considerable work in progress in various hydraulic laboratories under the direction of members of the Committee and also in connection with field-work of many governmental departments and private enterprises, a subcommittee was commissioned with the preparation of a report including the results of certain research-projects which are under way or have been recently completed and are not yet published. The members of the Subcommittee are H. A. Thomas, Chairman, H. N. Eaton, F. T. Mavis, M. P. O'Brien.

All of these subcommittees have been actively engaged in securing the information for which they were established. Although their respective functions have not yet been completed, the respective chairmen have prepared reports which form major parts of this report of the Special Committee on Flood-Waves.

Special assignments--In order to more fully utilize the knowledge and experience of individual members of the Committee in the preparation of a comprehensive report, certain members of the Committee have been given special assignments. Reports on these assignments are to form appendices to the report of the Committee. More recently special assignments have been made to persons outside the membership of the Committee and contributions relative to flood-waves are being sponsored at the annual meeting of the American Geophysical Union. The following assignments are in progress or have been recently completed:

(1) A project-statement on flood-flow hydrodynamics, R. S. Goodridge
(2) Rain-wave trains, R. E. Horton
(3) A review of Seddon's and Forchheimer's formulas for crest-velocity of river flood-waves, R. E. Horton
(4) Review of experiments on velocity of impulse-waves in a channel, R. E. Horton
(5) A mathematical analysis of flood-wave phenomena, H. A. Thomas
(6) Standard notation, R. E. Horton
(7) Classification of flood-waves, R. E. Horton
(8) A review of the mathematical treatments of flood-waves, G. H. Keulegan and R. T. Zoch

Further special assignments are being made as the work of the Committee progresses. The results of a number of these studies are here presented as appendices to the preliminary report of the Committee.

Suggestions have been made that the Special Committee on Flood-Waves serve immediately as an advisory committee to cooperate with certain governmental bureaus that are engaged in projects specifically within the scope of the Committee's activities. Such cooperation seems to come within the functions for which the Committee has been established. Further clarification of this relationship is to be recommended for consideration of the Executive Committee of the Section of Hydrology.

University of Minnesota,
Minneapolis, Minnesota

APPENDIX A--REPORT ON ACTIVITIES OF SUBCOMMITTEE RELATING TO GOVERNMENTAL ACTIVITIES OF THE SPECIAL COMMITTEE ON FLOOD-WAVES, MAY 1938

W. G. Hoyt, Chairman

In Dr. Straub's letter dated September 2, 1937, a subcommittee composed of Messrs. Bernard, Goodridge, Houk, Hoyt (Chairman), and Kramer, was asked to prepare a report covering the extent of the work of the various governmental agencies dealing with flood-wave phenomena. Under date of October 11, 1937, Dr. Straub advised the Chairman of the Subcommittee that the report should cover all agencies interested in flood-wave phenomena. Under date of December 4, 1937, the following organizations were requested to furnish a statement outlining projects which may have been completed or are now under way, with relation to the general subject of flood-wave phenomena:

National Hydraulic Laboratory
Corps of Engineers, U. S. Army
Geological Survey
Weather Bureau
Forest Service
Indian Service
Soil Conservation Service
Federal Power Commission
Bureau of Agricultural Engineering
Tennessee Valley Authority
Los Angeles Flood-Control District
Massachusetts Institute of Technology
Cornell University
University of Iowa
University of California
California Institute of Technology

Replies have been received and briefed as follows:

CORPS OF ENGINEERS, U. S. ARMY (prepared by Captain Hans Kramer, and authorized by the Chief of Engineers)--The War Department, under the direction of the Secretary of War and supervision of the Chief of Engineers, is charged by law (Flood-Control Acts of 1936, as amended) with the jurisdiction and prosecution of Federal investigations and improvements of rivers and other waterways for flood-control and allied purposes. In the planning and design of authorized flood-control projects, complete hydraulic studies embracing all phases of flood-wave phenomena pertaining to a particular project are conducted under the immediate direction of the District Engineer of the Engineer Department under whose jurisdiction the project is located. Extensive hydraulic research and model-experimentation work are conducted at the United States Waterways Experiment Station, Vicksburg, Mississippi, which is operated under the direction of the President of the Mississippi River Commission, except for special local problems which are investigated in colleagiate laboratories or field-laboratories in convenient locations.

U. S. GEOLOGICAL SURVEY (prepared by N. C. Grover, Chief Hydraulic Engineer)--The present program of the Geological Survey for surface-water investigations includes approximately 3,300 gaging stations on the streams of the country for observing stage and discharge. The coverage of streams is considerably more complete in some parts of the country than in others, but generally supplies much representative information concerning the behavior of the stream comprising the more important natural drainage-systems.

Approximately 2,400 of the stream-measurement stations are equipped with water-stage recorders which afford a continuous record of stage and discharge, and thus supply very essential basic information for studying the phenomena of flood-waves, power-waves, etc. In general, the network of stations is comprehensive rather than detailed in its coverage, and therefore specific studies of wave-phenomena may be expected to require considerable information in addition to those at regular stations. However, on many river-systems of the country the present program gives information which affords a very valuable general knowledge of the behavior of rivers and the characteristics with respect to concentration of flow and progress of waves.

In some of the recent notable floods, the Geological Survey has made special investigations for the purpose of collecting and publishing records obtained by its own and other organizations in greater detail and amount than is customary in the annual reports on surface-waters, with a view to facilitating studies of behavior of rivers in connection with floods.

It is appropriate to refer also to the mapping work of the Geological Survey as furnishing information regarding the characteristics of river-channels with respect to slope, overflow, etc., which is indispensable to the studies which are under consideration.

It is evident from the foregoing that the primary contribution of the Geological Survey in relation to the activities and investigations relating to flood-routing, flood-forecasting, etc., is that of furnishing important basic information. Its participation in detailed studies and analyses of these phenomena is incidental and of relatively less importance than the activities referred to.

U. S. FOREST SERVICE (prepared by E. N. Munns, Chief, Division of Forest Influences)--The program of the Forest Service has a number of different aspects which are discussed under the headings of "Construction" and "Investigation."

Under "Construction" the Forest Service is carrying on three activities: (1) Engineering measures on slopes and in water-courses; (2) debris-basins at the mouths of canyons; and (3) revegetation.

Under "Investigation" it has: (1) Studies of runoff from drainages; (2) studies of water-behavior from plots; and (3) studies of mud-flows.

Construction--The Forest Service is constructing engineering devices on slopes to delay the runoff. These devices take the form of terraces and other contour-devices designed to catch and hold eroded material and to retard surface-flow until it has a chance to be absorbed. The catchment-basins formed behind the terrace-trench are designed to hold flows in excess of normally expected rains. These devices are used only on those areas where the vegetative cover has been so depleted that the present growth must be reinforced in order to retard runoff and to check erosion.

Check-dams, retarding dams, and similar devices are used in stream-channels and water-courses to delay flow, to reduce velocity in the stream-channel and thereby to decrease the carrying power of water. These are also designed to catch eroded material that may be moving in the stream-bed and by providing a series of small settling basins also to trap eroded material that may be washed from the slopes. Dams are also constructed which permit a uniform flow of water through the dam but are designed to hold back water when delivered in excess of a regular flow.

Debris-basins are constructed at the mouths of small mountainous watersheds as a means again of trapping the heavier eroded material which is deposited as the water flows through such basins. These devices are used only where necessary to protect agricultural lands or communities and as a means of delivering to them water free from eroded material.

Revegetation is employed wherever suitable opportunities afford. This is done to reclothe the ground with vegetation in order that the litter and humus may be built up and that the soil itself may be protected from wind and sun. In areas where forests cannot be maintained, brush or herbaceous species are used.

Investigations--Studies of rates of runoff from drainage-basins having different slopes, soils, cover-types, and density of cover, and regarding different quantities and intensities of rain are under way in a number of different localities. The most widely known of these is the San Dimas Project in Southern California. Other projects on a less extensive scale are under way in a number of other places in the United States. These investigations have a wide variety of purposes, one of which is the effect of cover in land-use treatments.

Studies are also being made on a plot-basis where it is possible to hold all the factors constant but to vary rainfall. This is done through the application of different amounts of artificial as well as natural rain. Such studies aid in understanding the relationship of intensity and amount of rain to absorption and percolation, the rate of surface-runoff, water-flow through the soil, the formation of mud-flows, and similar related phenomena. This work, while highly indicative, is primarily to furnish the basis for understanding the behavior of water on large drainage-basins. The results of these studies are, however, significant in themselves.

The studies of mud-flows are being undertaken in areas where such flows occur, such as in various parts of the West. Studies are being made of the formation of mud-flows, the consistency of the material being moved, causes, and possible treatment of areas where these flows occur so that they may be prevented.

It should be realized that the construction-phases of the work enumerated above rest upon the investigative work described. It should also be recognized that the general purpose of our investigations are not so much in the field of hydrology as to work out those methods of land-management which will result in the delivery of maximum amounts of usable water.

SOIL CONSERVATION SERVICE, U. S. DEPARTMENT OF AGRICULTURE (prepared by R. S. Goodridge)--The Department of Agriculture, under the direction of the Secretary of Agriculture, is charged by law (1935) to "coordinate and direct all activities with relation to soil-erosion" with authority "to conduct surveys, investigations, and research" and "to carry out preventive measures, including, but not limited to, engineering operations." Further, under the Omnibus Flood-Control Act (1936) and as amended (1937), the Secretary of Agriculture is charged with jurisdiction and prosecution of "measures for runoff and water-flow retardation and soil-erosion prevention on watersheds" . . . "to supplement" the work of the War Department on "rivers and other waterways for flood-control and allied purposes" . . . .

One of the agencies of the Secretary of Agriculture engaged in carrying out the provisions of the acts of Congress is the Soil Conservation Service. Water in movement is the foremost agent causing soil-erosion. Consequently, the Soil Conservation Service is carrying on investigational research-activities designed to augment our largely empirically founded knowledge of water-movements and their relationships with the phenomena of erosion and its complement sedimentation. The Service is concerned with the processes resulting in accelerated erosion on areas with a disturbed natural balance. Notable evidences of accelerated erosion attend flood-occurrences in which the transporting power of the runoff-waters is increased many times. Accordingly, the Service is interested in the flood-wave phenomena and is engaged in a program of correlative research designed to strengthen the available procedures for assembling and routing flows through upstream watersheds for the purpose of serving as a basis for the design of soil- and water-control measures and supplementary works. The correlative research will be done in such a manner as to supplement that being conducted at the Bureau of Standards' National Hydraulic Laboratory and, in turn, will be guided by the advice of the Special Committee on Flood-Waves of the Section of Hydrology of the American Geophysical Union, National Research Council.

ADDENDA--For the purpose of enlarging upon the statement of the type of investigational and research activities being carried on, the following is added:

One of the objectives of the Research Division of this Service is to attack the problem of the flood-wave from the ground up to aid in determining the effect of soil- and water-control practices on flood-flows. Of first importance is the work on the independent factors of the soil-plant-water relationships to reveal knowledge of the manner in which these factors affect the ground-surface through antecedent and current conditions which influence the development of flood-waves during rainstorms. A coordinated analysis and synthesis of factors which underlie the phenomena of the flood-wave is being advanced to develop a more nearly complete understanding of the flood-wave and to strengthen the design of economically sound soil- and water-control measures and supplementary works. The more pertinent activities follow.

Storms--Work is being conducted in cooperation with the Weather Bureau on the life-history and analysis of storms and in developing improved methods of determining the expected frequency of rains of high intensities with particular reference to the storm as a natural entity with "eye" and migration-characteristics. The knowledge of the form and structure of storms will be materially advanced by the cooperative work with the Muskingum Conservancy District of Ohio on an area of 8,000 square miles on which are installed 500 self-recording rain-gages with an equal number of instruments for measuring temperature, relative humidity, and velocity and direction of wind. The average interval between stations is four miles. Collection of data began June 1, 1937.

Surface-areas--Much data have been collected by the Department of Agriculture on small plots and portions of watersheds, which cannot be directly applied to whole farm units nor to entire watersheds without a thorough knowledge of the hydraulics of surficial flow and channel flood-wave travel. Comprehensive studies of rainfall and runoff being conducted on full-size actual drainage-basins include the analysis of the flood-flow hydrograph and should do much to bridge the gap between the plot-experiments and their basin-wide application and to show quantitatively the extent to which practices of "Upstream engineering" may be effective.

Hydrologic research is being conducted on surface-areas with a wide range in extent. The size of the areas vary from a few square feet for the lysimeters to nearly 5,700 acres comprising the experimental watershed at Waco, Texas. Cooperative arrangements have been made with the Muskingum Conservancy District of Ohio which will provide the opportunity for certain types of flood-wave studies within its watershed-area of 8,000 square miles.

The Section of Soil and Water Conservation Experiment Stations has 23 experimental farms, distributed in 15 States, on which are made measurements of hydrologic factors (1) on about 250 plots, ranging in size from 0.02 acre to one acre, (2) on about 150 terraces, 0.5 to seven acres, and (3) on about 30 fields or small watersheds, 1 to 68 acres. Thirty of the small-plot surfaces are equipped for an accurate rate of surface runoff and others are being equipped similarly as rapidly as possible. The same section is conducting studies on infiltration from 16 centers in the country.

In cooperation with the Division of Operations the Section of Watershed and Hydrologic Studies has installed (May, 1938) equipment for hydrologic observations on 18 watersheds varying in size from 12 to 282 acres in six States. Present plans provide for a total of 84 watersheds varying in size from 10 to 350 acres in 20 States. These are being advanced as rapidly as possible.

An artificial sprinkler apparatus for simulating rainstorms on plots is being developed at the Bureau of Standards' National Hydraulic Laboratory and in the field for the purpose of aiding the development of information on infiltration and runoff. The procedures developed will be used on small watersheds, and correlation-studies are to be made with the experimental data obtained from natural rains.

Erosion and sedimentation--As the surface-waters move over the ground, erosion and sedimentation take place to a greater or less degree, materially influencing the dynamics and, in turn, the regimen of the stream-channels. On the above-mentioned surface-areas, measurements of the erosional debris will be made as far as possible and correlated with the hydrologic observations.

The Section of Sedimentation Studies is concerned with the approach to the problems involved in the measurement and analysis of the total sediment-load of natural streams by observations on the headwaters of the Enoree River, a tributary of the Santee River in northeastern South Carolina about eight miles east of Greenville at a gaging station specially fitted for taking bed-load observations. The drainage-area is 64.4 square miles. Observations are being limited to this station for the present because of the lack of funds. Two other similar stations have been built as shown in Table 1.

Table 1--Gaging stations of Section of Sedimentation Studies

Experiment station	Drainage-area (sq.mi.)	Tributary to	State	Location
Sandy Creek	98.2	Alabama River	Alabama	East-central, 2 miles south of Dadeville
Rocky River	85.2	Yadkin River, south	North Carolina	Central, 10 miles northeast of Statesville

The sedimentation-studies as they relate directly to flood-waves are carried on under two separate but interrelated projects which may be stated briefly as:

(1) Investigations of the total sediment-loads carried by natural streams at field-stations, as above; correlation of the different phases of these loads with the hydraulic functions of the streams and with the land-use and topography of the watershed; establishment of empirical methods for the determination of the bed-load carried by any stream; and studies of engineering methods for the control of the movement of sediment within a stream.

(2) Fundamental laboratory-investigations of principles governing the entrainment, transportation, and deposition of erosional debris by flowing water and the solution, under laboratory-controlled conditions of related problems. This project has been under way for the past two and a half years at the Cooperative Laboratory at the California Institute of Technology, Pasadena, California.

Parallel and concurrent research and hydraulic-laboratory investigations--The experimental watersheds at Waco, Texas (5,700 acres), and also at Coshocton, Ohio (4,600 acres), have seven stream-gaging stations on the principal channel. Six tributaries are gaged at Coshocton and several additional sites are possible. At Waco, however, no tributaries are being gaged because of the relatively flat topography with the resultant high degree of channel-storage which backs water up in the tributaries. Obviously, the opposing effects on the flood-wave hydrograph of the travel at wave-velocity and the configuration-change effected by channel-storage will be masked by other factors, especially by the ungaged lateral accretions to the flood-wave en route. In addition, the stream-systems in each of the watersheds are essentially fixed with respect to physiographic features. Measurements thereon, under varying conditions, will provide a series of observations from which empirical relations only may be indicated for the particular watershed studied and for the conditions observed. The limitations of such studies are recognized. There are basic relationships common to all watersheds which can best be studied under laboratory-control. The Service is engaged in correlative research to supplement that being done by other Federal and private agencies in order that desirable and feasible hydraulic laboratory-projects may be indicated which will strengthen the knowledge of fundamental laws of flow common to all watersheds.

The intensively gaged watersheds at Coshocton and Waco are to serve as proving grounds and will provide field-observations of the flood-waves which occur. This information can then be studied in the light of the current laboratory-results available for the extent to which the laws of flood-waves are applicable and furnish a more complete understanding of flood-wave phenomena of the stream-system. This approach will provide for the development of an adequate method for the assembly and routing of runoff which may be used to incorporate the findings of the Division of Research in the process of applying that knowledge to larger stream-systems.

It is of prime importance to establish the extent to which erosion-control is a part of flood-control, and to express this relation in terms which may be used in the design and financing of flood-control projects throughout this country.

Underlying the above is the problem of the flood-wave. It is basically required that engineers know more of the fundamentals governing flood-wave propagation and the extent to which nature may depart from the regimen indicated by their interpretation of the simple storage-equation. In this way their contributions, when combined with those of other scientists, may truly approximate the natural flood-wave phenomena.

BUREAU OF RECLAMATION (prepared by Ivan E. Houk)--The hydraulic-laboratory section of the Bureau of Reclamation, under the supervision of Engineer J. E. Warnock, is continually conducting theoretical, experimental, and practical studies of wave-conditions in connection with the design of spillway-channels, channel-outlets, chutes, drops, stilling pools, and other irrigation-structures. Types of waves considered in such studies include hydraulic jumps, standing waves, and translatory waves. However, since the laboratory-work is conducted primarily for the purpose of developing adequate and satisfactory designs, the investigations of wave-phenomena are more or less of an incidental nature. For this reason no reports on wave-investigations have been prepared which could be submitted to the Subcommittee at this time.

NATIONAL HYDRAULIC LABORATORY (prepared by H. N. Eaton)--The National Hydraulic Laboratory of the National Bureau of Standards has under way two projects that are related to flood-waves, both of them under the direction of Dr. G. H. Keulegan.

The first of these is a study of steady uniform flow in open channels. A paper in which modern flow-theories are applied to open channels is nearly ready for publication. The theory developed in the paper starts off with the acceptance of the general applicability of the Kármán law of universal velocity-distribution near solid boundaries. This law takes on three different forms according as the boundaries are smooth, wavy, or rough. In applying these laws to open channels of any cross-section, certain correction-terms are introduced in principle which, however, are shown to be negligible when the integration is carried out for the entire cross-section. These corrections are due first to the fact that the wall-shear is not uniformly distributed, and secondly because the free surface may be considered as a source of friction. The concept of "equivalent sand-grain roughness" as a means of establishing a standard of roughness is introduced, following the lead of Kármán and others. This makes it possible to establish a single general formula for the mean velocity of flow in rough channels.

The equations thus developed are applied to Bazin's data on open channels, and it is shown that his results fit into the framework afforded by the theory developed in this paper. In particular, the theory makes it possible to obtain a single curve in expressing the results of tests on a given rough channel with different slopes, whereas previous investigators have obtained different curves for different slopes, owing to the fact that they did not select properly the variables used in representing their results.

A study of Bazin's experiments on channels with wooden surfaces has revealed two regimens of resistance; one in which the surface behaves as if it were wavy, so that the effect of kinematic viscosity is important, and a second regimen, at higher velocities, in which the surface behaves as if it were hydraulically rough. This fact has a very important bearing on the achievement of dynamical similarity in model-tests.

Finally Manning's formula is discussed and is modified so as to express it in terms of dimensionless variables, one of which involves the equivalent sand-grain roughness.

The second project involves a study of the mathematical methods of treating flood-wave problems. It has been undertaken as a joint project between the National Bureau of Standards and the United States Weather Bureau. Drs. G. H. Keulegan and R. T. Zoch are collaborating in this study. The first objective is to obtain a comprehensive knowledge of the methods that have already been used by various investigators and the restrictions introduced by their assumptions. Following this, it is planned to prepare a resumé of the subject and to attempt the application of selected methods to practical cases.

U. S. WEATHER BUREAU (prepared by Merrill Bernard)--There is probably no agency more directly interested in the phenomenon of the flood-wave than the Weather Bureau in its river- and flood-forecasting service. This service is in the course of an expansion which has been stimulated largely by recent unprecedented flood-experiences on important headwater-areas and on a number of important smaller river-basins.

New methods are being continually injected into forecasting procedure and, while much remains to be done, significant gains have been made in the past several years. The objective of our service is an integrated forecast from headwaters to outlet on our major river-systems, which can, we feel, now be approached in practical terms.

On the headwater-streams the more recently developed methods of estimating flow from rainfall are being profitably utilized. In this field the greatest discouragement lies in finding published rainfall-data inapplicable to small drainage-areas, necessitating the establishment in almost all cases of an almost entirely new data-base.

On the larger rivers, forecasting will be a matter principally of solving the storage-equation progressively downstream. As the larger streams go under control their slopes, already characterized as flat, will be modified and still further flattened. This may have the effect of completely removing from the problem the need to consider the translation of the flood-wave throughout the length of a reach, or it may point to the necessity of knowing much more about wave-movement under abnormally flat slopes than we now know.

The flood-wave, as I conceive it, under the conditions in larger rivers, is nothing more than Nature's effort to establish equilibrium for the conditions of unequal inflow and outflow. This takes the form of a progressive distortion in the shape of the storage-prism throughout the length and through time, and which under fixed conditions of inflow and outflow may prove to be fairly constant.

I am fully in accord with a program of basic research on flood-waves. Our own needs, however, are so urgent that I am hoping a program of correlative research may be undertaken which will yield the practical means of meeting immediate situations. I had, in a recent work [Giving areal significance to hydrologic research on small areas--headwaters control and use, U. S. Dept. Agric.], to resort to a method utilizing the tangent to the area-discharge curve and graphic translation of the hydrograph as a means of estimating wave-velocity. Such a method has no rational explanation that is at all satisfying, but it does have a background in the work of Seddon and others. I have found also that the method gave results in approximate accord with the method presented by Professor Thomas, but entailed much less labor. The work to which I refer should be reviewed critically, in that the idea presented seems to be almost alone in its availability to meet certain immediate problems involving flood-wave velocity.

FEDERAL POWER COMMISSION--The Chief Engineer reports that the results of their flood-wave studies are so tied up in cases of litigation that no information can be made available at the present time.

LOS ANGELES COUNTY FLOOD-CONTROL DISTRICT (prepared by Paul Bouman, Jr.)--The Los Angeles County Flood-Control District has been and still is interested in the investigation of flood-wave phenomena. In its design of hydraulic structures, some attempt has been made to allow for certain flood-wave phenomena at present only imperfectly understood. The physical conditions of our watersheds and channels are apparently conducive to manifestations of wave-phenomena to a greater degree than in most sections of the country.

The District has installed a series of staff-gages on some of its steep concrete channels and has assigned observers to these locations to obtain information on the flows which are expected. The gages were so designed that data on wave-phenomena could be procured if opportunity presented itself. Up to the present time no sizeable flows have occurred and no opportunity has been given to obtain data.

Additional data on "surge" or "travelling" waves have been obtained in a few instances, such data being limited to very low flows in small channels. They were obtained as they occurred and where they were found, no previous preparation having been made for their accurate observation. Under these circumstances the accuracy of the data falls short of that required for scientific analysis.

In connection with its hydraulic studies and design, the District has planned to have an experimental investigation made by the California Institute of Technology on two phases of wave-phenomena. These projects are mentioned in the National Hydraulic Laboratory Bulletins. However, the extension of the present investigation now under way on superelevation in curved channels has not permitted initiation of the work on waves.

In general, it may be stated that no definite projects on flood-wave phenomena have been completed. The collection of field-data is the present order of business, and arrangements have been made to obtain these data when and if they become available. Such data as have already been collected are indicative rather than conclusive.

TENNESSEE VALLEY AUTHORITY (prepared by A. S. Fry)--We have made a number of studies of the travel of flood-waves in streams in the Tennessee Valley Region. Releases from Norris Dam have been studied as these travel down the Clinch River and into the Tennessee. It has been necessary for us to determine the time of travel for releases from Norris in order to estimate when such releases will be effective at down-river points, such as in the vicinity of Chattanooga and Wheeler and Wilson dams. Norris Dam is 79.8 miles above the mouth of Clinch River. Five recording gages are installed in this stretch of river-channel which furnish data as to time, beginning of rise, crest of rise, beginning of fall, and other points pertinent in the study of wave-travel down the Clinch River. We have also made some studies of wave-travel below Wilson Dam for fluctuations in the discharge at Wilson.

Another study has been made of fluctuations on the Hiwassee and Ocoee rivers below existing power-plants where diurnal fluctuation comes into play.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY (prepared by Prof. H. K. Barrows)--We have done nothing in the way of flood-wave study here at Massachusetts Institute of Technology since 1931, due to lack of men. In 1931 Messrs. Brown, Childs, and Moody made some preliminary experiments using a 75-foot wooden flume, two feet wide by 1.5 feet deep, their results appearing in a thesis of that year. The same year another student, Harry Landsman, made a critical review of the subject, making use of the data obtained by the first-mentioned trio. Arrangements can probably be made to make the results available to the Flood-Wave Committee.

CALIFORNIA INSTITUTE OF TECHNOLOGY (prepared by Prof. Th. von Karman)--I am afraid that I cannot contribute essentially to the present work of your Committee. I am interested in the theoretical aspect of the question and in the application of some mathematical methods for forecasting the wave-motion. For such a purpose, I would like to have data but I am afraid I cannot furnish much information. There is only one investigation which is going on on the campus of the California Institute of Technology which is related to the problem of flood-waves. This investigation is carried out on behalf of the flood-control of Los Angeles County. The subject is "High-velocity flow in curved channels." Professor R. T. Knapp and Dr. A. Ippon are in charge of the experimental investigation and I, myself, contributed something to the theoretical analysis of the problem.

THE UNIVERSITY OF CALIFORNIA (prepared by Prof. M. P. O'Brien)--The following projects are now under way at the University of California:

(1) Small-model experiment on the travel of flood-waves in a trapezoidal channel with the wave generated by a known variation in the rate of discharge. Last year the work included a constant slope with variation in the discharge-cycle. This year we are investigating the effect of slope with the principal objective a determination of the conditions under which flood-waves increase in steepness on the forward face. Dr. Horton has proposed a criterion for this condition and we are attempting to check it experimentally.

(2) A model-experiment on the rate of advance and the rate of damping of waves generated by a known variation in elevation at the mouth of a long channel or regular section. This investigation has to do primarily with the problem of tidal waves in estuaries and tidal rivers but should also supply general information on the propagation of flood-waves.

(3) A study of oscillations (seiches) in bodies of water with free surfaces. The program includes channels of both constant and variable depth. There is a considerable literature on this subject. We are attempting first to check the theoretical formulas for the period of oscillations and will then investigate the rate of damping with particular reference to the problem of whether damping is proportional to the first power or the square of the velocity.

(4) In connection with these projects we are attempting to develop a theory of wave-motion which will include the effect of friction.

THE STATE UNIVERSITY OF IOWA (prepared by Prof. F. T. Mavis)--The following studies conducted largely by graduate students under direction at the hydraulics laboratory of the University of Iowa may be mentioned: "Prediction of floods," by Ping-Yi-Lin, Master's thesis, University of Iowa, 1925; "A study in flood-waves," by Elmer E. Moots, Ph.D. thesis, University of Iowa, 1927; "A study of flood-waves on the Iowa River at Iowa City," by G. H. Hickox, Master's thesis, 1926; "The moving hydraulic jump," by G. H. Dunston, Master's thesis, 1929.

The following papers by members of the staff of the University are selected from recent papers bearing upon the hydraulics of flood-waves rather than the statistics of floods: "Flush-wave velocities in sewers," by E. W. Lane and O. J. Baldwin, Eng. News-Rec., June 11, 1936; "Certain flood-flow phenomena of Iowa streams," by F. A. Nagler, Trans. Amer. Geophys. Union, Section of Hydrology, pp. 322-327, April 28, 1932; "Slide-rule for routing floods through storage-reservoirs or lakes," by C. J. Posey, Eng. News-Rec., v. 114, No. 17, p. 580, April 25, 1935.

Two current theses deal with the hydraulics of flood-waves, one a library-study, and the other a laboratory-study which is still in its formative stage.

As Chairman of the Subcommittee on Bibliography, I hope to be able to include abstracts of a considerable number of the 300-odd references to hydraulics of flood-waves which have been compiled to date. Abstracts of the papers and theses enumerated above will be available, and considerable progress has already been made in the preparation of abstracts in some parts of the Bibliography.

U. S. Geological Survey,
Washington, D. C.

APPENDIX B--REPORT OF SUBCOMMITTEE ON BIBLIOGRAPHY OF SPECIAL COMMITTEE ON FLOOD-WAVES

F. T. Mavis, Chairman

The Subcommittee on Bibliography appointed by the Chairman of the Special Committee on Flood-Waves on September 2, 1937, consists of H. N. Eaton, Robert E. Horton, R. S. Goodridge, M. P. O'Brien, and F. T. Mavis, Chairman. The Subcommittee was instructed to prepare "an annotated bibliography of classical and recent works on flood-waves ... probably giving consideration to unanalyzed experimental data as fully as to mathematical treatments."

As a first step, a list of some 300 references has been assembled. This list will probably be reduced somewhat after those articles having inconsequent bearing on the flood-waves problem have been eliminated. These references have been distributed to all members of the Subcommittee, and each member has been asked to prepare or enlist assistance in preparing annotations and abstracts on articles pertinent to flood-waves.

The following suggestions were outlined as a general guide in the preparation of annotations and abstracts:

(1) Only articles pertinent to flood-waves should be included in the annotated bibliography and abstracts. Their scope may be theoretical, mathematical, experimental (laboratory), observational (field) of natural or controlled phenomena, analytical; pertinent to regular or irregular channels; not directly applicable to flood-waves problem but probably helpful in analysis; original work or restatement.
(2) References should be complete, giving author, title, journal, volume, pages, date (for books, give publisher's name and address).
(3) Annotations and abstracts should be informative rather than merely descriptive. They should brief new methods and results whenever practicable.
(4) The length of annotation or abstract will depend upon the importance and availability of the original article. The average length should be 10 to 15 typewritten lines (100 to 150 words) exclusive of the reference itself. Length should not exceed 500 words in any case.
(5) If essential, a small figure (not larger than 4 by 6 inches and suitable for half-reduction) may be included.
(6) If references in considerable number accompany the paper, state this fact in the abstract or annotation.
(7) Abstracts and annotations should preferably be typed double-spaced on 8-1/2 by 11-inch paper with 1-1/2-inch margin on left.
(8) Each abstract should be signed (and dated) by the abstractor.

In connection with a Master's thesis, H. C. Lay, graduate student in hydraulics at the University of Iowa, has prepared digests of more than 50 papers on the hydraulics of flood-waves and related phenomena. These digests, prepared under the direction of Professor F. T. Mavis, will be further condensed with a view to including them in the abstracts of flood-wave literature. The studies reported in the foregoing papers are being further correlated by Mr. Lay.

In cooperation with H. N. Eaton, Dr. G. H. Keulegan, National Bureau of Standards, Washington, D. C., has assisted in reviewing a number of mathematical articles pertinent to flood-wave movements. Dr. Keulegan and Dr. R. T. Zoch, of the United States Weather Bureau, will collaborate in preparing "A review of the literature on the mathematical theory of flood-waves," as a special assignment of the Committee.

In addition to special assignments related to the work of the Subcommittee on Bibliography, namely, "A review of Seddon's formula," and "Review of experiments on velocity of impulse-waves in a channel," Robert E. Horton is preparing abstracts of his own experiments and the experiments of Darcy and Bazin.

R. S. Goodridge reports that the Division of Research of the United States Soil Conservation Service under the direction of Dr. W. C. Lowdermilk, through the Section of Watershed and Hydrologic Studies directed by C. E. Ramser, is considering a program of correlative research pertinent to routing flows. This will supplement correlative research at the Bureau of Standards' National Hydraulic Laboratory, and in turn will be guided by the advice of the Special Committee on Flood-Waves of the American Geophysical Union, Section of Hydrology.

Acknowledgment is gratefully made to Dr. C. A. Wright, Dr. G. H. Keulegan, Professor O. J. Baldwin, Professor Elmer E. Moots, and John B. Drisko, who have assisted in preparation of annotations and abstracts.

State University of Iowa,
Iowa City, Iowa

APPENDIX C--REPORT OF SUBCOMMITTEE ON CURRENT INVESTIGATIONS OF SPECIAL COMMITTEE ON FLOOD-WAVES

H. A. Thomas, Chairman

This Subcommittee was commissioned with the preparation of a report including the results of certain research projects which are under way or have been recently completed and not yet published. With this in mind as many detailed statements as possible have been obtained by corresponding with those in charge of the projects. Although the results have been not too encouraging, it is felt that some progress has been made.

Though not all of the following deal directly with flood-waves, it is thought that they are so closely related that they should be included. The following abstracts are listed according to the laboratory at which the work has been or is being done.

University of California

"Flood-wave studies" by L. B. Wilby--The study is concerned with the factors which affect the travel of a flood-wave (a wave of comparatively long wave-length and small amplitude), down a channel, and principly to find out what happens to the shape of the wave, and how it is damped out (or built up as the case may be). This study is a continuation of the work begun at this University by Lieutenant J. O. Killian last year. The problem of Lieutenant Killian was to find out by means of a model-study what modifications could be made in the capacity of a canal. The canal was to carry the discharge from a power-plant which was to operate on a certain cycle. It was to discharge 2000 foot-seconds for 15 hours of the day, and only 400 foot-seconds for the remaining nine hours. The canal was to be 70 miles long, discharge into a large reservoir, and have an initial depth of ten feet. In continuation of this study, it is hoped to be able to generalize the results of Lieutenant Killian's study to obtain results applicable to at least prismatic channels.

Because of the specific nature of his problem, and also due to limitations imposed by time, Lieutenant Killian was forced to maintain certain factors constant throughout his experiment. His channel was a straight trapezoidal reproduction of the prototype (distorted in three dimensions) of constant slope. He reproduced the cycle of discharge both as to time and quantity as determined by model-laws. He obtained friction by means of wire-screens, varying the number per foot of length of channel for different runs. He also varied the wave-length by using multiples of the original cycle of discharge. These are the two principal factors whose effect he investigated. For the specific problem, he found out that at a distance downstream from the power-plant of 49 miles the depth could be reduced to approximately 8.2 feet, but that from that point on down, the back-water from the reservoir caused the curve of maximum depths to rise instead of continuing to fall. In general, he found out the effect of both friction and wave-length very well for his particular waves. However, as he points out, the results are not generally applicable.

In this continuing study, the first phenomena to be studied is the possibility of obtaining a combination of slope and friction for which the waves will build up instead of damping out, as they travel down the channel. It may be that this will occupy the whole year (1937-38). However, if this special problem can be disposed of, and the factors which cause it ascertained, it is desirable to find out the effect of the principal factors which were kept constant, or not studied sufficiently last year, that is, bottom-slope, time-ration in wave-cycle, average Q, change of cross-section of impounding structures. It will probably be that in attempting to find the point at which the waves build up, the effect of several of these variables can be found in addition. The channel used this year is the same channel that Lieutenant Killian used, except that the effect of this backwater will be eliminated. Appropriate weirs are at the discharge-end of the channel.

"Surges and oscillatory waves in open channels" by R. G. MacDonnell--This thesis will be limited to a consideration of the effect of ocean-tides on tidal canals and rivers. During the past five years there have been two theses on this general subject by graduate students at the University of California.

The first of these theses, written in 1932 by J. L. Green and R. E. Smyser, Jr., is entitled "River tides," an application of Airy's theory to the Sacramento River. In this investigation a simplified application of Airy's theory was used to compute the curves for the Sacramento River, and the curves thus obtained were compared with measured data. Reasonable agreement was obtained although certain small but consistent variations were noted.

The other previous thesis, written in 1936 by R. R. Arnold, is entitled "Application of the theory of Col. E. I. Brown for flow in tidal canals and estuaries." The purpose of this investigation was to apply the theory of Col. Brown, as given in "Flow of water in tidal canals" [Proc. Amer. Soc. Civ. Eng., December 1930] and in an unpublished paper, "Flow in tidal rivers," to experimental data in order to test the reliability of the theory. The experimental data were obtained by tidal measurements in a small channel. In this investigation general agreement of computed and measured data was again obtained, and the reflected negative wave-theory as expounded by Col. Brown was found to be correct. However, it was not possible to ascertain what degree of agreement can be expected between Col. Brown's theory and more precise observations, because of certain factors inherent in the methods of observation and equipment used. Interference from bends in the channel and the presence of a large number of reflected waves made very precise observations difficult to obtain.

The investigation to be conducted this year (1937-38) is a continuation and expansion of the work done by Arnold. Observations will be taken in a 3-inch by 9-inch channel, 200 feet long, made of 1/16-inch sheet-metal. A tidal range of about 0.08 foot will be used. An attempt will be made to obtain only the desired number of reflections by the use of friction-plates inserted into the water from above. Heights and velocities along the length of the channel will be measured to obtain the tidal curves.

For flow as in a tidal canal the channel will be horizontal and the data will be taken using several depths. For flow as in tidal rivers the conditions will be varied by varying the slope and discharge.

The principal effort of this year's investigation will be to obtain a complete and reliable set of data for a number of different conditions, and to make a comparison of these measured data with those computed from the theories of Col. Brown.

"Surging in harbors" by C. R. Broshous--Bodies of water, such as lakes, seas, canals, harbors, bays, and sounds all have natural periods of oscillation, and when these oscillations occur they are known as surges or seiches. When a disturbing force has a period which is the same or an integral multiple of this natural period, a condition of resonance is said to exist and the amplitude builds up to such a point that the energy dissipated in friction equals the rate at which it is supplied.

A seiche being a stationary oscillation can be considered as made up of two waves travelling in opposite directions, and so displaced that the height of the water-surface at the nodes will always be zero. Considering this theory the following formula for the longest period of free oscillation in a closed basin of length L has been developed: $T = 2L/\sqrt{GD}$, in which T = period of oscillation in seconds, L = length of closed basin in feet, G = acceleration of gravity in feet, and D = constant depth in feet, and the harmonics or periods of binodal, trinodal seiches, etc., are $T = 2L/K\sqrt{GD}$, where T = period in seconds, L = length of basin in feet, G = acceleration of gravity in feet, D = constant depth in feet, and K = 2, 3, 4, 5, - - - - -. In a basin of constant depth but with one end open to a larger body the water, the water-elevation at the open end is held constant and the conditions are the same as a closed basin of twice its length, that is, $T = 4L/\sqrt{GD}$, etc. Further along in the theory the formula $T = 2^{1}\, dx/0\sqrt{GDx}$ has been developed for seiches in closed basins of variable depth, depth varying along the length of the basin but remaining approximately constant through the width.

Other equations are developed for the theory taking into consideration variable depth through the width of the basin and other variable factors.

The object of the project will be first to construct small laboratory-basins of various sizes, depths, etc., and to supply these basins with disturbances of varying periods, using a plunger-mechanism, causing oscillations, surges, or seiches. By taking sufficient data it is intended to either prove or disapprove the theory as stated above.

Should the theory be proven to a satisfactory degree, it is intended to further apply it to available data of actual lakes, harbors, bays, and sounds, if and where it may be obtained.

Iowa Institute of Hydraulic Research

"The hydraulic jump in trapezoidal channels" by P. S. Hsing--The whole investigation is described in the thesis "The hydraulic jump in trapezoidal channels" by P. S. Hsing, on file in the library of the State University of Iowa. An article bearing the same name, by C. J. Posey and P. S. Hsing, is scheduled to appear in Engineering News-Record.

The tests were run in 20-foot Masonite channels having bottom widths of 1.250, 0.551, and 0.235 feet, and side slopes of 1:2, 1:1, and 2:1. More than 200 test-runs were made, depths varying from 0.033 to 0.852 foot.

While the water-surface before the jump was level transversely, transverse profiles through the jump were not level. They are described in detail in the thesis, and briefly in the article. With the higher jumps unstable dissymmetry in the flow-pattern was observed.

The depths before and after the jump were found to agree with those computed according to the law of momentum.

The jump was longer than for similar heights in rectangular channels. Nearly all of the lengths measured fell with the extremes given by the formulas $L = 5d_2 \left\{1 + 3\,[(w_2 - w_1)/w_1]^{0.7}\right\}$ and $L = 5d_2 \left\{1 + 6\,[(w_2 - w_1)/w_1]^{0.3}\right\}$ with mean values at $L = 5d_2 (1 + 4\sqrt{w_2 - w_1/w_1})$, where L is length of jump along center-line of channel, d_2 is the depth before the jump, and w_1 and w_2 are the widths of the water-surface before and after the jump, respectively.

Horton Hydraulic and Hydrologic Laboratory

"Flood-wave experiments" by Robert E. Horton--Some years ago an extensive series of flood-wave experiments in an experimental canal was carried out. The results have been quite completely analyzed but no attempt has been made to draw any conclusions from them, but it is hoped that some of these results can be gotten in shape to present to the Special Committee on Flood-Waves as a special report. This material is directly in line with work being done now and it is hoped that it can be taken up before very long.

An experimental canal 120 feet long was used with recording gages 20 feet apart, operated through a common draw-bar, so as to make the records absolutely simultaneous. A series of waves was started down from the head of the canal by means of a suitable control and in this way records were obtained of change of form as well as velocity. However, considerable difficulty was found in recording correctly the wave-form as it passed the gage.

There are perhaps about 200 of these experiments. Probably a description of the apparatus, which is already written, and at least the best of the experiments, in the form of accurate graphs but without analysis, will soon be published so as to make them a matter of record and available for general use.

Columbia University

"The hydraulic jump in sloped channels" by B. A. Bakhmeteff and A. E. Matzke--This paper was published in the Transactions of the American Society of Mechanical Engineers for February 1938 and was presented at the spring meeting of the American Society of Mechanical Engineers at Los Angeles, California, March 23-25, 1938.

Carnegie Institute of Technology

Work is being done on traveling waves in a trapezoidal channel. A thesis will be published on the subject about June 1, 1938.

The following are notes on work relating to flood-waves being done in Russia.

Scientific Research Institute of Hydrotechnics, Leningrad

Exchange of hydrotechnical research results, U.S.S.R.: Hydroelectric Laboratory (under direction J. B. Egiazaroff), Bulletin No. 5, 1935--Paper No. 6 on "Theoretical and experimental investigations of negative waves" is a study of the influence of friction and slope upon the propagation and shape of negative waves. Negative waves were experimented upon in flumes 30 meters long with depth of water equal to 0.3 meter. Observations were made by means of 14 self-recording devices, six current-meters, and six electric contacts. This work represents an analysis of investigations carried out in 1932 and in 1933 in a flume 30 meters long, and a continuation of experiments conducted in 1934 in a 75-meter flume. Theoretical analysis of the results permitted to estimate the influence of friction and slope (question not investigated up to this time). Final conclusions have not, however, been reached.

On the basis of these tests, Professor J. B. Egiazaroff developed a number of formulas which can be used for computation of wave-shapes and of distribution of discharges along the wave. These formulas take into account the influence of friction and slope, but must be regarded as the first approximation of the solution of the problem.

The results of tests under way and theoretical conclusions, regarding cases where the effect of friction may be neglected, have been published by Professor J. B. Egiazaroff (Trans. Sci. Res. Inst. of Amelioration, v. 23]. Some results of tests and conclusions regarding experimental work carried out lately have been expounded by Professor Egiazaroff in Chapter 3 of his book "Hydroelectric power-plants," 1934 (3rd ed.), and in the report to the XVIth International Navigation Congress in 1935 [the latter is to be published also in Trans. Sci. Inst. Hydrotechnics, Leningrad].

Paper No. 7 on "Experimental investigation of positive waves" is an experimental study of the shapes and velocities of propagation of positive waves at various rates of flow-increase. Experiments were made in the 75-meter flume, each experiment being recorded automatically by 51 pens. The data obtained, together with results of other tests, will constitute a base for the development of methods for computation of positive waves in lower pools of power-plants.

Paper No. 8 on "Development of apparatus for direct recording of water-level fluctuations on sensitive paper" describes an apparatus--invented, designed, and constructed--by means of which photo-diagrams can be obtained. Its main feature is the absence of parts directly connected with the stream, obstructing the flow.

In the Division of Special Field-Investigations (direction A. T. Potapov, Research Engineer), paper No. 95 by M. G. Krukovsky, Senior Research Engineer, on "Study of water-level fluctuations on the White Sea Canal during lockages", results from a field-study to determine the dimensions of negative and positive waves occurring during lockages and analyzes causes tending to increase the amplitude of fluctuations. Water-level fluctuations were observed by means of several ordinary gages and two self-recording devices at one reach of canal having regular prismatic bed and at two reaches of irregular shape. Twenty tests have been made. Comparison of measured values with those calculated from Lagrange and Boussinesq formula (velocity of propagation) and from Professor J. B. Egiazaroff's formulas (height of wave) gave satisfactory results.

Paper No. 99 by A. J. Schwarz, Senior Research Engineer, on "Experimental investigations of hydraulic conditions at structures of the Niva II Project" is a field-study of the hydraulic conditions at steady and unsteady flow at various structures of the Niva II Water-Power Plant. Measurements and observations were made at the intake and at diversion-canal. The work is completed. Comparison of observed data with those obtained by computation had shown satisfactory results. For instance, heights of waves and their celerities of propagation calculated by means of Professor J. B. Egiazaroff's formulas differed from directly measured values not more than 18 per cent.

In Bulletin No. 5 (13), 1936, paper No. 302 by V. T. Shilov, Engineer, on "Investigations of unsteady flow in the diversion-canal of the Niva II Hydroelectric Plant" reports on a field-study to determine if the free-board margins in the canal-embankments will be sufficient in case of partial or total shut-downs of the plant and to check theoretical formulas used for analysis of wave-motion in canals. Investigations were made with one turbine operating all the time under fairly constant load (about 10,000 kw), while the other unit worked on a rheostat and was used for creating sudden increases and decreases of load. In all, eight tests have been made at two water-levels in the canal (113.5 and 115.0 meters). Four tests were made with a throw-off of 10,000 kilowatts, one test with 12,500-kilowatt throw-off, two tests with 15,000-kilowatt throw-off, and one test with 10,000-kilowatt sudden increase (negative wave in channel). Observations of water-levels in the canals (4.5 km long) were made simultaneously at seven gaging points every 30 seconds; three of these points were provided with recording gages, while four others had only common measuring rods. The maximum height of positive wave at the end of the canal reached 54 cm (at the water-level elevation 113.5 meters and with a 15,000-kw throw-off load). The velocity of wave-propagation in the canal was equal to about five or six meters per second. All field-data are now being arranged and analyzed, which work was to be completed the first part of 1936.

Paper No. 303 by M. J. Krukowsky, Senior Research Engineer, with Professor A. N. Rahkmanov as consultant, on "Determination of the velocity of wave-propagation under ice-cover in lower pool of the Svir Hydroelectric Plant" is another field-study. The propagation of the wave was recorded at five gaging points installed for that purpose in the lower pool of the Svir Hydroelectric Plant to a distance of about 16 km. The character and behavior of the ice-cover were also observed.

Upon a special agreement with the administration of the Plant and with the Lenenergo, the operation of the Plant during the period allotted for tests was planned as follows: During the day the turbines were to operate with the flow of 700 cubic meters per second; during the night with 250 or 300 cubic meters per second; the change in load was to be made at 8^h and 23^h and was to be effected in as short a time as possible. Experinence has shown that the time required to effect the change in load ranges from 20 minutes to one hour. During the whole period of tests (from 22^h April 9 to 2^h April 12) observations of water-levels were made at all gaging points at one-minute intervals. The time was marked by means of synchronized watches. The amounts of flow were computed from wattmeter-records and head fluctuations (water-levels in upper and lower pools being constantly measured).

Water-level records permitted determination of the moments when wave-crests passed each of the gaging points. It may be mentioned here that perceptible waves occurred at each change in the amount of flow, whether caused by a considerable change in magnitude and gradual time-change due to load-regulation or by a small but instantaneous change effected to correct the frequency of the current. A series of computations of the velocity of wave-propagation were then carried out for different reaches of the river. For positive waves 35 computations were made and for negative waves 12 computations. Instantaneous profiles of water-surface for each one-half or one hour were also plotted and the check of formulas giving wave-heights as a function of the change in the flow was made.

The following conclusions have been made on the basis of the above-described investigations:

(1) Measured velocities of wave-propagation are close to the theoretical values computed from Lagrange's formula. Points obtained by actual observations are, of course, scattered about the curve, which, however, is due to inevitable errors of measurements. It may be concluded, therefore, that the presence of ice-cover, which according to original expectations should have increased the velocity of wave-propagation, did not in fact produce such an effect. This circumstance permits the generalization that under conditions similar to those of the tests in a broad river-channel and at the end of winter, the presence of ice-cover does not have any perceptible influence upon the velocity of wave-propagation.

(2) It has been observed that negative waves have lower velocities of propagation than positive waves.

(3) The shapes of water-surface coincide entirely with those obtained theoretically from differential equations of unsteady flow.

Paper No. 304 is on "Wind-waves in large pools of hydrotechnical structures" and describes development of methods of the analysis of wave-formation in large pools of hydraulic structures, of wave-transformation taking place on shoals, and of wave-action against structures and banks. The methods thus worked out will be used for computations of the elements of wind-waves in the upper pool of the Perm Hydroelectric Plant on the Kama River. Data used in this work were mainly those accumulated during many field-observations of wind-waves on the Onega, Wyg, and other lakes, made in 1921 and 1922 under the direction of V. G. Andreanov.

The program of the work is as follows:

(1) Preliminary determination of the elements of wind-waves for the upper pool of the Perm Hydroelectric Plant on the basis of theoretical and empirical formulas.

(2) Systematization of available theoretical and empirical studies of wind-waves.

(3) Arrangement and analysis of wave-studies carried out during the construction of the White Sea Canal.

(4) Checking of the existing and establishing of new empirical formulas, expressing the interrelations of various elements of the wave and of external factors influencing the latter, mainly on the basis of observations made on the White Sea Canal.

(5) Establishing final data for the computation of wave-elements for the case of the Perm Hydroelectric Plant.

Three points of the above program have been carried out as planned in 1935 and point (4) is just started. The work belonging to the first point of the program consisted in determination of elements of the wave for two lake-like reaches of the upper pool, one of which is located directly above the structures (having an area of 50 square km), the other in the region where the rivers Obva, Inva, and Kowa flow into the Kama River (area of this reach is about 400 square km). The wave-height for both these reaches has been determined as equal to 1.60 meters; the wave-length for the first reach was 20 meters and for the second reach 15 meters. The height of the reflected wave at the concrete dam has been found to be equal to 0.94 meter above the plane of calm water; the maximum value of the wave-pressure at that plane was 1.24 tons per square meter. The height of the wave breaking down on the slope of the earth-dam has been determined as equal to 2.0 meters and the maximum pressure of that broken wave 4.3 tons per square meter.

It was recommended on the basis of these data to increase by 0.5 freeboard selected for the earth-dam and to strengthen the revetment of the dam-slope. It was further recommended to build harbors at the reach near the mouth of the above-mentioned tributaries in order that ships might find refuge there during storms.

The work belonging to the second point of the program consisted in accumulation of data from technical literature (from 22 Russian and four foreign sources) concerning theoretical and empirical investigations of waves, with the purpose of using these data in the work denoted under point (4) in the program. A short report summarizing the data accumulated has been compiled. The work belonging to the third point of the program consisted in arrangement of field-data obtained at the Onega, Vyg, and other lakes in 1931 and 1932. A detailed report of that work was compiled containing about 250 pages of text and tables and 45 drawings. These data comprise the elements of wind-waves forming in deep water, the transformation of waves on shoals, and the phenomena of wave breaking at the shore line. They have been obtained under various physical and geographical conditions on five lakes, for wind-velocities from four to more than 20 meters per second, for the range of wind-action from 0.4 km to 30 km, and for depth from 0 to 17 meters.

The number of measurements and the amplitude of their values are tabulated below.

Elements of the wave	Number of observations	Amplitude of measurements
Height of wave, h, in meters	2246	0.05 - 3.20
Rise of wave-crest above the middle plane, a, in meters	2246	0.05 - 1.90
Length of wave, L, in meters	226	3.8 -23.6
Period of wave, T, in seconds	2564	1.6 - 5.7
Velocity of propagation, v, in meters per second	226	1.4 - 6.9
Ratio of height to length	226	0.024- 0.1
Ratio of depth of water to length of wave	226	0.05 - 2.18

In addition to these, data were obtained concerning the action of waves upon the shores, the change of the direction of wave-propagation with the approach to shores, and the wind and water-levels regime during storm weather.

The data obtained represent a sufficiently reliable basis for the development of practical formulas expressing the relations between various wave-elements and external factors upon which these elements depend (velocity and duration of wind, range of wind-action). Such formulas would be applicable, of course, for conditions existing on inland bodies of water, such as lakes and impounded rivers.

The work belonging to the fourth point of the program consisted in an analysis of existing formulas and in developing new ones for the following items:

(1) The height and length of wave, as depending upon the range of wind-action and upon the velocity and duration of wind.

(2) The transformation of waves while passing through shallow places.

(3) The determination of depths at which a wave is broken (depending also upon the height of wave and other factors).

It may be noted that the Stevenson's formula commonly used in engineering practice (deduced for conditions of sea-bays) and containing an expression of wave-height in function of the range of wind-action, gives values which are below actual results (this refers to lakes and artificial reservoirs). That formula, therefore, must be replaced by a new one, giving more accurate results.

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APPENDIX D--RAIN WAVE-TRAINS

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Introduction--Water is sometimes observed to flow in a shallow channel not as uniform flow but as a train of uniformly spaced, traveling, standing waves, sometimes called roll-waves. The hydraulics of such wave-trains are not well determined. Similar trains of waves may sometimes be observed on smooth slopes as a direct result of intense rain. In this case the volume of flow is not uniform but increases progressively down-slope. Such a series of waves resulting from rain may be designated a rain wave-train. The author has carefully observed (but not measured) such rain wave-trains on two occasions, both in intense rains, one on the concrete slope of Loudenville Reservoir, Albany, New York, on a slope of 1 1/2 on 1. The other was on a moderately sloped paved alley in Richmond, Virginia. Rain wave-trains may, therefore, occur on either moderate or steep slopes. The spacing of the waves was apparently about three feet in the first and six feet in the second instance, and the velocity was much higher on the sleep slope. In both cases the waves appeared to remain nearly uniformly spaced from the head to the foot of the slope although their volumes increased proceeding down-slope. The author has not observed and no one seems to have published an account of rain wave-trains on natural ground-surfaces.

It appears that rain wave-trains may be productive of greatly increased soil-erosion as compared with overland flow without waves. It is probable that rain wave-trains account for some otherwise obscure phenomena not uncommon in cloudburst-floods. These include instances of the tearing up of the sod in strips down a hill-slope and the origin of mud-flows. These phenomena will be more fully treated in another paper [see 1 of "References" at end of paper]. Most persons think of mud-flows as originating in stream-channels. There is evidence that debris carried in mud-flows, such as fences, livestock, and even large boulders, are derived not from the stream-channels but from adjacent hillsides. It is difficult to account for this on the basis of overland flow without waves, since the surface-depth on steep slopes where this phenomenon occurs is rarely more than one inch even in the most intense rains. For overland flow without waves, surface-runoff begins gradually and increases at a more or less uniform rate. Debris brought down by a stream, whether derived from the stream-channel or from the adjacent hillsides, would therefore be expected to come not at or near the front of the rise but later on and in increasing volume. The explanation usually given is that the first debris obstructs the flow and allows more debris to accumulate until a mud- and debris-front is built up in the stream-channel. Suppose, on the other hand, that debris from the hill-slopes begins to enter the channel abruptly and in large volumes early in the stream-rise--all the subsequent phenomena of the mud-flow would be readily understandable. The observed phenomena seem to require an abrupt change in the type of overland flow at about the time the overland flow-profile becomes stable and surface-runoff becomes noticeably intense. This change in type of overland flow must be such that the depth and erosive power of overland flow is greatly augmented. These results would follow from an abrupt change of overland flow from the quasi-steady type to the wave-type. The expression "quasi-steady flow" may be used to describe either overland flow or channel-flow increasing uniformly in volume proceeding downstream or down-slope and flowing without abrupt changes of depth or velocity.

In the following paragraphs an attempt is made to determine the conditions under which rain wave-trains may form on steep slopes.

Conditions of formation of rain wave-trains--Harold Jeffreys [2] has studied mathematically the conditions of occurrence of wave-trains in channels with uniform flow. While some of his hydraulic assumptions appear faulty, these probably do not affect the particular results here cited. Jeffreys concludes that instability which may give rise to a wave-train with uniform flow in the channel will result if the relations of depth, slope, and roughness are such that the velocity for steady flow becomes $v \geqq 2\sqrt{g\Delta}$, where Δ is the depth in feet, v is the velocity in feet per second, and g is the acceleration of gravity.

It is well known that a standing-wave may form at a change of channel-section and that a bore or traveling standing-wave may occur as the result of an abrupt influx of water to a channel when the velocity for uniform flow attains Belanger's critical value $v_c = \sqrt{g(A/W)}$, A being the cross-section and W the width of the channel. For a wide channel this reduces to $v_c = \sqrt{g\Delta}$. It appears from Jeffreys' result that the occurrence of a wave-train in a channel requires twice as great an initial velocity as that required for a bore. This, if true, accounts in part for the infrequent occurrence of wave-trains in streams. On the basis of Jeffreys' result, the ratio of velocity to depth for the occurrence of wave-trains must be $v/\Delta = 2\sqrt{g/\Delta}$. In natural stream-channels the slope, depth, and roughness are usually such that requisitely high values of v/Δ are seldom attained within the natural range of variation of Δ.

It can readily be shown that for quasi-steady overland flow resulting from rain, the profile of the overland flow for turbulent flow in terms of distance x, measured down-slope from the divide, is characterized by the following equations [3]. The equation of the profile of overland flow is

$$\Delta_x = (1/12)\ [(\sigma/K_S)(x/\ell_0)]^{3/5} \qquad (1)$$

The mean velocity of overland flow at the point x is

$$v_x = 0.0177\ (S^{0.3}/n^{0.6})\ (\sigma x)^{0.4} \qquad (2)$$

and the discharge-rate, in inches depth per hour, is

$$q_S = (1020/n\ell_0)\ (12\Delta)^{5/3}. \qquad (3)$$

In (1) the value of K_S is $K_S = 1020\sqrt{S}/n\ell_0$, where Δ = depth of sheet flow at x in feet, x = distance in feet from crest of slope, ℓ_0 = length of slope in feet, S = slope, n = coefficient of roughness in the Manning formula, $\sigma = (i - f)$ where i is the rain-intensity in inches per hour, and f is the infiltration-capacity in inches per hour.

Dividing (2) by (1) gives, for quasi-steady overland flow

$$v/\Delta = (1.486/12^{2/3}n)\,(\sigma x/K_S\ell_0)^{1/5}. \tag{4}$$

This shows that the ratio v/Δ increases slowly as the distance x, measured down-slope, increases. Slopes on steep areas where cloudburst-floods produce great erosional damage are in general relatively short. A rain wave-train must, therefore, form at not too great a distance from the crest of the slope in order to produce cloudburst-flood effects. The distance x_c from the crest of a slope at which a rain wave-train may form can be obtained by equating the velocity of overland flow, as given by (2), with the critical velocity. Inasmuch as there is, as subsequently appears, some question as to whether the condition precedent for wave-trains is a velocity $2\sqrt{g\Delta}$, as given by Jeffreys, or $\sqrt{g\Delta}$, in accordance with the theory of bores, determinations of the value of x_c or distance from the crest of slope at which rain wave-trains may form, have been computed on both bases. On Jeffreys' basis, with $v_c = 2\sqrt{g\Delta}$,

$$v_x = 0.0177\,(S^{0.3}/n^{0.6})\,(\sigma x)^{0.4} = 2\sqrt{g/12}\,(\sigma x/K_S\ell_0)^{0.3}$$

Solving for x

$$0.00885\sqrt{12/g}\,(\sigma x)^{0.1}\,(K_S\ell_0)^{0.3}\,S^{0.3}/n^{0.5} = 1$$

$$x = (23.1487)^{10}\,(1/\sigma)\,(n^9/S^{4.5}) \tag{5}$$

or

$$x = 44{,}177{,}000{,}000{,}000\;n^9/\sigma S^{4.5} \tag{6}$$

On the basis of Belanger's critical velocity

$$v_c = \sqrt{g\Delta}$$

$$v_x = 0.0177\,(S^{0.3}/n^{0.6})\,(\sigma x)^{0.4} = \sqrt{g/12}\,(\sigma x/K_S\ell_0)^{0.3}$$

Solving for x

$$0.0177\,\sqrt{12/g}\,(\sigma x)^{0.1}\,(K_S\ell_0)^{0.3}\,S^{0.3}/n^{0.6} = 1$$

$$x = (11.58)^{10}\,(1/\sigma)\,(n^9/S^{4.5}) \tag{7}$$

$$= 43{,}360{,}000{,}000\;n^9/\sigma S^{4.5} \tag{8}$$

Computations of x_c have been made for a rough surface, with $\sigma = 1.0$ and $n = 0.050$, with results as given in Table 1. This shows at once that for a rain wave-train to form close to the crest of the slope on a natural ground-survace, the slope must be steep. Thirty per cent or more is required on the bore-theory, whereas on Jeffreys' basis a slope of 100 per cent or more would be required. Similar computations for a smooth slope, on Jeffreys' basis, show that with n = 0.010, a rain wave-train to form close to the crest of the slope requires a slope of 20 per cent or more, whereas on the bore-basis, a rain wave-train may form close to the head of any slope exceeding one or two per cent. Inasmuch as the author has observed well-formed rain wave-trains in a paved alley at Richmond, Virginia, with a slope not exceeding one or two per cent, the question may fairly be raised whether Jeffreys' criterion is correct in the case of rain wave-trains.

Table 1--Values of x_c for $\sigma = 1.0$

S	Using $2\sqrt{g\Delta}$ x_c	Using $\sqrt{g\Delta}$ x_c
.10	2,610,000	2567.
.20	120,000	119.
.30	19,400	19.1
.40	5,340	5.25
.50	1,950	1.92
.60	862	0.844
.80	235	0.231
1.00	86	0.085

Crest-belt of laminar flow--Since laminar flow does not produce surface-erosion, the distance down-slope to which the belt of laminar flow extends is of interest. The minimum velocity of overland flow for which the flow can be laminar is given by the author's criterion [4]

$$v_H = \nu/5n^2\ \Delta^{2/3} \tag{9}$$

where ν is the kinematic viscosity of water in foot-pound-second units. Equating this with the velocity of overland flow, (2),

$$\nu/5n^2\ \Delta^{2/3} = 0.0177\ (S^{0.3}/n^{0.6})\ (\sigma x)^{0.4}$$

where Δ is the surface detention-depth in feet.

Substituting the value of Δ from (3)

$$\Delta_x = \frac{1}{12}\ \left(\frac{\sigma x}{K_S \ell_0}\right)^{3/5}$$

$$\nu = 5n^2 \left[\left(\frac{1}{12}\right)^{2/3} \left(\frac{\sigma x}{K_S \ell_0}\right)^{2/5} \left(0.0177\ \frac{S^{3/10}}{n^{3/5}}\ (\sigma x)^{2/5}\right)\right]$$

$$= \frac{0.0885\ n^{7/5}}{(12)^{2/3}}\ \frac{(\sigma x)^{4/5}\ S^{3/10}}{(K_S \ell_0)^{2/5}}$$

and solving for x

$$x = \frac{1}{\sigma}\left[\frac{\nu (12)^{2/3}\ (K_S \ell_0)^{2/5}}{(0.0885)\ (n)^{7/5}\ S^{3/10}}\right]^{5/4} = \frac{\sqrt{K_S}\ \sqrt{\ell_0}\,\nu}{\sigma n^{7/4}\ S^{3/8}} \left(\frac{12^{5/6}}{0.0885^{5/4}}\right)$$

Since $K_S = 1020\sqrt{S}/n\ell_0$

$$x = \frac{\sqrt{1020}\ S^{1/4}\ \sqrt{\ell_0}\,\nu}{\sqrt{n}\ \sqrt{\ell_0}\,\sigma n^{7/4}\ S^{3/8}} \left(\frac{12^{5/6}}{0.0885^{5/4}}\right) = \frac{524\ \nu}{\sigma n^{9/4}\ S^{1/8}} \tag{10}$$

For 60° F, $\nu = 0.0000121$, $x = 0.00634/\sigma n^{9/4}\ S^{1/8}$. With $\sigma = 1$ and $n = 0.05$,

$$x = 5.373/S^{1/8} \tag{11}$$

Table 2--Values from equation (11)

S	$S^{1/8}$	x_t	S	$S^{1/8}$	x_t
0.01	0.561	9.56	0.50	0.915	5.87
0.10	0.750	7.16	0.60	0.938	5.73
0.20	0.817	6.58	0.80	0.973	5.53
0.30	0.859	6.25	1.00	1.00	5.37
0.40	0.891	6.03			

The width of the belt of laminar flow at the head of a slope does not exceed 10 feet for $\sigma = 1.0$ for slopes ≧ one per cent and is less for larger values of σ.

Laminar flow does not inhibit the formation of rain wave-trains on rough slopes. On a smooth slope, with $n = 0.01$, the value of x_t is $5^{9/4}$ or 37.4 times as great as for $n = 0.05$, and a rain wave-train can not extend close to the head of the slope unless σ is large. On smooth slopes, where rain wave-trains are most likely to be observed, their occurrence is likely to be inhibited by laminar flow except for extremely high values of σ.

Erosional power of rain wave-trains--Figure 1 shows, schematically, (a) a profile of quasi-steady overland flow and (b) the same flow broken up with a wave-train.

Using DuBoys' formula for the tractive force per unit of boundary-surface with which a flowing stream is in contact, and introducing a factor C_b equal to the ratio of bottom to mean velocity, to correct for the fact that the tractive force is exerted at a rate $C_b v$--not at the mean velocity--gives

$$F = w_1 \Delta S/C_b \tag{12}$$

where F = tractive force in pounds per square foot, w = weight of water and suspended sediment in pounds per cubic foot, Δ = depth of overland flow or average height of wave in feet, S = slope. For turbulent flow, except on a sod-surface, C_b is commonly 0.30 to 0.60; its average is about 0.50. For uniform overland flow produced by rain on slopes, Δ is usually not over 0.1 foot.

It will be seen from (1) that the profile of overland flow for turbulent flow is a parabola

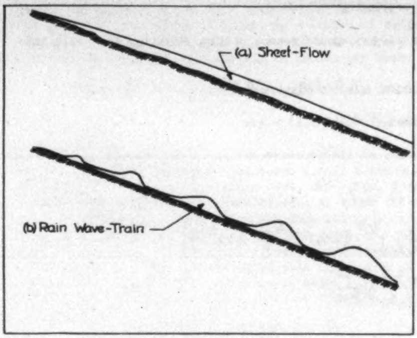

FIG. 1-SHEET-FLOW AND RAIN WAVE-TRAIN (Schematic)

with exponent 3/4 and that the average depth of surface-detention on the slope upstream from x is $3/8\Delta_x$. If, instead of uniform flow, a rain wave-train occurs with waves spaced at an interval z, then each succeeding wave sweeps into itself the accumulated surface-detention since the passage of the last preceding wave. When a wave-train occurs there will be ℓ_0/z waves on the slope, and the average volume per wave will be

$$V_w = (3/8)\Delta z \qquad (13)$$

In the rain wave-trains observed, the waves had base lengths of perhaps one-half foot at the head of the slope, increasing to one or one and one-half feet at the foot of the slope, the bases being longer for the waves on the flatter slopes. Calling the average base length per wave b, the average wave depth Δ_w will be

$$\Delta_w = (V_w/b) = (3/8)\ (\Delta z/b) \qquad (14)$$

In the cases observed, the average value of b was about (1/4)z to (1/6)z. Taking the average value of b from the top to the foot of the slope as z/5, gives the relation

$$\Delta_w/(3/8)\Delta = z/b = \text{say } 5.0 \qquad (15)$$

This is the ratio of the average wave-depth to the average depth of surface-detention on the slope, the latter for flow without a wave-train. Since tractive force is proportional to the depth, and erosional power is a function of tractive force and probably proportional to it, it appears that the erosional effect of a rain wave-train on the soil-surface may be of the order five times the erosional effect of the same volume of overland flow without waves. The difference is that between a steady moderate pull and a succession of sharp blows repeated at uniform intervals.

The preceding analysis throws light on other characteristics of rain wave-trains, particularly their apparently nearly constant velocity proceeding down the slope. The volume of the wave increases in proportion to the distance x travelled. If the velocity remains constant, obviously the wave-spacing z will remain constant. The velocity can, therefore, remain constant only in case the frictional resistance increases in proportion to the wave-volume. The wave-sections are apparently nearly parabolic and the average wave-depth about two-thirds the crest depth, so that the area of wave-section at a given point x is $A = (2/3)\Delta b$. From DuBoys' law, the tractive force per unit of base area is proportional to Δ, and the total tractive force per wave is proportional to Δb. The energy available to overcome resistance and produce velocity is proportional to A, and since $A = (2/3)\Delta b$, it is evident that the velocity may remain sensibly constant and the waves equally spaced as they travel down the slope.

Velocity of wave-trains--Considering wave-trains in channels, Jeffreys' analysis as well as observations of Vaughan Cornish on wave-trains in the Grünnbach conduit [5] indicate that where there is an appreciable antecedent depth of water, a wave-train virtually rides the antecedent flow at a velocity $\sqrt{g\Delta}$ in relation thereto. Consequently if the antecedent or initial velocity equals the critical instability-velocity given by Jeffreys, that is, $v_c = 2\sqrt{g\Delta}$, then the wave-velocity relative to the Earth is $w = 3\sqrt{g\Delta}$ and $w/v = 1.5$. Cornish remarks (p. 327): "I have never seen a stream with a uniform depth of more than four inches adopt spontaneously the intermittent flow in a series of roll-waves." Apparently there is a distinction between the channel wave-trains observed by Cornish and discussed by Jeffreys, and rain wave-trains here under consideration. In the case of the former there is an appreciable antecedent depth and the wave-train rides on the antecedent flow at a velocity $\sqrt{g\Delta}$.

Equating $v_c = 2\sqrt{g\Delta}$, as given by Jeffreys, with the velocity as given by the Manning formula

$$v = 2\sqrt{g\Delta} = 11.33\sqrt{\Delta} = (1.486/n)\,\Delta^{2/3}\sqrt{S}$$

$$\therefore\quad 11.33\sqrt{\Delta}\,n = 1.486\Delta^{2/3}\sqrt{S}$$

$$11.33n/1.486\sqrt{S} = \Delta^{1/6}$$

$$\Delta_c = (7.62)^6\,n^6/S^3 = \text{least depth at which a wave-train can occur.}$$

$$\Delta_c = 195{,}750\ n^6/S^3 \tag{16}$$

By a similar analysis for $v_c = \sqrt{g\Delta}$

$$\Delta_c = 3215\ n^6/S^3 \tag{17}$$

The value of Δ_c comes out extremely small. Computed values are given in Table 3.

Table 3--Computed values of Δ_c from equation (17)

S	S^3	n = 0.010 Δ_c	n = 0.050 Δ_c
0.10	0.001	0.00019570	3.059
0.25	0.0156	0.00001252	0.1961
0.50	0.125	0.000001564	0.02446
1.00	1.0	0.00000019570	0.003059
1.50	3.375	0.0000000579	0.000906
2.00	8.00	0.00000002446	0.0003824

The value of Δ_c varies inversely as S^3. It is to be kept in mind that slopes tributary to a stream are always steeper than the stream-channel. For some mountain streams in the State of New York the author found the slope-ratio (S_g/S_c) of the ground to the stream-channel to be of the order 5.0 [6].

The author's observations and preceding calculations indicate that in the case of rain wave-trains the antecedent depth is negligible and the succeeding waves sweep into their volume the antecedent flow and virtually glide down the slope, apparently at a velocity $\sqrt{g\Delta}$, as in the case of bores. All the facts point strongly to the conclusion that, while the conditions precedent for wave-trains in a channel of appreciable depth is probably $v_c = 2\sqrt{g\Delta}$, as given by Jeffreys, rain wave-trains may form with $v_c = \sqrt{g\Delta}$.

Effect of grass on rain wave-trains--As elsewhere shown by the author [7], a dense cover of grass subdivides overland flow in such a manner that the hydraulic radius and consequently the velocity of overland flow is sensibly constant not only at different depths at the same point but at different points down a stream-slope. Under these conditions the ratio v/Δ decreases with distance x, measured down-slope, and wave-trains are apparently impossible on well-sodded surfaces as long as the grass remains standing.

In case of cloudbursts, where the sod has been torn up, the author's observations indicate that the grass has first been beaten down flat on the ground. Since the retardation of overland flow by grass greatly increases the surface-detention, if the grass is beaten down, a considerable volume of surface-detention is more or less abruptly released. Under these conditions this may lead to the abrupt formation of a rain wave-train, with greatly increased intensity. Grass has the same effect on flow as greatly increased roughness. This cuts down the width of the belt of laminar flow. Other things permitting, rain wave-trains can form close to the crest of a sodded slope. Grass and roughness projections on the soil-surface tend, however, to throw off reflected waves and disperse a rain wave-train in much the same way that irregularities of a channel-section tend to disperse wave-trains in channels. This may account for the absence of observations of rain wave-trains on natural ground-slopes. Cornish has called attention to a similar effect in channel wave-trains, where the wave-trains would not be noticed unless looked for in the confused water surface-conditions.

Conclusions--It is interesting to note that recent activities in the study of soil-erosion have brought to light some new things in hydraulics, including the possibility of the occurrence of subdivided sheet-flow, with sensibly constant velocities on sod-covered slopes, and the possibility of occurrence of rain wave-trains on steep slopes. This paper is intended merely as an introduction to the subject of rain wave-trains, a subject which has apparently nowhere received attention hitherto in hydraulic and hydrologic literature.

The preceding study of rain wave-trains is based on mathematical analyses and ocular observations. It leads to results in accordance with the available data. The following conclusions should, however, be checked experimentally:

(1) The occurrence of rain wave-trains on natural ground surfaces is limited to steep slopes and high rain-intensities.

(2) The waves which form a train may have a tractive force or eroding power of the order of five times that of the same flow without waves.

(3) Standing grass inhibits the formation of rain wave-trains by preventing the instability-ratio v/Δ to attain sufficiently requisite values, but grass beaten down by rain or overland flow may accentuate wave-train erosion thereafter by releasing excess surface-detention.

(4) Wave-trains are shallow-flow phenomena and are inhibited in most natural channels by the fact that the depth is never sufficiently small to give the requisite value of v/Δ for instability.

(5) The waves in a rain wave-train apparently travel with a constant velocity $\sqrt{g\Delta}$ and remain spaced at nearly uniform intervals down the slope but increase in depth, volume, and width of base as they proceed down-slope.

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Voorheesville, New York

APPENDIX E--SEDDON'S AND FORCHHEIMER'S FORMULAS FOR CREST-VELOCITY OF FLOOD-WAVES SUBJECT TO CHANNEL-FRICTION CONTROL

Robert E. Horton

Introduction and synopsis--Among the least-understood phenomena of runoff are those involving the time of transit of the crest of a stream-rise from a given point of channel-inflow to an outlet. Practical importance attaches to this question in connection with flood-stage prediction. Time of transit is also involved where attempts are made to determine channel-outflow by the time-contour method. The validity of that method depends to a considerable extent on the accuracy with which time of transit can be determined.

There is a growing belief that the time of transit of a stream rise is governed by some type of wave-velocity and is not governed by the velocity of ordinary hydraulic flow--in other words, that the wave-crest velocity and the velocity at which water would flow at crest-depth, in accordance with, say, the Manning formula, are not the same, and that the former applies more nearly to the movement of stream-rises.

It seems obvious, although it does not appear to have been clearly pointed out before, that in natural stream-channels there is a range of conditions extending from those in which the wave-movement is almost completely subject to channel-friction control, to those in which the wave-movement is almost exclusively subject to gravity- or momentum-control, and channel-friction plays only a negligible role.

The conditions to which these two cases apply are apparently related through three factors: (1) Wave-length relative to wave-height; (2) wave-height relative to initial depth; (3) wave-height relative to hydraulic radius or, perhaps better, to the reciprocal of the hydraulic radius. Hydraulic considerations, as well as the author's experiments [see 1 of "References at end of paper], indicate clearly that the movement of channel-waves changes gradually from that subject to momentum-control to that subject to friction-control as the ratio of wave-length to wave-height increases. While apparently the type of wave-control, whether by momentum or friction, is also related to the last two factors, the nature of this relationship is as yet undetermined. As the flood-wave due to a stream-rise traverses the system of stream-channels, the crest-velocity may not only change but the laws governing the velocity may apparently change from those for momentum-control to those for friction-control or vice versa, depending on the initial depths, relative sizes and characteristics of channels, and tributary inflow for successive reaches traversed by the wave.

Complete analytical treatment of the movement of flood-waves subject to friction-control has been several times attempted without attaining results which were either simple or satisfactory. One reason is that the differential equations involved become complicated, difficult of solution, and simplifying assumptions must be made which more or less invalidate the results. No attempt will be made here to review this previous work.

There is another line of approach which involves only the use of the equation of continuity or the storage-equation in conjunction with a suitable slope-formula, such as that of Manning. This mode of approach has led to two expressions for wave-crest velocity--that of Seddon [2]

$$u = (1/w)\ (dq/dh) \qquad (1)$$

and that of Forchheimer [3]

$$u = (1/w)\ [(q_2 - q_1)/(h_2 - h_1)] \qquad (14)$$

The last equation has also been attributed to Boussinesq.

An expression which can readily be converted into Seddon's formula was published by Saint-Venant in 1871. Seddon appears to have been the first to put this expression directly into the form of a wave-velocity equation and to apply it to natural river flood-waves.

Forchheimer's "Hydraulics" gives an equation identical with that of Seddon and credits it to E. Lauda [9]. Seddon's first publication of his formula was in the Proceedings, American Society of Civil Engineers, December 1899.

Forchheimer does not give equation (14) in this form. Instead, he expresses q in terms of the Chezy formula for flow in a wide channel. Forchheimer's original formula applies only to such channels and is invalidated by the incorrectness of the Chezy formula. Forchheimer's formula does not appear in his treatese on "Hydraulics" [4]. Equation (14) is limited to wide rectangular channels but does not involve the Chezy formula. Equation (17)

$$u = (q_2 - q_1)/(A_2 - A_1) \qquad (17)$$

is more general in form and applies to all types of channels and to all stage-discharge relationships. Neither Seddon nor Forchheimer gives equation (17) explicitly although both state the equation of continuity in a form easily reducible to this equation.

L. K. Sherman [5] gives equation (14) but refers to Seddon's paper. Harold A. Thomas [5,6] gives a derivation of equation (14). It seems clear that equation (1), giving wave-velocity in terms of the differential ratio or slope of stage-discharge relationship curves, should be designated "Seddon's formula," and equation (14), based on a similar ratio but using finite differences, may be most appropriately called "Forchheimer's formula." The meanings and limitations of these equations and the relations between them do not appear to have been generally understood. Simple derivations are given for both equations, their limitations are pointed out and it is shown that, as above given, they apply primarily to wide channels in which the hydraulic radius is sensibly equal to the depth.

Most natural stream-channels have sections which are approximate parabolas of some form. A wave-velocity equation of the same type as that of Seddon is derived in the paper for a parabolic channel-section, applicable to any channel whose form of section can be represented by a simple power-function, $w = K_w h^c$, or for which the stage-discharge relation or rating curve can be represented by a simple parabolic or power function, $q = K_q h^m$. An equation of the same type as Forchheimer's is also developed applicable to all forms of parabolic channel-sections. This may conveniently be written in the form

$$u/v_2 = [1 - (h_1/h_2)^m]/[1 - (h_1/h_2)^{(m-2/3)}] \qquad (21)$$

All three equations, namely, Seddon's, Forchheimer's, and that of the author, indicate that if the initial depth is zero ($h_1 = 0$), as for a wave of the bore-type, the ratio (u/v_2), or the ratio of the wave-crest velocity to velocity of neutral flow at crest-depth, is unity. This evidently means that if uniform inflow is admitted to an initially dry stream-channel and continued long enough, a steady state will ultimately be attained at a given gage or cross-section and the velocity will be that corresponding to neutral hydraulic flow. The author's experiments on channel-waves (op. cit.) show that when water is admitted at a uniform rate to a uniform channel, initially empty, the toe of the wave travels downstream with a velocity which is related in some way, not yet determined, both to the velocity v_2 of stable flow and to $\sqrt{gd_2}$. Hence, Seddon's and Forchheimer's formulas, which give $u = v_2$, fail for this case.

In accordance with Forchheimer's formula, as the wave-height decreases relative to the initial depth h_1, the wave-velocity increases. The highest or limiting velocity as $h_1 \rightarrow h_2$ is not, however, 5/3, as indicated by the Forchheimer formula (or 3/2 if the Chezy formula is used instead of the Manning formula) but this limiting velocity varies with the form of channel cross-section, as reflected by the exponent m.

Channel-sections range from those which are triangular ($m = 2$) through various types of parabolic sections ($m = 2$ to $m = 1$). In natural stream-channels the form of channel-section generally changes from a nearly triangular section on headwater or first-order tributaries, to nearly the equivalent of a wide channel ($m = 1$) near the mouth of a large river. Consequently the wave-velocity for a given value of (h_1/h_2) also varies proceeding downstream.

A flood-wave may be considered as a mass of water having its center of gravity above the plane of equilibrium and which tends to flatten as the result of the action of gravity as it travels downstream. The tendency to flatten is resisted by channel-friction downstream from the crest but is augmented by channel-friction upstream from the crest. As a rule, the wave encounters increased depth h_1 of initial flow as it proceeds downstream. These conditions combine to increase the wave-velocity proceeding downstream. In view of the usual changes in form of channel-section and change in the ratio h_1 to h_2 as a wave travels downstream, it is not difficult to see why the simple Seddon and Forchheimer equations have not generally been found to give satisfactory results when applied to flood-wave movement in natural stream-channels.

Notation--Units are feet and seconds unless otherwise noted. Directions and velocities are positive in the direction of the initial flow. Subscripts 1 and 2 relate, respectively, to initial conditions antecedent to the wave and to conditions underneath the wave-crest.

h = stream-stage above plane of zero-discharge.
m = exponent in stage-discharge relation-formula.
n = Manning's coefficient of roughness.
p = wetted perimeter of cross-section.
q = discharge past a given section.
R = hydraulic radius of cross-section.
S = slope of water-surface.
u = velocity of wave-crest.
v = mean velocity by Manning formula for neutral flow at any given depth and slope.
k_q = coefficient in stage-discharge relation for a given reach or at a given cross-section.
k_w = coefficient in parabolic width-formula.
c = exponent in parabolic width-formula.

Seddon's formula--In 1900 James A. Seddon [2] published the formula

$$u = (1/w)\ (dq/dh) \tag{1}$$

for the velocity u or rate of downstream advance of the crest of a flood-wave. (dq/dh) is the rate of change of discharge with stream-stage. This equals the slope of the stage-discharge relation-curve (rating curve). Substantially the following proof, also another proof based on the idea of differential transfer of the water, are given in Seddon's paper. Referring to Figure 1, let $ab = d\ell$ be a short reach or element of length of stream-channel such that the wave-crest moves from a to b in the time dt. If q_2, h_2 and q_1, h_1 are average inflow- and outflow-rates and stages for this reach in the time dt, then from the storage-equation, $q_2\ dt = q_1\ dt + dS$, where $dS = [(dh_2 + dh_1)/2]\ w d\ell$ = change of storage.

The average change of stage in the reach is $dh = (dh_2 + dh_1)/2$, giving $(q_2 - q_1)\ dt = w \times dh \times d\ell$, where w = channel-surface width.

But $q_2 - q_1 = dq$, so that $dq \times dt = w\ dh\ d\ell$ and $(1/w)\ (dq/dh) = d\ell/dt$. But $(d\ell/dt)$ is a velocity, and if the crest moves from a to b in the time dt, $(d\ell/dt) = u$ = crest-velocity.

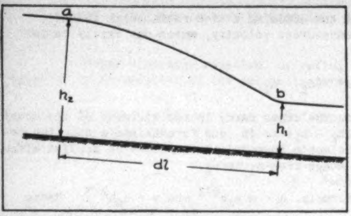

FIG.1-DERIVATION OF SEDDON'S FORMULA

From Manning's formula, for a wide rectangular channel,

$$q = w \quad (1.486/n)\ h^{5/3}\sqrt{S} = w\ k_m h^{5/3} \qquad (2)$$

where $k_m = (1.486/n)\sqrt{S}$. (3)

Also

$$(1/w)(dq/dh) = (5/3)(1.486/n)h^{2/3}\sqrt{S} = (5/3)v_2 \qquad (4)$$

where v_2 is the mean velocity of hydraulic flow at the crest-stage.

The Chezy formula leads to the result that crest-velocity does not exceed 1-1/2 times the velocity for steady flow. This result, often quoted, is incorrect. The more accurate Manning formula gives $u = (5/3)v$ for a wide channel.

Seddon formula for a rectangular channel--For a rectangular channel

$$q = k_m\ [wh/(w + 2h)]^{2/3}\ wh \qquad (5)$$

$$\begin{aligned}(1/w)\ (dq/dh) &= (d/dh)\ k_m w^{2/3}\ [h^{5/3}/(w + 2h)^{2/3}]\\ &= k_m w^{2/3}\ [(w + 2h)^{2/3}\ (5/3)h^{2/3} - (4/3)h^{5/3}/(w + 2h)^{1/3}]/(w + 2h)^{4/3}\\ &= k_m w^{2/3}\ [5/3(w + 2h)\ h^{2/3} - (4/3)h^{5/3}]/(w + 2h)^{5/3}\\ &= (k_m w^{2/3}/3)\ [(5p - 4h)\ h^{2/3}/p^{5/3}]\end{aligned}$$

But $v = k_m w^{2/3}\ h^{2/3}/p^{2/3}$

$$u/v_2 = (p^{2/3}/3)\ [(5p - 4h/p^{5/3}] = (5p - 4h)/3p \qquad (6)$$

If h is small relative to w, and hence relative to p, $(u/v_2) = 5/3$ as before.

Seddon formula for a general parabolic channel--Many channels are nearly parabolic in section. For a general parabola, $w = k_w h^c$, we have

$$R = h/(c + 1),\ A = h_w/(c + 1) = k_w h^{(c + 1)}/(c + 1),\ v = k_m\ [h/(c + 1)]^{2/3} \qquad (7)$$

$$q = Av = k_m k_w\ [h^{(5/3) + c}/(c + 1)^{5/3}] \qquad (8)$$

$$(dq/dh) = k_m k_w/(c + 1)^{5/3}\ [(2/3) + c + 1]\ h^{(2/3) + c} \qquad (9)$$

Let $k = k_m k_w$, then

$$u = (1/w)\ (dq/dh) = k/(c + 1)^{5/3}\ [(2/3) + c + 1]\ h^{2/3} \qquad (10)$$

$$v_2 = k[h_2^{2/3}/(c + 1)^{2/3}] \qquad (11)$$

$$u/v_2 = [(2/3) + c + 1]/(c + 1) \qquad (12)$$

If $c = 1/2$

$$u/v_2 = 13/9 = 1.444 \qquad (13)$$

For a common parabolic section, Seddon's wave-velocity is (13/9)v.

Proofs of his formula given by Seddon and others, and that given above, are neither rigorous nor wholly satisfactory because: (1) They neglect the effect of the initial flow and stage; (2) variation of slope is not taken into account although the slope often varies considerably in the vicinity of the crest, being less behind, and greater than the normal slope in front of the crest; (3) as the crest-profile is curved, dS is not identical with $[(dh_1 + dh_2)/2]\ wd\ell$ but is somewhat greater. Neither has the formula been tested by any adequately controlled experiments.

Forchheimer's formula--Using the Chezy formula and assuming a wide rectangular channel, Forchheimer [3] published in 1904 an equation for wave-crest velocity, which can easily be reduced to the form

$$u = (1/w)\ [(q_2 - q_1)/(h_2 - h_1)] \qquad (14)$$

This equation fails when h_1 and h_2 are equal but, on the other hand, in the vicinity of the crest of a long wave, $(q_2 - q_1)$ may be taken as dq, and $(h_2 - h_1)$ as dh, and Forchheimer's equation reduces to identity with Seddon's. Apparently Forchheimer's formula can therefore be applied either to a river flood-wave or to an abrupt change of stage from h_1 to h_2.

For a wide rectangular channel, by Manning's formula, $q = w\ k_m h^{5/3}$ and $v = k_m h^{2/3}$. Hence

$$u = (1/w)\ [(q_2 - q_1)/(h_2 - h_1)] = k_m\ (h_2^{5/3} - h_1^{5/3})/(h_2 - h_1)$$

and

$$u/v_2 = (h_2^{5/3} - h_1^{5/3})/h_2^{2/3}\ (h_2 - h_1)$$

Let $\rho = h_1/h_2$, then

$$u/v = (1 - \rho^{5/3})/(1 - \rho) = 1 - (h_1/h_2)^{5/3}/(1 - h_1/h_2) \qquad (15)$$

Generalized wave-velocity equation--The following equation is derived in the same way as that of Forchheimer excepting that it is based on the Manning formula, $v = (1.486/n)\ h^{2/3}\sqrt{S}$ and applies to any cross-section where the width is a simple power-function of the depth, $w = k_w h^c$, that is, to any type of parabolic cross-section ranging from a wide rectangle where $c = 0$, through parabolas with exponents $c = 0$ to $c = 1.0$, the latter corresponding to a triangular stream-section.

The section area is

$$A = \int_0^h w\,dh = [k_w/(c + 1)]\ h^{(c + 1)}$$

and the hydraulic radius is, nearly, (A/w), or $R = h/(c + 1)$; also $A = hw/(c + 1)$,

$$v = k_m R^{2/3} = k_m/(c + 1)^{2/3}\ h^{2/3} \qquad (16)$$

where k_m is as given by (3).

From the equation of continuity, neglecting changes of channel-storage due to channel-friction, for any particular section with $w = k_w h^c$, the mean depth is $h/(c + 1)$ and the change of channel-storage in any length $d\ell$ during the passage of the crest-stage is

$$A_1\ (u - v_1) = A_2\ (u - v_2)$$

$$A_1 u - A_1 v_1 = A_2 u - A_2 v_2$$

$$u = (A_2 v_2 - A_1 v_1)/(A_2 - A_1) = (q_2 - q_1)/(A_2 - A_1) \qquad (17)$$

For a parabolic section, $A = hw/(c + 1)$ and

$$u = (c + 1)\ (q_2 - q_1)/(h_2 w_2 - h_1 w_1) = (c + 1)\ (q_2 - q_1)/[k_w(w_2 h_2^{c + 1} - w_1 h_1^{c + 1})] \qquad (18)$$

but

$$q = k_w k_m\ h^{(5/3) + c}/(c + 1)^{5/3} = Av \qquad (19)$$

This gives

$$u = k_m/(c + 1)^{2/3} \times [(h_2^{(5/3) + c} - h_1^{(5/3) + c})/(w_2 h_2^{c + 1} - w_1 h_1^{c + 1})] \qquad (20)$$

For a wide rectangular section $c = 0$ and $w_2 = w_1$, (19) gives $u = (1/w)\ [(q_2 - q_1)/(h_2 - h_1)]$ the same as by (14). This in turn is identical with Seddon's formula for this case if $(q_2 - q_1) = dq$ and $(h_2 - h_1) = dh$.

Equation (20) may be used to compute u directly but it is convenient to express u as a ratio to v_2.

The stage-discharge relation, or rating curve, for many natural stream-channels can be expressed by an equation of the parabolic form, $q = k_q h^m$. From (17)

$$u = (q_2 - q_1)/(A_2 - A_1)$$

From (19)

$$q = k_w k_m h^m/(c + 1)^{5/3}$$

and since $A = hw/(c + 1)$

$$u = k_w k_m\ (h_2^m - h_1^m)/(c + 1)^{2/3}\ (h_2 w_2 - h_1 w_1)$$

But $w = k_w h^c$, therefore

$$u = [k_m/(c + 1)^{2/3}]\ [(h_2^m - h_1^m)/(h_2^{c+1} - h_1^{c+1})$$

Let $\rho = h_1/h_2$. Then, since $[m - (c + 1)] = 2/3$

$$u = [k_m/(c + 1)^{2/3}]\ [(1 - \rho^m)/(1 - \rho^{(c+1)})]\ h^{2/3}$$

But from (18)

$$v = [k_m/(c + 1)^{2/3}]\ h^{2/3}$$

and

$$u/v_2 = (1 - \rho^m)/(1 - \rho^{(m - 2/3)}) = [1 - (h_1/h_2)^m]/[1 - (h_1/h_2)^{(m - 2/3)}] \qquad (21)$$

This gives the wave-crest velocity-ratio in terms of the depths and the exponent in the stage-discharge equation. This and equation (17) are the simplest general forms of equations for crest-velocity of channel-waves subject to friction-control.

The reason for the failure of the Seddon and Forchheimer equations to give correct results for natural channels or narrow experimental channels is apparently due to the following facts:

(1) The ratio (u/v_2) is a function of the exponent m in the rating-curve equation, $q = k_q h^m$.

(2) The ratio (u/v_2) varies with the ratio (h_1/h_2) for the same crest-stage h_2. For example, for a wide channel, m = 2. If $h_2 = 16.0$ feet and $h_1 = 12$, $u/v = (1 - 9/16)/(1 - 3/4) = 7/4$. If, as before, $h_2 = 16.0$ feet but $h_1 = 4$ feet, $u/v = (1 - 1/16)/(1 - 1/4) = 5/4$, or for the same crest-stage the crest-velocity is 40 per cent greater for a 4-foot rise on a 12-foot initial stage than for a 12-foot rise on a 4-foot initial stage.

The values of m in the discharge equation, $q = k_q h^m$, and c in the width equation, $w = k_w h^c$, for various parabolic sections, are given in Table 1.

Table 1--Values of m and c

Section	c	m
Wide channel	0	5/3
Parabola	1/4	23/12
Parabola	1/3	2.0
Common parabola	1/2	13/6
Parabola	2/3	7/3
Triangle	1	8/3

Equation (21) gives $(u/v_2) = 1.0$ for $h_1 = 0$.

Figure 2 shows values of the ratio (u/v_2), or the ratio of the wave-crest velocity to the velocity of stable flow at crest-stage, for parabolic channel-sections of different types, in accordance with equation (21). The values of the ratio (u/v_2) start at unity for $(h_1/h_2) = 0$ for all types of channels and approach a limiting value as $h_1 \rightarrow h_2$. This limiting value is dependent on the form of channel cross-section and ranges from 4/3 for a triangular channel to 5/3 for a rectangular channel. As already noted, the ratio (u/v_2) = unity for $h_1 = 0$, or, for inflow to a dry channel, does not correctly represent the velocity of the front or toe of the wave.

Comparison of Seddon and generalized Forchheimer formulas--While equation (17), $u = (q_2 - q_1)/(A_2 - A_1)$ and Seddon's formula, $u = (1/w)\ (dq/dh)$, become identical if $(q_2 - q_1)$ and $(A_2 - A_1)$ are differentials, the equations do not give identical results except for

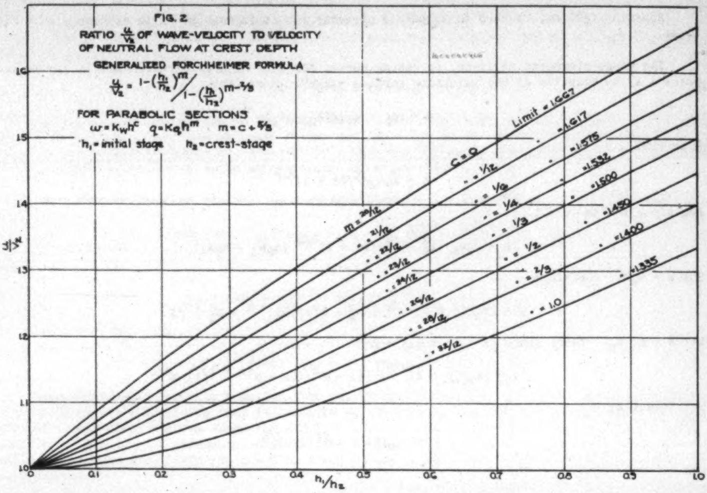

FIG. 2

RATIO $\frac{u}{v_2}$ OF WAVE-VELOCITY TO VELOCITY OF NEUTRAL FLOW AT CREST DEPTH

infinitesimal waves. This suggests at once that the Seddon formula really applies only to small displacements of the water-surface. The argument has been made by Seddon and others that any long wave, such as a river flood-wave, may be considered as the summation of a series of successive infinitesimal increments, and that the Seddon formula therefore applies to all flood-waves.

For ordinary river flood-waves where there is an initial depth h_1 which is different from the crest-depth h_2, the velocity by Seddon's formula is that given by the ratio (dq/dh) at the crest-stage, and Seddon's formula does not take into account the effect of the initial flow. Undoubtedly the initial stage h_1 has some effect on the wave-crest velocity, a marked effect for instantaneous increments, and an effect decreasing as the wave-length increases, and possibly becoming negligible for waves of such great length relative to their height as ordinary flood-waves. It seems quite certain that given a wave of say 20 feet crest-stage, or $h_2 = 20.0$, the crest-velocity would be different for initial stages $h_1 = 5$ feet, 10 feet, and 15 feet, respectively, whatever might be the wave-length. Seddon's formula, however, gives identical crest-velocities for all these cases. On the other hand, equation (17) takes into account the initial stage h_1 and not only gives different crest-velocities from those given by Seddon's formula where the ratio (h_1/h_2) is different from zero, but for a given value of h_2, equation (17) gives a different crest-velocity for each different value of the initial stage h_1. Taking, for illustration, a hypothetical case of a river-channel 100 feet wide, with uniform slope, for which the stage-discharge relation is, in accordance with the Manning formula, $q = 100.0\ h^{5/3}$, the crest-velocities given by the generalized Forchheimer formula, for $h_2 = 20.0$ feet and h_1 having different values, are shown in Table 2.

Table 2--Velocities in feet per second by equation (17), for a channel 100 feet wide, with $v = 1.0h^{2/3}$ and $q = 100h^{5/3}$, for $h_2 = 20.0$ and for various values of h_1

h_1 =	1	2	4	6	8	10	12	14	16	18	20
u =	7.70	8.07	8.58	9.11	9.61	10.10	10.56	11.00	11.44	11.88	12.28

$v = 7.37$. u_s (Seddon) = 12.28 feet per second

It will be seen from Table 2 that the generalized Forchheimer formula gives crest-velocities which range from the velocity of stable flow for steady inflow to an initially dry channel, or for $(h_1/h_2) = 0$, to the velocity given by the Seddon formula for $(h_1/h_2) = 1.0$, or for a slight displacement. The change from one of these velocities to the other as (h_1/h_2) increases, is not precisely uniform but, to a fair degree of approximation,

$$U_F = v_2 + (u_S - v_2)(h_1/h_2)$$

where u_F and u_S are the wave-crest velocities by the generalized Forchheimer formula and by the Seddon formula, respectively.

It is of interest to note that for a steady release of water into an initially dry channel, both the Seddon and generalized Forchheimer equations give $(u/v_2) = 1.0$, which is the velocity of stable flow at the depth h_2; in other words, it is the velocity for complete friction-control. The author's experiments and those of Bazin and Darcy show that the velocity of the wave-front under these conditions is not v_2 but is evidently a function both of v_2 and $\sqrt{gd_2}$, indicating that the velocity of the wave-front is subject in part to momentum-control and in part to friction-control. At a sufficient distance back from the wave-front, where the flow has become steady, the velocity is subject to friction-control and is given correctly by the Seddon and Forchheimer equations.

In general, the stage-discharge relation at a given cross-section of a stream-channel can be represented by the equation $q = K_q h_2^m$. Hence $(dq/dh_2) = mKh_2^{m-1}$ but the velocity of normal or stable flow is given by $v_2 = Kh_2^{m-1}$. Consequently the ratio of the wave-velocity to the velocity of stable flow is equal to the exponent m. For different types of parabolic sections this ranges from 1.33 to 1.67.

For an infinitesimal wave or a small displacement wave, or, in the limiting case, where $(d_1/d_2) = 1.0$, Seddon's formula gives the same wave-velocity as for inflow to an initially dry channel if the final stable depth and velocity, d_2 and v_2, are the same. The generalized Forchheimer formula is in form indeterminate for this case, since $(q_1 - q_2)/(A_1 - A_2) \rightarrow 0/0$ as $(q_2 - q_1)/(A_2 - A_1)$ approaches zero. The graph (Fig. 2) shows, however, that in this case the wave-velocities by the generalized Forchheimer formula are the same as those given by Seddon's formula. This result does not appear to be correct in the light of the author's experiments and those of Bazin and Darcy, which show that the velocity of an infinitesimal wave or a slight displacement wave is accurately given by $u = v_2 \sqrt{gd_2}$. Whether the Seddon and Forchheimer equations give correct wave-velocities for long waves under intermediate conditions remains to be determined.

Checking Seddon's and Forchheimer's equations--Table 3 shows the results obtained by the application of the Seddon and the generalized Forchheimer equations to some of the author's experiments [1]. In the case of these experiments, accurate stage-discharge relation curves were available for the experimental channel for each series of experiments, and the channel width was 5-5/8 inches, of rectangular cross-section. The wave-velocities by Seddon's formula were computed by determining the equation of the stage-discharge relation curve and finding therefrom the ratio (dq/dh). In general, the velocities by the Seddon and generalized Forchheimer equations are not materially different for the conditions of these experiments. Velocities by the Forchheimer equation are given for all of the experiments in the original paper [1]. The value of q_2 used in computing the velocities in Table 3 are those for stable flow after the wave has passed. It will be noted that with exception of one case, both equations give velocities much below the observed velocities. The observed velocities were, however, those of the toe of the wave in cases indicated by a star. The exception occurs in case of rectangular waves. For various reasons, pointed out in the original paper [1], the experiments on waves of initially rectangular profile are much less reliable than those on other forms of waves. Even in case of triangular waves used in these experiments, the ratio of wave-length to wave-height was relatively small compared with that occurring in case of natural flood-waves in river-channels. So far as the author is aware there are no laboratory-experiments on flood waves adequate to afford a reliable check on the validity of either the Seddon or Forchheimer equations, and it is evident that to check these equations adequately by laboratory-experiments requires an experimental channel of relatively great length. Pending the acquisition of experimental data for this purpose, it is desirable to undertake to determine the relation between actual flood-crest velocities in natural stream-channels and the crest-velocities as computed by the two formulas. This, however, involves practical difficulties owing to variations in channel-width, form of cross-section, variations in initial depth h_1, and owing also to the effect of lateral inflow. Furthermore, in order to apply either the Seddon or Forchheimer equation it is necessary to have available rating curves and average channel-widths representative of the stream-reaches within which the formula is to be tested. Adequate data of stream-widths are not usually available. It appears, however, that data have been obtained in certain cases adequate to permit a study to be made of the validity of the Seddon and Forchheimer equations. In particular:

(1) Flood-crest velocities in the Rio Grande River--See discussion in Water Bulletin 6, International Boundary Commission, United States and Mexico, State Department, Washington, D. C.,

Table 3--Wave-velocities by Seddon and generalized Forchheimer equations compared with observed wave-velocities of Horton's experiments

No. of Exp. (1)	Slope (2)	Seddon $u^{(c)}$ fps (3)	Forchheimer $u^{(c)}$ fps (4)	Observed u fps (5)	No. of Exp. (1)	Slope (2)	Seddon $u^{(c)}$ fps (3)	Forchheimer $u^{(c)}$ fps (4)	Observed u fps (5)
Instantaneous increase				(a)	Gradual increase				(a)
4	level	1.88	2.23	4.45	9	level	1.77	2.12	4.19
4	level	3.69	2.31	4.01	7	level	3.87	2.28	3.82
2	.00102	1.96	1.82	3.23	4	.00790	4.24	3.12	4.79
3	.00102	2.14	2.09	4.44	20		3.00	2.38	4.18
3	.00186	2.41	2.32	4.50	Ratio to obs'd		0.716	0.569	
5	.00392	3.35	2.92	4.66	Gradual decrease				(b)
9	.00736	4.63	4.64	5.58	8	level	1.24	1.60	4.50
2	.00790	4.02	4.00	4.95	8	level	2.71	3.05	4.05
32		3.32	3.11	4.71	4	.00790	3.84	4.37	5.50
Ratio to obs'd		0.704	0.660		20		2.35	2.73	4.52
Instantaneous decrease				(b)	Ratio to obs'd		0.520	0.604	
4	level	1.43	2.18	4.69	Rectangular				(b)
5	level	2.28	1.78	4.23	9	level	1.82	3.17	3.90
2	.00102	1.65	1.83	4.33	8	level	1.08	4.82	4.72
3	.00102	2.00	1.97	5.44	9	level	1.64	4.84	4.44
3	.00186	2.21	1.67	4.68	26		1.53	4.26	4.34
4	.00392	2.97	3.00	5.31	Ratio to obs'd		0.353	0.980	
10	.00736	3.93	4.36	5.73	Triangular				(a)
2	.00790	3.42	3.55	5.91	7	level	1.90	2.55	4.35
33		2.76	2.88	5.28	8	level	3.73	2.51	3.91
Ratio to obs'd		0.523	0.545		15		2.87	2.53	4.12
					Ratio to obs'd		0.697	0.614	

(a) Velocity of toe. (b) Velocity of crest. (c) Computed for stable depth.

1936; also Special flood report, flood of September and October 1932, Rio Grande River, International Boundary Commission, State Department, Washington, D. C., 1933.

(2) North Branch Platte River, flood of May-June, 1936--Data in manuscript form are available in the files of the United States Geological Survey, with excellent rating curves at numerous gaging stations, but additional data of stream-widths are needed.

(3) Delaware River--Data of flood-graphs, stream-widths, etc., are available in the author's files for a study of the applicability of the Seddon and Forchheimer formulas to flood-crest velocities in the Delaware River.

(4) In addition to the above it appears certain that there is a large amount of data on flood-crest velocities in the files of the United States Geological Survey and United States Weather Bureau which could be utilized for the purpose of testing the validity of the Seddon and Forchheimer equations but additional data of stream-widths would probably have to be obtained.

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APPENDIX F--A STUDY IN THE THEORY OF FLOOD-WAVES

Elmer E. Moots

Synopsis--The hydromechanic equations of a simple flood-wave in a wide rectangular channel are developed and discussed herein. The paper includes: (1) A restatement of the fundamental equations governing a simple flood-wave in a wide rectangular channel; (2) the development and solution of the equations of the roll-wave as a special case of the flood-wave; (3) a discussion of the order of occurrence of the three maxima of mean velocity, flow, and stage-height; (4) the determination of the velocity of a flood-wave as the rate of movement of a constant flow; (5) the determination of the velocity of a flood-wave with small amplitude; (6) the development and solution of the equations of the rising flood-wave.

The equation of continuity and motion--The equation of continuity of flow in a wide rectangular channel referred to the coordinate axes, h, x at a time t is

$$(\partial h/\partial t) + h(\partial u/\partial x) + u(\partial h/\partial x) = 0 \tag{1}$$

where u is the mean velocity of a transverse section and t represents the time. For the simple flood-wave the usual three Euler's equations of motion are reducible to the single equation

$$(\partial u/\partial t) + u(\partial u/\partial x) + g(\partial h/\partial x) = g(s - K\,u^2/h) \tag{2}$$

where g represents the acceleration of gravity, s is the slope of the channel-bed, and $K = (1/C^2)$ (C is Chezy's coefficient).

The roll-wave, unsteady flow--When a flood moves down a shallow mountainous stream or a dry creek, the front may be steeply inclined, at times a nearly vertical wall of water. The profile of this moving wave is apparently unchanging so long as the channel-conditions and the supply remain constant. A wave of this type also may be generated by the sudden release of water caused by the failure of a dam.

In the solution that follows the roll-wave is assumed to move along its course with a motion approximating that of a sliding solid body. This assumption, of course, is not true for the extreme frontal portion of the wave where there is turbulence; however, since the turbulent area is only a small part of the entire wave it is believed that the solution on the whole should represent closely the profile of the wave. Accordingly, for the roll-wave, $(\partial u/\partial x) = (\partial u/\partial t) = 0$ and the equations of continuity and motion become

$$(\partial h/\partial t) + u(\partial h/\partial x) = 0 \tag{3a}$$

$$(\partial h/\partial x) = (S - K\,u^2/h) \tag{3b}$$

Rearranging and understanding that $t = t_1$

$$dh = [(Sh - Ku^2)/h]\,dx$$

Integrating

$$h/S + Ku^2/S^2 \log (Ku^2 - Sh) = x + C_1 \tag{4}$$

Choosing $h = 0$ where $x = 0$ and substituting from Chezy's formula

$$u = C\sqrt{h_2 S} = \sqrt{h_2 S/K}$$

where h_2 is the depth at the crest of the wave, the constant of integration is

$$C_1 = h_2/S \log Sh_2$$

Solving for x, we have

$$x = (1/S)\,[h + h_2 \log (1 - h/h_2)] \tag{5}$$

A representative profile of the roll-wave obtained from equation (5) is shown in Figure 1.

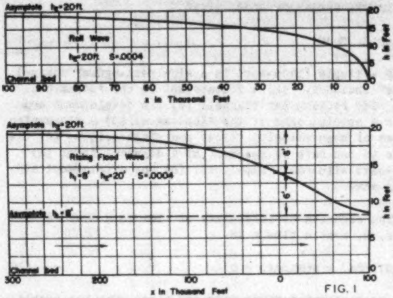

FIG. 1

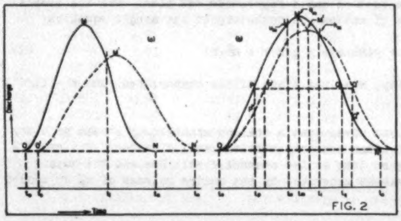

FIG. 2

Velocity of flood-waves

Time-discharge curves--As a simple flood-wave moves down a channel the discharge, q, per unit-width varies with both x and t.

$$q = F(x, t) \tag{6}$$

It follows that at a given station, X_1, we may plot the flow, q, as ordinates and the corresponding time, t, as abscissas for the duration of the flood. [In the presentation of this section as far as equation (10), substantial use has been made of a study compiled by M. Kleitz entitled "Note sur la theorie du movement non permanent des liquides et sur application a la propagation des crues des rivieres," Annales des Ponts et Chaussees, 2^o Semestre, pp. 133-196, 1877.]

Curves for two stations are shown in Figure 2-a. Either of the areas OMIO'O or N'NIM'N' represents the maximum storage possible between the stations for the given flood.

Order of occurrence of the three maxima of mean velocity, flow, and stage-height--The curves in Figure 2-b are similar to those in Figure 2-a, excepting that the two stations are separated by a differential distance, dx. Consider the flow, Pt_p, observed when the flood is rising, and an equal flow, Qt_q, observed when the flood is receding. The time-interval PP' is in general less than QQ', since the mean velocity u is greater when the river is rising than it is for an equal flow when falling. Since the areas OPIP'O and NQIQ'N are equal, the point of intersection of the two hydrographs will fall to the right of the two maxima M and M'. The flow q at the time t_1 is the same at both stations, and at this instant q does not change with x, or in symbols, $(\partial q/\partial x) = 0$. From the equation of continuity in the form

$$(\partial h/\partial t) + (\partial q/\partial x) = 0 \tag{7}$$

we obtain $(\partial h/\partial t) = 0$, which is a condition for the flood to reach its maximum height. This maximum occurs at the time t_1 which is later than the time of maximum flow, t_m.

Differentiating the equation, q = hu, we obtain

$$(\partial q/\partial t) = u(\partial h/\partial t) + h(\partial u/\partial t) \tag{8}$$

In (8) when $(\partial q/\partial t) = 0$, it is clear that both $(\partial h/\partial t)$ and $(\partial u/\partial t)$ must be zero or they must be of opposite signs. But we have just seen that the maximum flow at a given station precedes the maximum stage. Hence $(\partial q/\partial t) = 0$ occurs at a time when h is still increasing and therefore $(\partial h/\partial t)$ must be positive. It follows that $(\partial u/\partial t)$ is negative, the velocity is decreasing, and it therefore has already taken on its maximum value. Hence, the maximum velocity precedes the maximum flow. Finally, the maxima under consideration of a simple flood-wave occur in the following order: (1) Maximum velocity; (2) maximum discharge; and (3) maximum stage.

The relation between stage and discharge is single-valued only in the case of steady flow. During a flood the discharge for a given gage-height has two different values depending on whether the stage is rising or falling.

The velocity of a flood-wave as the rate of movement of a constant flow--Writing (6) in the differential form, we have

$$dq = (\partial q/\partial x)\, dx + (\partial q/\partial t)\, dt \tag{9}$$

If q now be regarded as constant we may obtain from (9) a relation between x and t satisfying the condition $dq = 0$. We may solve for (dx/dt) and obtain a virtual velocity representing the time-rate of displacement of the cross-section at which the discharge is the constant q. Substituting (7) into (9) it follows that $(\partial h/\partial t)\, dx = (\partial q/\partial t)\, dt$ and the virtual velocity, V_q, of the section of constant flow is

$$V_q = (dx/dt) = (\partial q/\partial t)/(\partial h/\partial t) = u + h(\partial u/\partial t)/(\partial h/\partial t) \tag{10}$$

in which the last step was the substitution of equation (8).

Flood-wave with constant velocity

Assuming the virtual velocity V_q, is constant and equal to V_c for any value of x,

$$V_c = (\partial q/\partial t)dt/(\partial h/\partial t)dt = dq/dh \tag{11}$$

In equation (11) q, the discharge, depends only on h. This equation is true when the flood is small. The velocity, V_c, may be interpreted as the velocity of an element of intumescence which has in front of it the same volume Q, of the swelling.

Replacing the differentials by finite differences,

$$V_c = (q_2 - q_1)/(h_2 - h_1) = (Q_2 - Q_1)/(A_2 - A_1) \tag{12}$$

where A_1 and A_2 represent the areas of the transverse sections at normal and the highest stage, and where Q_1 and Q_2 represent the corresponding flows. Equation (12) is true for a flood of any magnitude.

Velocity of a flood-wave with small amplitude
(This case of the flood-wave was also solved by Forchheimer [Hydraulik, 3rd ed., Berlin, p. 298, 1930].)

The velocity of a small flood-wave may be obtained from (11), as follows:

$$V_c = (dq/dh) = d(hu)/dh = (hdu + udh)/dh = u + h(du/dh) \tag{13}$$

The river is assumed to be in an initial state of uniform flow with discharge q_1 and stage h_1. The discharge is then increased by a small amount which causes a correspondingly small increase in stage height.

Assuming the velocity, u, varies as the square root of the depth of flow, h, as given by the Chezy formula, it can be shown that $h(du/dh) = u/2$ and, assuming Manning's formula, in which u varies as $h^{2/3}$, the wave-velocity $V_c = (5/3)u$

$$V_c = u + (u/2) = (3/2)u \tag{14}$$

where u and h may to a first approximation be replaced by u_1 and h_1 determined from low stage. Hence, it would appear that the velocity for a small wave is considerably greater than the mean velocity.

The velocity of the roll-wave

The velocity V_q for the roll-wave may be obtained very simply from Kleitz's equation (10). Here $u = K$ and $(\partial u/\partial t) = 0$. It follows that V_q is equivalent to the mean channel-velocity, a result that might easily have been anticipated.

Profile of the rising flood-wave

Velocity of the wave constant for fixed maximum stage--An equation will now be developed which gives the profile of the rising or "monoclinal" flood-wave moving with a constant velocity whose maximum stage is fixed over a considerable period of time.

Making use of (12) and replacing the lower limits of integration q_1, h_1 by general values, q, h corresponding to any position along the wave-front, the following is written $V_c = (q_2 - q)/(h_2 - h) = (h_2u_2 - hu)/(h_2 - h)$. Solving for u, we have $u = V_c - (h_2/h)(V_c - u_2)$. But from Chezy's formula, $u_2^2 = C^2h_2S$, $u_1^2 = C^2h_1S$ or substantially $h_1 = (u_1^2/u_2^2)h_2$ since the two values of C will be nearly the same. Hence, from (12)

$$V_c = h_2u_2 - (u_1^3/u_2^2)h_2/(h_2 - h_1) = (u_2 - u_1^3/u_2^2)/(1 - h_1/h_2) = (1 - y_1^{3/2})u_2/(1 - y_1) = K_1u_2 \tag{15}$$

where $y_1 = h_1/h_2 = u_1^2/u_2^2$. The mean velocity u may now be expressed as

$$u = V_c - (h_2/h)\,(V_c - u_2) = K_1u_2 - (h_2/h)\,(K_1u_2 - u_2) = [K_1 - (K_1 - 1)\,(h_2/h)]\,u_2 \tag{16}$$

The wave under consideration is long, and it is evident that the mean velocity, u, will change very slowly both with respect to the time, t, and the abscissa, x, that is, we may regard the expression $(du/dt) = (\partial u/\partial t) + (\partial u/\partial x)u$ as representing a small quantity. Accordingly, from (2) we now have

$$\left\{[(\partial u/\partial t) + u(\partial u/\partial x)] = 0\right\} + g(\partial h/\partial x) = g(S - Ku^2/h$$

or

$$(\partial h/\partial x) = S - Ku^2/h \tag{17}$$

Substituting for u as expressed in (16), $(\partial h/\partial x) = S - (K/h)\,[K_1 - (K_1 - 1)(h_2/h)]^2\,u_2^2$. But $K = 1/C^2 = h_2S/u_2^2$. Then $(\partial h/\partial x) = S - (h_2S/h)\,[K_1 - (K_1 - 1)/h_2/h)]^2$. To arrange for integration, let $t = t_1$ and use differentials as was done with (3) and

$$dx = dh/S\left\{1 - (h_2/h)\,[K_1 - (K_1 - 1)(h_2/h)]^2\right\}$$

Let $h = h_2z$, then

$$\begin{aligned} dx &= (h_2/S)(dz/\left\{1 - (1/z^3)[K_1z - (K_1 - 1)]^2\right\} \\ &= (h_2/S)\left\{1 + [K_1z - (K_1 - 1)]^2/(z^3 - [K_1z - (K_1 - 1)]^2)\right\} dz \\ &= (h_2/S)\left\{1 + (K_1^2z^2 - 2K_1(K_1 - 1)z + (K_1 - 1)^2)/(z - 1)[z^2 - (K_1^2 - 1)z + (K_1 - 1)^2\right\} dz \end{aligned}$$

This equation may be integrated in terms of elementary functions after expanding into partial fractions. The result is the equation of the profile of the wave and is

$$\begin{aligned} x + C_1 = h_2/S\Big\{&z + [1/(3 - 2K_1)]\log(1 - z) \\ &- [(K_1 - 1)^2(2K_1 + 1)/2(3 - 2K_1)]\log[z^2 - (K_1^2 - 1)z + (K_1 - 1)^2] \\ &- [(K_1 - 1)^2[(2K_1 + 1)(K_1 + 1) - 4]/2(3 - 2K_1)\sqrt{(K_1 + 1)^2 - 4}] \\ &\log[2z - (K_1^2 - 1) - (K_1 - 1)\sqrt{(K_1 + 1)^2 - 4}/2z - (K_1^2 - 1) + (K_1 - 1)\sqrt{(K_1 + 1)^2 - 4}]\Big\} \end{aligned} \tag{18}$$

The constant of integration is obtained from the condition $x = 0$ when $h = (h_1 + h_2)/2$. A representative profile of the rising flood-wave is shown in Figure 1.

Conclusions

(1) The profile of the roll-wave after equilibrium has been established may be obtained in terms of elementary functions from the general hydrodynamic equations of a simple flood-wave.

(2) The profile of the monoclinal, or rising flood-wave, after equilibrium has been established, may be obtained in terms of elementary functions from the general hydrodynamic equations of a simple flood-wave.

(3) The velocity of the monoclinal, or rising flood-wave in equilibrium, may be obtained from Kleitz's equation for the velocity of a constant flow.

(4) The velocity of a flood-wave of small height is found to be greater than the mean velocity of the channel in normal flow.

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PROGRESS-REPORT OF THE NATIONAL RESEARCH COUNCIL INTERDIVISIONAL COMMITTEE ON DENSITY-CURRENTS

Herbert N. Eaton, Chairman

During the past year the Committee has concentrated its efforts on securing evidence as to the occurrence of silt-laden currents of water at the bottom of Elephant Butte Reservoir and of Lake Mead. As reported last year in these Transactions, programs of cooperative field-observations and laboratory-studies in which a number of bureaus of the Federal Government were to participate were prepared at a meeting of the Committee in June 1937. These programs were carried out in somewhat modified form through the cooperative efforts of the United States Geological Survey, the United States Bureau of Reclamation, the Division of Western Irrigation Agriculture of the United States Bureau of Plant Industry, the United States Bureau of Fisheries, the International Boundary Commission, and the National Bureau of Standards. The United States Weather Bureau office at Albuquerque also contributed by furnishing warnings of flood-flows on the Rio Puerco, and the Metropolitan Water District of Southern California assisted by analyzing samples of water from Lake Mead. The present progress-report is not complete, since information as to some parts of the program carried out will not be available until the next meeting of the Committee, presumably in June, 1938. However, sufficient interesting data are available, particularly from Lake Mead, to warrant presenting it at this time.

Elephant Butte Reservoir

This Reservoir had the record of at least one flow of silty water as a bottom-current in each odd-numbered year since 1917, with the exception of 1925. Consequently it was anticipated, on the basis of this record, that such a current might be observed during 1937. However, the peak-flows of the Rio Puerco, which is believed to be the principal source of these muddy bottom-currents, were comparatively small last year, and L. R. Fiock of the United States Bureau of Reclamation reported as of November 22, 1937, that no flows of silty water through the Reservoir have been observed up to that time.

Inflow-conditions--The International Boundary Commission has continued to operate a gaging station at San Marcial, New Mexico, just above the Reservoir. The discharge has been measured at this station, and since February 1, 1937, water-samples have been taken daily for the determination of total dissolved solids by the electrometric method. Monthly composites have been made from these daily samples, and a detailed analysis of these composites has been made.

Conditions in the Reservoir--At a meeting of the Committee on November 24, 1937, Dr. M. M. Ellis of the United States Bureau of Fisheries reported that his field-party had spent eight days on the Rio Grande above and below the dam. Measurements were made over cross-sections of the Reservoir as planned. Detailed information as to the nature and scope of these field-data is not at hand as yet, however. It is interesting to note that twice during the season a sharply-defined finger or wedge of water having wavy surfaces and differing in salt content from the water of the Reservoir above and below it was observed, extending in one instance through approximately half the length of the Reservoir.

Outflow-conditions--The United States Bureau of Reclamation made observations at Elephant Butte Dam throughout the season. These included daily measurements of the volume and temperature of the discharge at the outlet-gates, the elevation of the surface of the Reservoir, the temperature of the surface-water, and weekly samples of the discharge- and surface-waters. These individual samples have been tested for conductance, and monthly composites have been analyzed for salt constituents.

Lake Mead

The United States Bureau of Reclamation has made monthly determinations of the water-temperatures in verticals midway between the intake-towers at Boulder Dam and at midchannel at Cape Horn, about 0.6 mile upstream from the Dam, and has taken water-samples in these same verticals. Similar observations and samples have been taken at less frequent intervals at Boulder Canyon and Virgin Canyon, approximately 21 miles and 50 miles, respectively, upstream from the Dam. One set of observations has been reported from a point 83 miles upstream from the Dam, as well as several sets from the Lower Narrows of the Virgin River. These observations have made it possible to locate the top of the layer of muddy water and the elevation of the bed, as well as the latter could be determined.

These data, in spite of the infrequent intervals at which they were taken, give quite good evidence of at least one flow of a silty bottom-current through the Lake over the period considered (see Fig. 2).

Laboratory-work of the United States Geological Survey

The work of the United States Geological Survey during the past year in connection with the program of the Committee was on two projects: (1) Daily samples collected by the Indian Service in connection with studies being made at Coolidge Dam; (2) samples from Lake Mead collected by the Bureau of Reclamation.

The laboratory-work has consisted in making certain determinations to indicate changes in the chemical composition and concentration of the dissolved salts in the inflowing, outflowing, and lake waters at Coolidge and Boulder dams. The laboratory-procedure was to determine the specific conductance of the water on all samples from each sampling point to ascertain variations in concentration and then to make other determinations, when variations in concentration occurred, which would indicate the nature of the changes in chemical composition. These other determinations usually included calcium, magnesium, bicarbonate, sulphate, and chloride.

The regular studies of the Geological Survey on the composition of the water at Grand Canyon and Willow Beach gaging stations on the Colorado were continued. These data are necessary for a complete study of the Reservoir conditions at Boulder Dam.

A brief summary of the results obtained on the samples from the two projects is given below. Complete analytical data for the samples from the Boulder Dam project have been tabulated.

Coolidge Dam Project--Samples of water were collected and observations made of its temperature during August and September 1937 by the United States Indian Service on the Gila and San Carlos rivers above the San Carlos Reservoir and at the outlet of the Reservoir at Coolidge Dam. The mean daily discharge of the San Carlos River during the period was less than 100 second-feet for all but four days. The flow of the Gila during this period was somewhat larger, and in the two months of the study more than 250,000 tons of suspended matter was carried past the Calva gaging station at the head of the Reservoir. Some of the samples of the inflowing water had more than one per cent of suspended matter, of which a large part consisted of particles with diameters less than 20 microns. No suspended matter was observed flowing out of the Reservoir, and it seems likely that the flows of turbid water into the Reservoir were not of sufficient magnitude to cause a density-current through the Reservoir.

Boulder Dam Project--During the period from July 1, 1937, to March 12, 1938, more than 300 samples collected from Lake Mead were sent to the Washington Laboratory of the Geological Survey. Other samples were collected for the Metropolitan Water District, and periodic determinations of certain constituents were made at the Boulder City Water-Works. The Washington Laboratory made a few determinations of the sizes of the particles of the suspended matter at the bottom of the Lake, but most of the samples received in Washington have been clear; and, because of certain chemical changes, all of the more recent samples have had the clear water decanted off from the suspended matter as soon as possible after the samples were collected.

The few determinations made in the Washington Laboratory on the sizes of the suspended matter show that it consists of very small particles, more than 90 per cent being less than 30 microns in diameter. Determinations of the sizes of the particles in the suspended matter at the bottom of the Lake in various places show that all of this suspended material consists of very small particles.

The results of determinations of the concentration and chemical composition of the water in various parts of the Lake and at different depths have indicated stratification within the Lake. A summary of these results is given in Table 1. It will be seen that the concentration increases with depth at each station and that at the bottom the water is considerably more concentrated than at the surface. Chemical analyses also show that the water at the bottom is considerably more concentrated than at the surface, and because of the nature of the dissolved salts no direct comparison of concentrations can be made from the conductance-values alone. The bottom-waters are higher in bicarbonate, calcium, magnesium, and sodium and often have less sulphate than the waters at other depths. The quantity of water of this character is relatively small compared with the total amount of water in the Reservoir, and further study is necessary to ascertain the importance of this water at the bottom of the Lake.

Analytical results for samples collected in the Lake compared with results obtained at Grand Canyon have made it possible to show changes in the composition of the water which indicate movement of density-currents through the Lake.

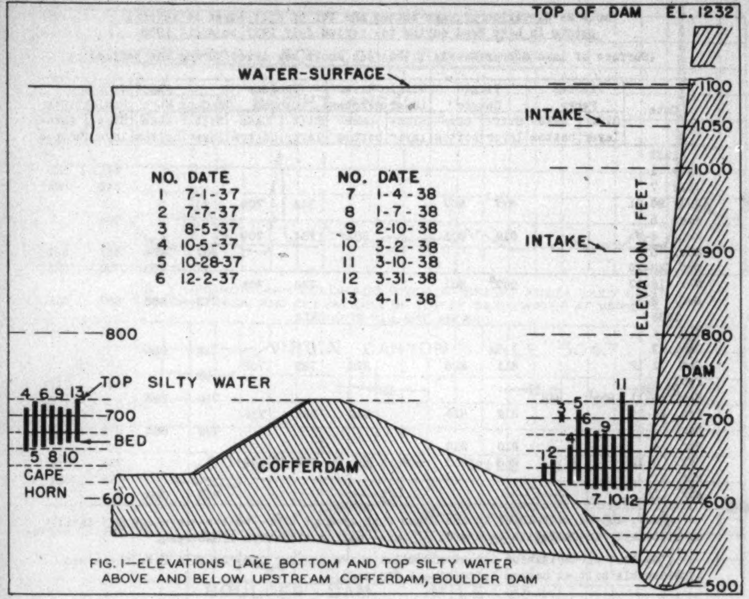


FIG. 1—ELEVATIONS LAKE BOTTOM AND TOP SILTY WATER ABOVE AND BELOW UPSTREAM COFFERDAM, BOULDER DAM

Table 1--Average values of specific conductance of water at different depths at four points in Lake Mead

(Specific conductance, $K \times 10^5$)[a,b]

Approximate elevation (feet above sea-level)	Black Canyon				Boulder Canyon				Virgin River Narrows				Virgin Canyon			
	1937		1938		1937		1938		1937	1938			1937		1938	
	Oct.	Dec.	Jan.	Mar.	Sep.	Nov.	Jan.	Mar.	Sep.	Jan.	Feb.	Mar.	Sep.	Nov.	Jan.	Mar.
1,000 to 1,100	84	88	92	104	71	81	96	103	76	98	103	103	85	110	109	110
850 to 950	115	114	116	121	117	116	121	121	119	99	128	184	121	133	135	127
800	121	120	119	134	119	127	124	133					162	138	141	155
700	145	124	129	143	114	123	167	166								
625	156	132	147	153												

[a] Specific conductance times 8 = approximate parts per million of dissolved solids.

[b] The last figures in each column represent samples from the bottom of the Lake.

In Table 2 there are tabulated the results of samplings at Lake Mead to determine the height of the top surface of the bottom layer (or current) of silty water, also the elevation of the bed, as nearly as could be determined by sampling. The observations extend over the period from July 1, 1937, to April 1, 1938, and were made at six stations. The data given in Table 2 are a part of the data secured by the staff of the United States Bureau of Reclamation at Boulder Dam and were furnished by Ralph Lowry of that office.

Observations of silty water in Lake Mead--Elevations of the top of the silty water and the bed are given in Figure 1 for the pool between the upstream cofferdam and Boulder Dam and for the pool upstream from the cofferdam (taken at Cape Horn, 0.6 mile upstream from the Dam).

Table 2--Elevation of Lake-bottom and top of silt-layer at several points in Lake Mead during the period July 1937 to April 1938

(Surface of Lake approximately 1,100 feet above sea-level during the period)

Date	Pierces Ferry		Virgin Canyon		Virgin River Lower Narrows		Boulder Canyon		Black Canyon			
									Cape Horn		Boulder Dam	
	Silt-layer	Lake-bottom	Silt-layer	Lake-bottom	Silt-layer	Lake-bottom	Silt-layer	Lake-bottom	Silt-layer	Lake-bottom	Silt-layer	Lake-bottom
1937												
July 1											645	625
July 7											649	625
July 20-21			827	807			742	705				
Aug. 5											704	...
Sep. 8-9			819	801		901	734	709				
Oct. 5									707	658	667	621
Oct. 28-29									717	663	709	625
Nov. 16-17			809[a]	801			726	708				
Dec. 2-3									713	663	687	615
1938												
Jan. 4											684	616
Jan. 7									712	666		
Jan. 11-12			811	808		892	729	708				
Feb. 2											681	613
Feb. 10-11	955[b]	948							710	666		
Feb. 24-25			812	810		892	725	715				
Mar. 1-2									709	668	679	615
Mar. 4			810	810								
Mar. 9-12			848	813	902	896	754	709			731	614
Mar. 31											715	614
Apr. 1									719	669		

[a] Nov. 16: No silt-layer in Las Vegas Wash Area of Lake--bottom-elevation 936 feet; no silt-layer in Callville Wash Area of Lake--bottom-elevation 1,026 feet.

[b] At head of navigation (about 110 miles above Boulder Dam) on this date there was considerable silt at bottom (elevation 1,068 feet).

During the earlier part of the period over which these observations extended, there was some question as to how the silty water passed the cofferdam, inasmuch as the elevations of the top of the layers in the two pools were never observed to attain to elevation 720, the height of the top of the cofferdam. In reply to a question as to whether the cofferdam might have been breached, Mr. Lowry stated: "It was our impression, since verified by consulting several who were here at the time and acquainted with the situation, that the cofferdam was left intact in the fullest sense, that is, not trenched or breached in any way, either deliberately, or accidentally by the overflowing sheet of water" when the level of water in the Lake as it filled reached the top of the cofferdam. Hence there are three possibilities by which the silty water could pass the cofferdam: (1) By a rise in the level of this silty water in the pool above the cofferdam to such an extent that it overflowed the cofferdam; (2) by waves set up at the interface of the silty water and the clear water above it by sudden changes in outflow at the intakes; and (3) by the deflection upward of a current of silty water striking the cofferdam.

The Colorado River flood of last March, however, gave fairly good evidence that at times the silty water does overflow the cofferdam, simply owing to a rise in the level of its upper surface (see Fig. 2). In this Figure are plotted the main features of the hydrograph of the Colorado River at Bright Angel 267 miles upstream from the Dam for the period during which observations on the silty water-layer have been taken, also elevations of the top surface of this layer at Boulder Dam, at Cape Horn, at Boulder Canyon, and at Virgin Canyon over this period.

On March 4 an observation at Virgin Canyon indicated that there was no layer of silty water at that station (Table 2) at that time. This was just before the effect on the layer of silty water in the Lake of a sudden flood that reached a peak of about 52,000 cfs at Bright Angel on March 5 was felt. Further observations at Virgin Canyon from March 9 to 12 showed that there was then a layer of silty water at that point 35 feet thick, and readings taken at Boulder Canyon at this same time showed that the layer of silty water had increased in thickness from 10 feet to 45 feet. This could be due only to a flow of silty water.

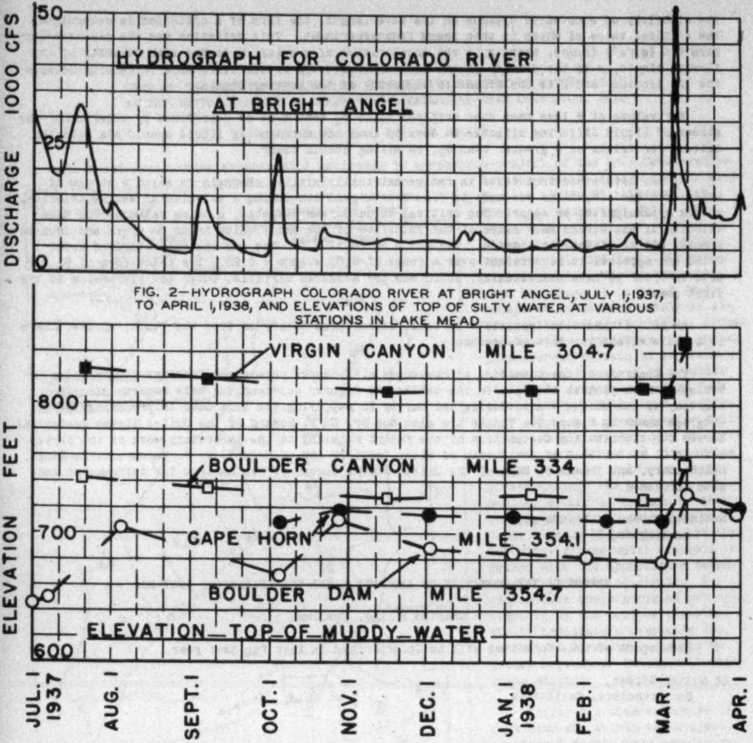


FIG. 2—HYDROGRAPH COLORADO RIVER AT BRIGHT ANGEL, JULY 1, 1937, TO APRIL 1, 1938, AND ELEVATIONS OF TOP OF SILTY WATER AT VARIOUS STATIONS IN LAKE MEAD

During the period from March 1 to 9, the level of the silty water at Boulder Dam increased from elevation 679 to 731, an increase of 52 feet; that is, the level of this silty water-pool was 11 feet higher than the crest of the cofferdam. Unfortunately no readings were taken at Cape Horn between March 1 and April 1, and by April 1 the level in the pool nearer the Dam had fallen 16 feet by settling of the suspended silt, and presumably lowering of the level in the upper pool had also taken place from this same cause. On April 1, when the next observation was made on the pool above the cofferdam, the level of the silty water in this pool had dropped a little below the crest of the cofferdam.

Laboratory-investigations

Experimental studies of the flow of stratified liquids have been under way at the National Bureau of Standards for more than a year and more recently have been undertaken at the California Institute of Technology.

At the National Bureau of Standards, B. H. Monish and G. H. Keulegan have developed a criterion for mixing in a form suitable for use when the mean velocity of the current, but not its velocity-profile, is known. This criterion has been developed in two different ways, by the methods of dimensional analysis and by the method of mechanics, conceiving of the mixing as due to the breaking of waves set up at the interface of the two fluids. Using the known fact that the stability of such waves depends on the wave-length, the form of a criterion is determined, the critical value of which is then found from experiment. This criterion has the dimensionless form $\theta = (g\nu/u^3)(\Delta\rho/\rho)$, where g is the acceleration of gravity, u is the mean velocity of the flowing liquid, ρ is the density of the heavier liquid, $\Delta\rho$ is the difference in density between the two liquids, and ν is the kinematic viscosity of the heavier liquid.

For values of θ less than some critical value θ_c that must be determined by experiment, the stream of liquid differing slightly in density from the surrounding liquid should mix with the latter; for values of θ greater than θ_c, no mixing should occur.

θ_c was determined from tests in two geometrically-similar channels in which a stream of water from the city-mains was made to flow over a pool containing a solution of sodium chloride, sodium hyposulphite, or sugar. The critical value θ_c was computed, u being taken as the mean velocity of the stream when waves at the interface of the two liquids began to break and thus to produce mixing of the two liquids. The value of $(\theta_c)^{1/3}$ for the two channels averaged about 0.155 and appeared to be constant over a range of $0.02 < \Delta\rho/\rho < 0.20$. The third root of θ_c was used in order to have the velocity, which was the measured variable, enter the expression to the first power.

As yet, it has been impossible to tell, either from theory or from the tests, whether there is a scale effect for this phenomenon.

The Chairman of the Committee acknowledges with thanks permission from the Commissioner of Reclamation to publish the data in the tables and figures accompanying this report, also the courtesy of the United States Geological Survey in supplying the data used in preparing the hydrograph shown in Figure 2. Thanks are also due Dr. C. S. Howard of the United States Geological Survey for contributing the portion of the report relating to the laboratory-work of the Survey, also to C. S. Scofield of the Bureau of Plant Industry and to Messrs. C. P. Vetter, L. R. Flock, Ralph Lowry, and Thomas C. Mead of the United States Bureau of Reclamation for information and data furnished.

National Bureau of Standards,
Washington, D. C.

REPORT OF THE COMMITTEE ON ABSORPTION AND TRANSPIRATION, 1937-38

Charles H. Lee, Chairman

The report of this Committee will be incorporated in that for next year.

58 Sutter Street,
San Francisco, California

PAPERS

SOME OF THE FACTORS INFLUENCING THE BEHAVIOR OF PERCHED WATER-TABLES AT THE NORTH APPALACHIAN EXPERIMENTAL WATERSHED NEAR COSHOCTON, OHIO

W. D. Potter and Merle V. Baker

The North Appalachian Experimental Watershed is a research-project of the Soil Conservation Service and is located for the most part in Little Mill Creek Watershed near Coshocton, Ohio. The topography consists of rolling hills, the average relief being approximately 300 feet (Fig. 1-A). A geological survey of the watershed has shown it to be an eroded peneplane consisting mainly of alternate and nearly horizontal strata of limestone, sandstone, and shale, interspersed by beds of impervious fire-clay and covered by a mantle of soil of from four to five feet in thickness. Each layer of fire-clay supports a perched water-table either wholly or partially separated from each other and discharging at the outcrops of the impervious strata through the overlying soil-mantle, either as springs or seeps or by percolation, to the water-table below. Figure 1-B shows the principal ground-water horizons together with the approximate thickness of intervening strata. For convenience these horizons are designated by the coal or limestone strata immediately overlying the impervious fire-clay, such as No. 6-coal horizon or Putnam-Hill-Limestone horizon.

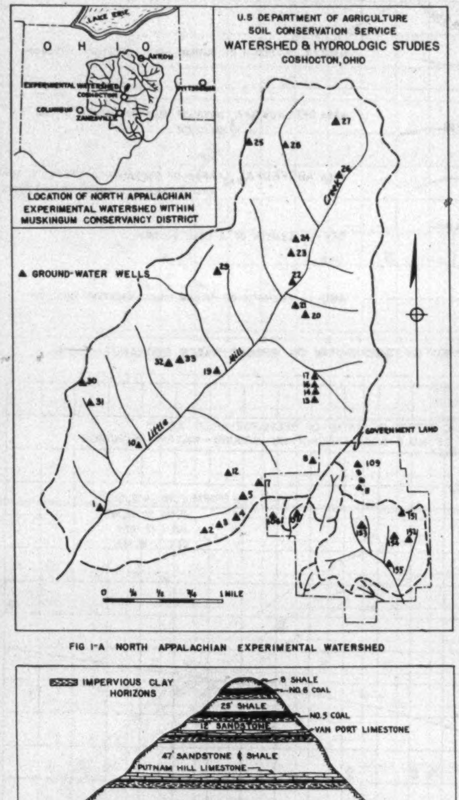

LAKE ERIE
O H I O
AKRON
PITTSBURGH
EXPERIMENTAL WATERSHED
COSHOCTON
COLUMBUS
ZANESVILLE
LOCATION OF NORTH APPALACHIAN EXPERIMENTAL WATERSHED WITHIN MUSKINGUM CONSERVANCY DISTRICT
U.S DEPARTMENT OF AGRICULTURE
SOIL CONSERVATION SERVICE
WATERSHED & HYDROLOGIC STUDIES
COSHOCTON, OHIO
▲ GROUND-WATER WELLS
27
25
28
26
Creek
24
23
29
22
21
20
32
33
19
Hill
17
16
14
13
30
31
Little
10
GOVERNMENT LAND
8
109
12
7
5
1
4
3
2
151
152
153
154
155
0 1/4 1/2 3/4 1 MILE

FIG 1-A NORTH APPALACHIAN EXPERIMENTAL WATERSHED

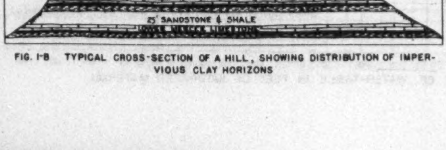

IMPERVIOUS CLAY HORIZONS
8 SHALE
NO. 6 COAL
25' SHALE
NO. 5 COAL
12' SANDSTONE
VAN PORT LIMESTONE
47' SANDSTONE & SHALE
PUTNAM HILL LIMESTONE
60' SANDSTONE & SHALE
UPPER MERCER LIMESTONE
25' SANDSTONE & SHALE
LOWER MERCER LIMESTONE

FIG. 1-B TYPICAL CROSS-SECTION OF A HILL, SHOWING DISTRIBUTION OF IMPERVIOUS CLAY HORIZONS

Thirty-six observation-wells have been dug within the project, an attempt having been made to secure several representative wells for each principal ground-water horizon. All wells, with the exception of those located in valley-bottoms, were dug to the first impervious strata, the average depth being approximately 30 to 40 feet. These wells were dug by hand with an unlined diameter of four feet and a finished diameter of 28 to 36 inches. Field-stones used as lining were free laid to the base of the weathered zone and from this point to the surface were laid in mortar. A reinforced concrete well top, having embedded in it a T-bar measuring point, was installed in each well.

Manual ground-water measure-

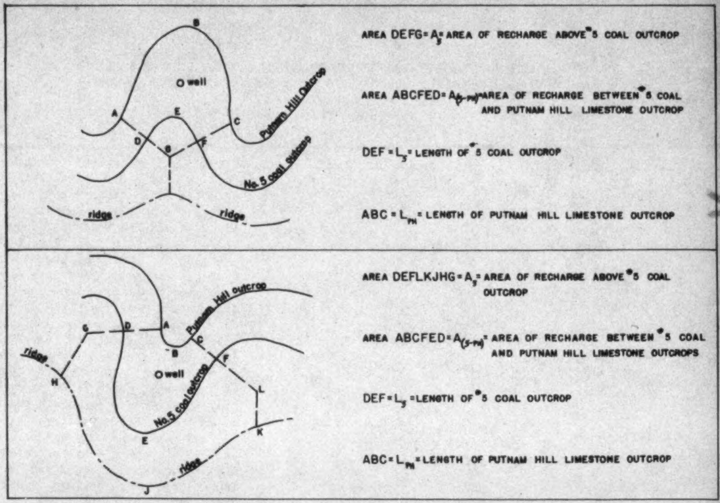

B
O well
Putnam Hill Outcrop
A
E
C
D
G
F
No. 5 coal outcrop
ridge
ridge
AREA DEFG = A_5 = AREA OF RECHARGE ABOVE #5 COAL OUTCROP
AREA ABCFED = $A_{(5-PH)}$ = AREA OF RECHARGE BETWEEN #5 COAL AND PUTNAM HILL LIMESTONE OUTCROP
DEF = L_5 = LENGTH OF #5 COAL OUTCROP
ABC = L_{PH} = LENGTH OF PUTNAM HILL LIMESTONE OUTCROP
Putnam Hill outcrop
G
D
A
C
B
ridge
H
O well
F
No. 5 coal outcrop
L
E
K
ridge
J
AREA DEFLKJHG = A_5 = AREA OF RECHARGE ABOVE #5 COAL OUTCROP
AREA ABCFED = $A_{(5-PH)}$ = AREA OF RECHARGE BETWEEN #5 COAL AND PUTNAM HILL LIMESTONE OUTCROPS
DEF = L_5 = LENGTH OF #5 COAL OUTCROP
ABC = L_{PH} = LENGTH OF PUTNAM HILL LIMESTONE OUTCROP

FIG. 2-A SKETCH SHOWING EFFECT OF TOPOGRAPHY ON GROUND-WATER RECHARGE-FACTOR

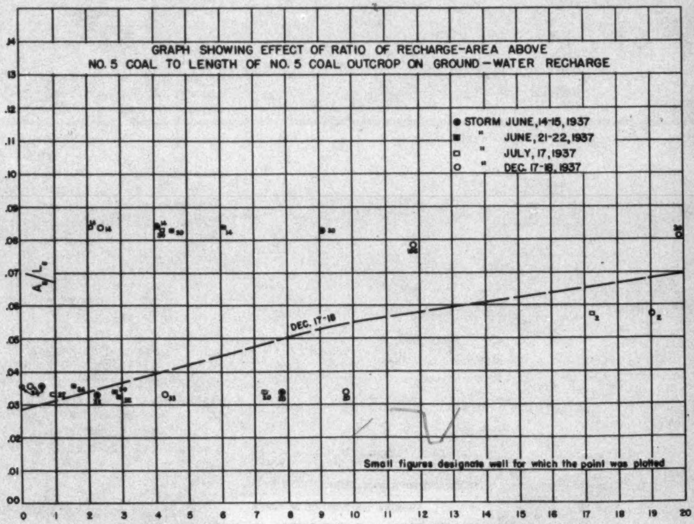


FIG. 2-B RECHARGE OF WATER-TABLE IN FEET OF SATURATED MATERIAL

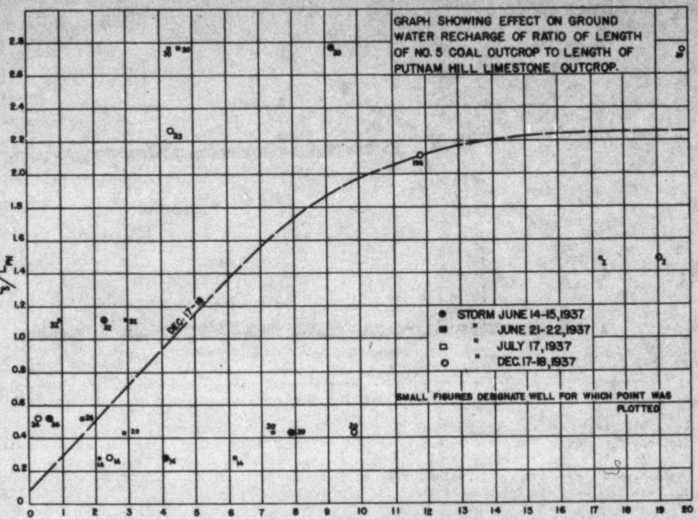


FIG. 3-A RECHARGE OF WATER-TABLE IN FEET OF SATURATED MATERIAL

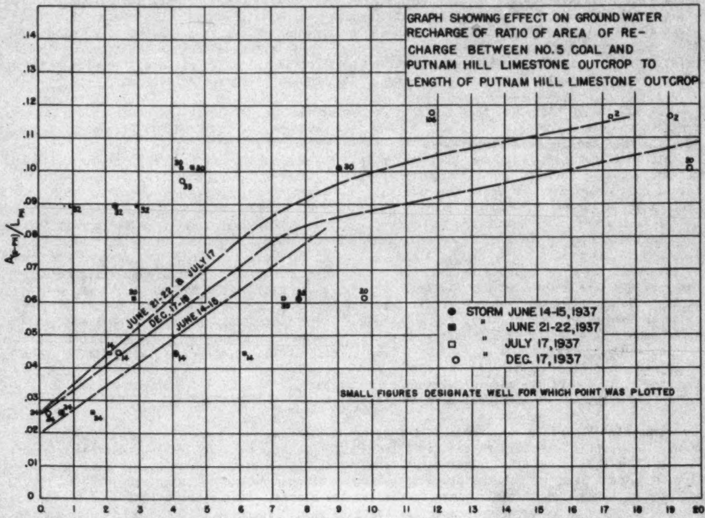


FIG. 3-B RECHARGE OF WATER-TABLE IN FEET OF SATURATED MATERIAL

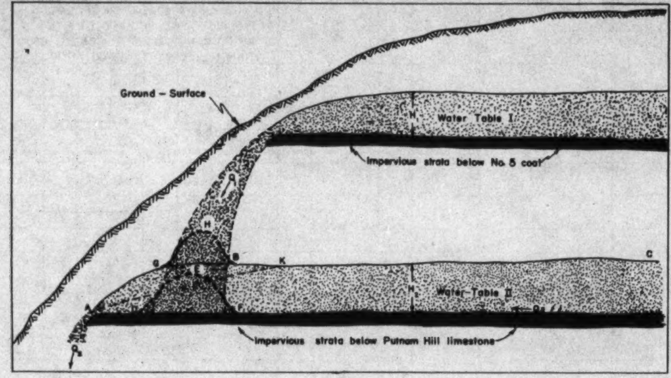

Ground – Surface
H_1
Water Table I
Impervious strata below No. 5 coal
Q
H
G
B
K
C
H_2
Water-Table II
A
F
Q_2
Impervious strata below Putnam Hill limestone
Q_3

FIG. 4 SKETCH SHOWING SHAPE OF WATER-TABLE FOR VARIOUS ASSUMED CONDITIONS

ments were begun at completed wells in May 1936. These measurements were made at intervals of from one to 15 days from May 1936 until February 1937 and daily measurements were taken on all wells from February 1937 until the time of installation of automatic stage-recorders. The installation of these recorders, which are Friez FW-1 vertical-drum type, was begun in April 1937, and continued until at the present time all wells are so equipped.

Recharge of the various water-tables is from two sources, namely, (1) the percolation of the precipitation falling on the surface of the relatively narrow strip between impervious outcrops and (2) the percolation of the discharge from an overlying water-table. Discharge takes place along the outcrop of the impervious strata in the form of springs or seeps, or as recharge to the underlying water-table. It was felt that a study of factors affecting the recharge-discharge relation in the vicinity of observation-wells would provide an explanation of the wide range of ground-water fluctuations observed among wells in the same ground-water horizon.

It was thought probable that the most important factors affecting the relative amounts of recharge and discharge in the vicinity of any well would be the ratio of the area of recharge to the length of the discharge-outcrop and the ratio of the length of recharge-outcrop to that of the discharge-outcrop. Fig. 2-A illustrates that these factors vary considerably for different topographical locations. Considering the effect of ground-water fluctuations for wells in the Putnam Hill Limestone, and using the notations A_5 = area of recharge above the No. 5-coal outcrop, $A_{(5-PH)}$ = area of recharge between the No. 5-coal and the Putnam-Hill-Limestone outcrops, L_5 = length of No. 5-coal outcrop, and L_{PH} = length of Putnam-Hill-Limestone outcrop, it is evident that while A_5/L_5 for a spur may be greater or less than A_5/L_5 for a draw, depending on the location of the ridge, $A_{(5-PH)}/L_{PH}$ and L_5/L_{PH} are considerably greater in the case of the draw. To determine the effect of the ratios A_5/L_5, $A_{(5-PH)}/L_{PH}$, and L_5/L_{PH} on ground-water recharge, these ratios were determined for all wells in the Putnam Hill Limestone and the values plotted against the ground-water recharge for several storms. Figures 2-B, 3-A, and 3-B show the results of such comparisons. It is recognized that the effect of any one factor on the ground-water recharge for any well may have been partially or wholly obscured by the effect of the other two factors. For example, A_5/L_5 may have been large, but its effect on the ground-water recharge could have been considerably reduced if $A_{(5-PH)}/L_5$ and L_5/L_{PH} were both small. It is also recognized that some of the non-uniformity in the graphs may have been due to differences in infiltration rates and to differences in specific yield.

Since the experimental watershed is located at the head of a valley, the total L_5/L_{PH} is greater than unity and likewise the total length of the Upper-Mercer-Limestone outcrop is less than that of the Putnam Hill Limestone and greater than that of the Lower Mercer Limestone. Also, the total area of recharge per unit-length of discharge-outcrop is greater for outcrops near the valley-bottom than for those further up the slopes. Such observations lead to the conclusion that the range of ground-water fluctuations should increase from the No. 6-coal to the

Lower-Mercer-Limestone horizons. Because, as will later be shown, the range of fluctuations actually decrease instead of increase from the ridge to the valley-bottom and because of the wide scattering of points as shown on the graphs (Figs. 2-B, 3-A, and 3-B), it is felt that while such ratios as A_5/L_5, $A_{(5-PH)}/L_{PH}$, and L_5/L_{PH} undoubtedly affect the ground-water recharge, they cannot be considered as the controlling factors.

A theoretical analysis of the effect of the recharge-discharge relation on the shape and fluctuation of the water-table at the site of any one well provides a possible explanation for the wide variation of ground-water fluctuations observed among wells in the same water-table, and for the decrease in the range of fluctuations of water-tables near the valley-bottom.

In Figure 4, let H_1 - head in Water-Table 1, Q_1 = discharge of Water-Table 1, and H_2 and Q_2 - head and discharge, respectively, for Water-Table 2, and assume that L_5/L_{PH} equals unity, that the discharges Q_1 and Q_2 result from laminar flow, that the impervious strata are impervious throughout and allow no leakage, that there is no flow through channels caused by the shattered condition of coal or joints in the limestone, that the permeability of water-bearing materials in both water-tables is substantially the same, and if the recharge to Water-Table 2 resulting from precipitation on the ground-surface between impervious outcrops is neglected so that the recharge is assumed to be wholly from Q_1. Then, if Water-Table 2 is dry, a mound, DEF, will be formed in the material directly under the diffused Q_1, a head will be built up, and flow will take place along both slopes of the mound. If conditions remain constant for an indefinite period of time, a state of equilibrium will be reached and Water-Table 2 will assume the surface ABC; the slope of KC will just be sufficient to equal the frictional resistance to flow, H_2 will equal H_1, and Q_2 will equal Q_1. If Q_1 is increased, a mound, GHB, will be formed within the limits of the diffusion of Q_1, a head will be built up on ABC, and flow will take place along both slopes of the mound until a state of equilibrium is reached; then Water-Table 2 will be parallel and above ABC, Q_2 will be equal to the increased Q_1, and H_2 will equal H_1. If Q_1 is reduced, Water-Table 2 will assume the surface AKC, Q_2 will equal the reduced Q_1, and H_2 will still equal H_1.

Due to the frequency of storms in this vicinity it seems probable that a mound of varying proportions would almost invariably be present on the surface of Water-Table 2, and that a state of equilibrium between the water-tables would seldom, if ever, be reached.

If it is now assumed that a portion of the discharge from Water-Table 2, Q_3 , is through channels caused by the shattered condition of the coal strata or through joints in the limestone strata, and appears at the surface in the form of seeps or springs, then in a state of equilibrium, Q_1 will equal the sum of Q_2 and Q_3, and H_2 will be less than H_1. Such a condition would result in a progressive decrease in the magnitude of ground-water fluctuations from the upper or No. 6-coal horizon to the lower or Lower-Mercer horizon.

Figure 5 shows the hydrographs of the water-table at representative wells in three of the principal ground-water horizons for the water-year 1936-37 (October 1, 1936, to September 30, 1937). A datum ten feet below the elevation of the water-surface as of December 5, 1936, was assumed for the purpose of these hydrographs. It will be noted that the magnitude of ground-water fluctuations decreased progressively from the Putnam Hill Limestone to the Lower Mercer Limestone, and it will also be noted that there is considerable difference in the range of fluctuations for wells in the same ground-water horizons. If the theoretical analysis of the relations between recharge and discharge, as outlined above, is sound, then a study of these hydrographs would lead to the following conclusions: (1) That discharge through channels formed in shattered coal-strata, or in jointed limestone-strata, exists in all ground-water horizons; and (2) that the range of fluctuations to be expected at a well in any ground-water horizon located between impervious outcrops depends on its position with respect to possible ground-water mounds or, in other words, to its position with respect to the recharge and discharge impervious outcrops.

In order to obtain a better understanding as to the shape and fluctuations of the water-table between recharge- and discharge-outcrops, all wells in the Putnam Hill Limestone were considered as though they had been located in the same vertical plane, the depth of water above the supporting impervious strata being plotted for several storm-periods. A generalized cross-section was obtained by using average values for the horizontal distance between the outcrop of the No. 5 coal and that of the Putnam Hill Limestone, and for the thickness of the intervening strata. Wells were located on this generalized cross-section so that the ratio of the horizontal distance between the well and No. 5-coal outcrop on the generalized section to the average total horizontal distance between outcrops was equal to a like ratio taken at the actual location of the well. Figures 6 and 7 show the results of plotting the position of the water-table with respect to the

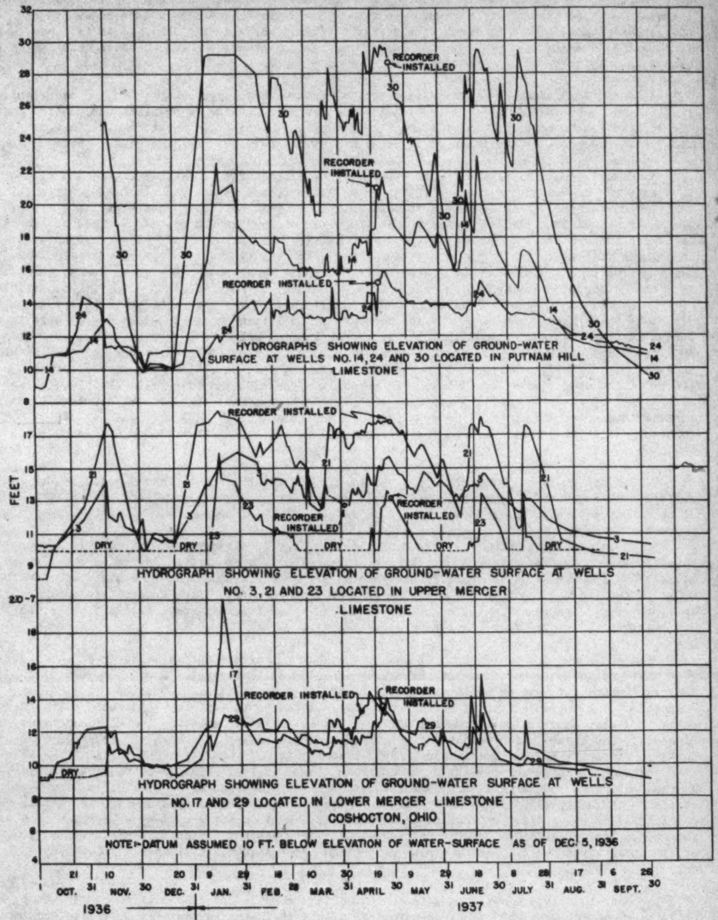


FIGURE 5

supporting impervious strata for each well during storm-periods occurring during June, July, and December 1937. It will be noted that the shape of the water-table, as shown by these graphs, is that of a ground-water mound and that the range of fluctuations is considerably greater at the summit than on the side-slopes.

The existence of this mound condition was also indicated in model tests. A tank--17 inches long, 12 inches high, and 4 inches wide--was constructed with a plate-glass front. Three wells,

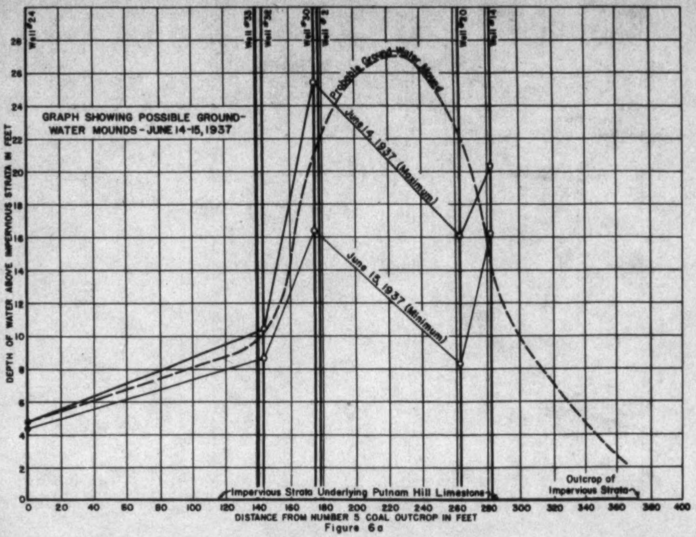


Figure 6a

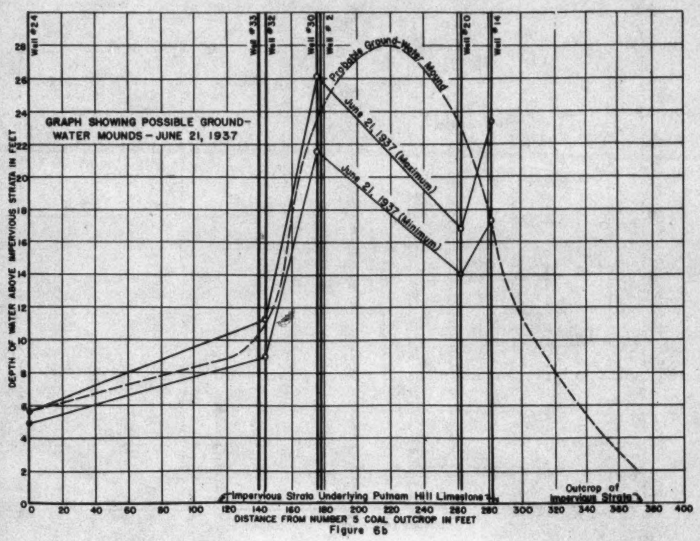


Figure 6b

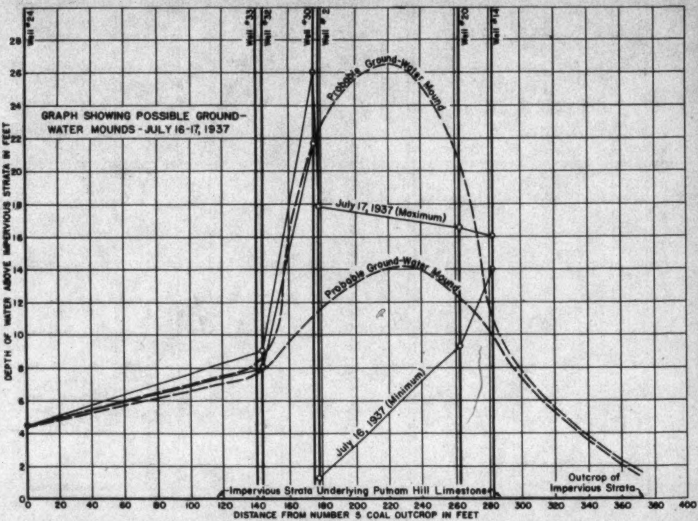


Figure 7a

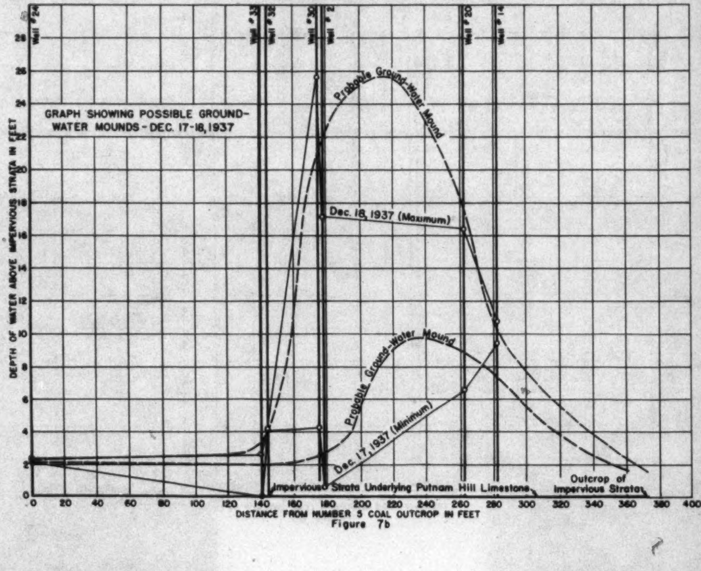


Figure 7b

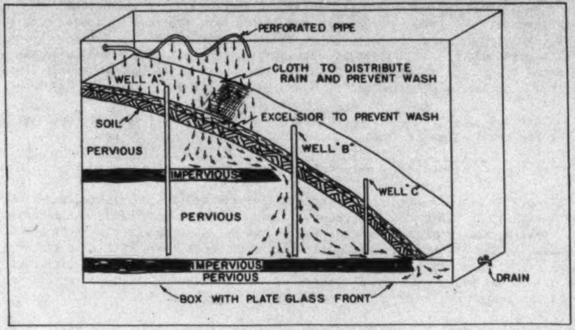
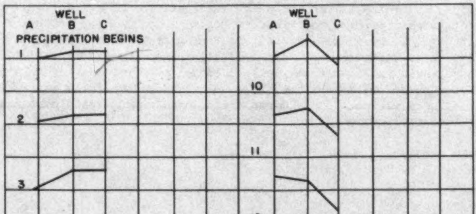

PERFORATED PIPE
CLOTH TO DISTRIBUTE RAIN AND PREVENT WASH
WELL "A"
SOIL
EXCELSIOR TO PREVENT WASH
PERVIOUS
WELL "B"
IMPERVIOUS
WELL "C"
PERVIOUS
IMPERVIOUS
PERVIOUS
DRAIN
BOX WITH PLATE GLASS FRONT

FIG. 8A SKETCH OF PRELIMINARY SMALL MODEL TANK USED FOR GROUND WATER STUDIES



HYDROGRAPHS OF LOWER WATER-TABLE SHOWN BY THREE WELLS
MODEL STUDY OF HILL CONTAINING TWO IMPERVIOUS HORIZONS

FIG. 8B

open at the bottom, were attached to this glass and reflected the head of the water at the location of the well. The tank was then filled with material simulating one-half of a hill and containing two impervious layers. The impervious layers were composed of wetted fire-clay of the plastic type, while the pervious material consisted of fine sand in which was a considerable per cent of silt-size particles. A layer of excelsior and a cloth were placed over the surface to distribute the precipitation and prevent surface-wash. An outlet and source of precipitation were provided. A diagram of this tank is shown in Figure 8-A.

During the test the precipitation was turned on for a period of time and water-levels in the three wells noted at approximately ten-second intervals. The graphs of these levels are shown in Figure 8-B. It will be noted that the rise in wells A and B continued for a period after the precipitation ceased and a well-defined mound was produced.

An attempt was made to use dyes to trace the course of the percolating water and the shape of the water-table, but this was unsuccessful after the first wetting of the material. Even in this first wetting the progressive boundaries of the percolating water were difficult to discern, and the possibilities of error were so great that the results were of little scientific value though they seemed to confirm the theory discussed in this paper. Several difficulties were encountered due to the small size and crudeness of the model and it is hoped that these will be overcome in the larger and more refined tank now being designed.

It is planned to drill at least six new wells during the next fiscal year. These wells will be located in the same vertical plane and will provide data for the more accurate determination of the shape and extent of the water-tables, as well as for a more detailed study of the recharge-discharge relations governing the ground-water fluctuations. Since at least one of the new wells will be drilled through the overlying impervious strata to that supporting the water-table under observation, data will be available not only on that portion of the water-table lying between recharge- and discharge-outcrops, but also on that portion located in the interior of the hill. It is also planned to install measuring devices on selected springs and to study the relation between the flow of such springs and the shape and fluctuation of the water-table. As a result of the analysis of such new data it is hoped that a more conclusive report can be made next year than was possible at the present time.

Soil Conservation Service,
U. S. Department of Agriculture,
Washington, D. C.

EXPERIMENTAL WATER-SPREADING FOR GROUND-WATER STORAGE IN SALT LAKE VALLEY, UTAH, 1936

A. J. Lazenby

Introduction

Location and general features of area--This report describes tests made during 1936 to artificially recharge the ground-water reservoir underlying a five-square-mile area located southeast of Salt Lake City, Utah. The area, shown by Figure 1, is about four miles long, from east to west, and about 1.25 miles wide from north to south. It is bounded on the north by Parleys Canyon Creek, on the south by Mill Creek, and on the east by the base of the Wasatch Mountains. The elevation near the base of the Wasatch Mountains is about 5200 feet above sea-level. Toward the west the slope of the ground-surface is more or less uniform to an elevation of about 4350 feet above sea-level. This lower level is at the approximate western edge of the area studied and is just below the flow line of a north-south line of natural springs which, according to Leggett and Taylor [see reference 1 at end of paper], is the natural overflow of the artesian reservoir lying to the west of this area.

The eastern part of the area is irrigated in the spring by using a part of the surplus water from Mill and Parleys creeks. The western part of the area has irrigation-rights during the summer and some small parts of the area have surface irrigation rights during the entire year.

The area studied was chosen because of the availability of existing ditches and ponding reservoirs which were not being used and because water from the Mountain Dell Reservoir on Parleys Creek was available. According to Gilbert [2], almost all the bench-land area bordering the west side of the Wasatch Mountains is very similar in character and therefore any results obtained from the area chosen for study can probably be applied to nearly all or any part of these benchlands extending from northern to central Utah.

Purpose of investigation--Ground-water is one of the most valuable natural resources of the

State, and of that area bordering the western face of the Wasatch Mountains in particular. It is found in natural storage-reservoirs which are annually replenished by the deep penetration of precipitation which falls on the recharge-areas and seepage from surface-streams which flow on or across the recharge-areas. Although these ground-water reservoirs are not inexhaustible, they will yield large quantities of water over long periods, if the ground-water is properly conserved. Furthermore, it is thought that most of these reservoirs can be artificially recharged and the amount of water available be thus materially increased.

As the population of the State increases, there will be an increasing demand upon the supply of ground-water. This has been the history of past years and there is every reason to believe that it will be the history of future years. Thus, anything that can be done to conserve or increase the supply of ground-water will be enriching the State by that amount. The studies described in this report were to determine the practicability and potential value of the use of surplus water to artificially recharge the ground-water reservoirs by spreading surplus water over permeable recharge-areas or by running it through ditches and into pits, from where it can be percolated downward to join the ground-water body.

Primarily because of the low annual precipitation from 1929 to 1934, inclusive, the flow from the springs and flowing wells throughout the State decreased considerably. During this period various plans were tried to increase and conserve the supply of water available for culinary use and irrigation. Numerous pumps were installed on wells to increase their discharge, and many new wells were drilled.

Much water was being lost by seepage from canals and ditches conveying water to the irrigated lands. Because of the decreased supply this loss was especially obvious in 1934, and in some areas, many of the main canals and ditches were lined with concrete. The loss from these ditches, as will be shown later, was probably a large factor in the recharge to the ground-water supply. The diversion of water from streams before they cross the ground-water recharge-areas and the lining of irrigation-canals to prevent conveyance-loss have undoubtedly decreased the recharge to the ground-water reservoirs, and some means to artificially increase this recharge should be planned.

During the spring of each year, having normal precipitation, a considerable amount of water is wasted into the Great Salt Lake. If this surplus water could be stored in the ground-water

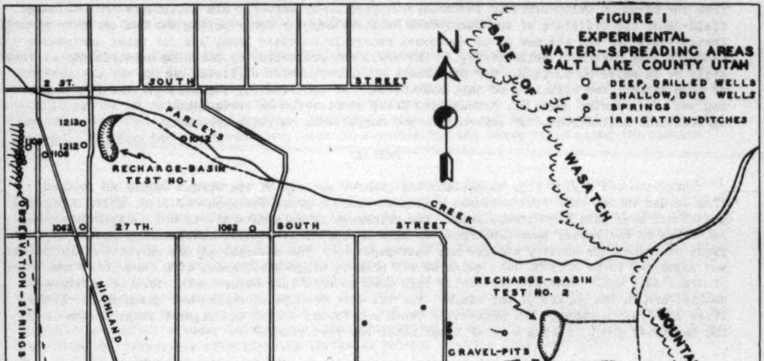


FIGURE I
EXPERIMENTAL
WATER-SPREADING AREAS
SALT LAKE COUNTY UTAH

N
○ DEEP, DRILLED WELLS
● SHALLOW, DUG WELLS
SPRINGS
IRRIGATION-DITCHES
BASE OF WASATCH MOUNTAINS
2 ST. SOUTH STREET
PARLEYS CREEK
1213
1212
1109
1108
1042
RECHARGE-BASIN TEST NO. 1
OBSERVATION-SPRINGS
1062
27 TH. SOUTH STREET
1062
HIGHLAND DRIVE
RECHARGE-BASIN TEST NO. 2
GRAVEL-PITS
33 RD. SOUTH STREET
SPRING FLOW LINE ALONG BASE OF PROVO BENCH
1052
MILL CREEK
39 TH. SOUTH STREET
SCALE
0
1 MILE

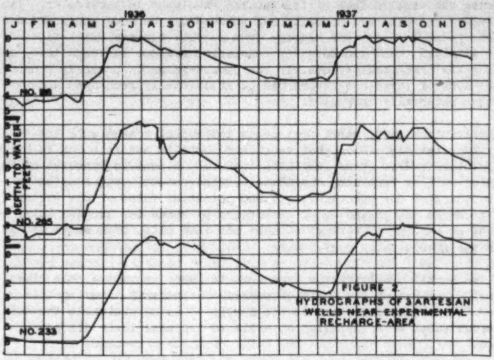


FIGURE 2
HYDROGRAPHS OF 3 ARTESIAN WELLS NEAR EXPERIMENTAL RECHARGE-AREA

basin it would be available later in the season when badly needed. Figure 2 shows the hydrographs of three wells located in the artesian basin near the area studied but at a lower elevation. Well No. 235 is located in the S.E. 1/4 of section 4, T. 2 S., R. 1 E., S.L.B. and M., Well No. 205; is located in the S.E. 1/4 of section 9, T. 2 S., R. 1 E., S.L.B. and M.; and Well No. 188 is located in the N.W. 1/4 of section 16, T. 2 S., R. 1 E., S.L.B. and M. As shown on these hydrographs, the ground-water level begins to rise about April of each year and reaches a maximum in July and August, when ground-water is most needed. The canyon streams usually supply enough water for culinary purposes and irrigation until the latter part of July, after which other means, such as reservoirs and wells, have to be depended upon to supplement the surface-supply. A series of experiments were made to determine whether or not artificial recharging of ground-water reservoirs in Utah was practicable.

Acknowledgements

Acknowledgements are due the late T. T. Burton for his help in obtaining permission to use available ditches and pits, and many helpful suggestions and assistance in the field; H. K. Burton, Superintendent of Salt Lake City Waterworks Department, for consenting to furnish water from the Mountain Dell Reservoir in Parleys Canyon; J. A. Ward for his assistance in obtaining field-data; the officials of the Utah State Penitentiary for contributing the use of their winter irrigation-water and the use of an abandoned reservoir on their land, and for other assistance rendered; the owners of wells living in the area, who cooperated by allowing measurements of their wells to be made; G. H. Taylor for his timely and constructive criticism during the preparation of this report; the officials of Salt Lake County for contributing a gravel-pit for flooding during the experiments; and W. W. McLaughlin, Chief of Division of Irrigation. U. S. Bureau of Agricultural Engineering, for information and suggestions on water-spreading.

Geology

Leggette and Taylor [1], summarized the general geology of the Jordan Valley as follows: "The Jordan Valley is a rock-bottomed valley in which a great thickness of clay, silt, sand, and gravel has been laid down irregularly. The thickness of this material is not definitely known, but wells in the Valley have penetrated as much as 2000 feet without encountering bed-rock. These sediments are chiefly stream- and lake-deposits. The material at the surface of the Valley was deposited in an ancient lake which at its highest stage stood about 1000 feet above the level of Great Salt Lake. The shore-deposits laid down in this lake occur in the form of terraces or benches around the margin of the basin. The two most prominent benches are known as the Bonneville and Provo benches. The Bonneville Bench was formed during the highest stage of the lake, and the Provo Bench, during a later stage about 400 feet lower."

The area selected for these tests lies principally on the Bonneville and Provo benches. The materials underlying this area consist primarily of the coarser sand and gravel ordinarily deposited by streams soon after emerging from the mouths of their canyons, combined with alternating layers of finer clay and sand laid down by stream- and lake-deposition--all of which were more or less reworked by littoral currents of Lake Bonneville to form interlocking fingers and larger irregular bodies of clay, sand, and gravel. A complete description of these stream- and lake-depositions is given by Gilbert [2]. The materials underlying the Bonneville and Provo benches are, in most instances, quite porous and ideal for the purpose of artificially recharging the ground-water basin, which lies beneath the bench-lands.

Reliable logs of six wells in and near the area considered in this experiment are available.

As shown from these logs, about 83 per cent of the material for the total depth of the well is a porous formation which could be filled with water. The percentages of materials in each well are given in Table 1.

Table 1--Percentages of materials in wells

Well No.	Depth in feet	Percentage			
		Sand	Gravel	Boulders and clay	Clay
1052	240	2	15	75	7
1054	240	0	0	97	3
1055	420	2	7	79	12
1062	535	0	28	36	36
1063	500	0	5	71	24
1064	500	1	58	18	23
Averages		1	18	64	17

In most cases the formations described as boulders and clay were porous and, if below the water-table, contained ground-water. Tests made of samples taken during the drilling of these wells and data obtained from the experiments indicate an average specific yield of the material of about 20 per cent. Assuming this to be correct, a rise of the water-table of five feet represents a storage of about one foot of water, which if occurring over the area included in this test, would give a storage of approximately 3200 acre-feet of water. The amount of water which can be stored by spreading it over the entire recharge-area at the base of the Wasatch Mountains probably equals or exceeds the amount of surplus water which now is being lost during heavy run-off-periods.

Source of ground-water

Ground-water in the Salt Lake Valley is derived principally from precipitation. The artesian reservoir is replenished by percolation of the rain and snow falling on the recharge-area or by seepage from streams and canals crossing the recharge-area.

Leggette and Taylor [1] make the following statements regarding the source of the underground water in the Jordan Valley: "The principal sources of ground-water in the Jordan Valley are the water that seeps into the ground from the streams entering the Valley, the water that penetrates directly from the rain and snow that fall upon the bench-lands on the east side of the Valley, and the water that percolated downward from irrigation-canals and from irrigated lands, chiefly derived from Utah Lake. In addition, some deep-seated thermal water rises along the Wasatch Fault.

"Some information as to the source of the ground-water can be obtained from its content of chloride, which is one of the constituents of common salt. Field-determinations were made of the chloride-content of the waters from about 140 wells. Nearly all the waters tested from the main part of the area of artesian flow extending from Salt Lake to Murray, have chloride-contents of less than 100 parts per million and most of them have less than 50 parts per million. The distribution of waters of different chloride-content indicates that the water of the main part of the area of artesian flow is derived chiefly from supplies that contain only small amounts of chloride--namely, seepage from the streams that head in the Wasatch Mountains and rain and snow that fall upon the Provo and Bonneville benches and penetrate downward through the permeable materials that underlie these benches--and that this artesian water is derived only to a minor extent from the relatively chloride-rich irrigated waters obtained from Utah Lake...

"The evidence obtained to date indicates that the principal area on which direct penetration of rain- and snow-water takes place is that occupied by the Provo and Bonneville benches. These benches are in most places sandy and permeable and hence are favorable for rapid penetration of the precipitation upon them. The nearly complete absence of drainage-channels on the benches is indicative of rapid penetration of the precipitation with no great amount of runoff. The greatest stream-losses from Mill Creek and Cottonwood creeks are believed to occur in the sections where these streams cross or cut through the Provo and Bonneville benches. It is estimated that the quantity of water that falls as rain or snow upon the Provo and Bonneville benches on the east side of the Valley, between Draper and City Creek is equivalent to an average of about 85 cubic feet per second. It is believed that a considerable part of this water reaches the underground reservoir. On the mountain sides above the benches, between the canyons of the principal streams are facet-shaped areas which drain on to the benches and not into the principal streams. The small drainage-systems on these facets are well developed, indicating considerable runoff. The small streams that discharge on to the benches have built small alluvial cones which, together with the material making up the benches, are favorable for absorbing and transmitting the storm-waters that are occasionally poured upon them. It is estimated that the quantity of water that falls upon the mountain facets tributary to the benches between Draper and City Creek is equivalent to an average of about 28 cubic feet per second. Of course, only a part of this water is discharged upon the benches and is eventually added to the ground-water supply. Although conclusive data as to the quantity of water that penetrates the benches are not available. the evidence indicates that this is a larger source of ground-water supply than the seepage from the principal streams.

"A considerable amount of water is contributed to the ground-water reservoir by seepage from irrigation-canals and ditches and from the water applied to the soil in irrigation. Recharge from this source is apparent from the facts that the water-levels in numerous wells rise when the canals are filled in the spring and drop when the flow through the canals ceases in the fall, and that a considerable net rise has occurred in many wells which were dug or drilled before much water was carried by canals from Utah Lake. The area in which this effect has been most notable is in the southern part of the Valley, where numerous canals run at higher levels than most of the existing wells. It is doubtful, however, whether much of the ground-water derived from irrigation-supplies reaches that part of the ground-water reservoir which is drawn upon by the artesian wells in the most heavily developed area in the east-central part of the valley."

Test No. 1

An abandoned reservoir, owned by the Utah State Penitentiary, was chosen for the first experiment. This reservoir was originally constructed to impound water for irrigation-purposes, but it failed because of leakage from the reservoir due to the character of the underlying materials. For the same reason it was ideal for ground-water recharging experiments. Also, water from the Mountain Dell reservoir in Parleys Canyon and suitable carrying canals and ditches were available. The reservoir is located about three miles west of the base of the Wasatch Mountains and about 2000 feet east of the natural spring-flow line. The wells and springs selected for observation were located, so far as possible, to study the movement of the ground-water in all directions from the reservoir (see Fig. 1). Their location with respect to the reservoir is shown in Table 2.

Table 2--Location of wells, Test No. 1

Well No.	Location from reservoir	Well No.	Location from reservoir
1213	About 600 feet west	1063	About 2800 feet south
1212	About 800 feet southwest	1042	About 2600 feet east
1108	About 1500 feet southwest	1062	About 5300 feet southeast
1109	About 1900 feet southwest	All springs	About 2000 feet west

Procedure followed--A flow of water of 2 1/2 cubic feet per second was turned into the reservoir on January 14, 1936, and on January 16, 1936, the flow was increased to 4 1/2 cubic feet per second. This flow was continued, except when partially interrupted for short periods by freezing, until February 18, 1936, when the entire flow was discontinued to determine the movement of the water-table before natural recharge began. This period of the year was chosen because the operations would not be unduly influenced by natural ground-water recharge, by seepage from Parleys Creek, or near-by irrigation. All observation-wells and springs were measured at frequent intervals for a month or more before the water was turned into the reservoir, to determine the general trend of the ground-water levels. The measurements were continued throughout the test and until interrupted by the natural spring recharge.

Results in wells--The water-level in most of the observation-wells began to rise soon after the water was turned into the reservoir. The rise of the water-level lagged approximately in proportion to the distance from the reservoir. As shown by Figure 3, the rise of the water-table was governed by the amount of water entering the reservoir. When ice diverted part of the water for short periods, the effect on the water-level in wells 1212 and 1213 could be seen within a short time. The effect on wells 1108 and 1109 began five or six days later than the effects on wells 1212 and 1213. The water-level in observation-well 1042, located east of the reservoir, began to rise as soon as the water in wells 1212 and 1213, even though it is located at a greater distance from the reservoir. This

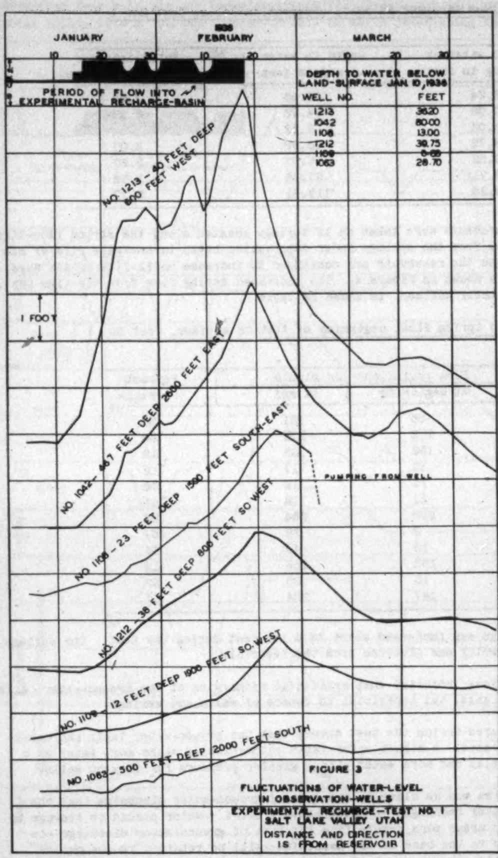


FIGURE 3

FLUCTUATIONS OF WATER-LEVEL IN OBSERVATION-WELLS EXPERIMENTAL RECHARGE-TEST NO. 1 SALT LAKE VALLEY, UTAH

DISTANCE AND DIRECTION IS FROM RESERVOIR

may have been caused by the seepage from the feeder-ditch, which passes close to Well 1042, by the backing up of water by the ground-water dam formed by seepage from the reservoir, or by a combination of both effects.

It is estimated that a continuous stream of four cubic feet per second running for 31 days, or about 245 acre-feet, will equal the amount of water put into the reservoir during the test. The effect of sinking this water was observed on the ground-water level over an area of about 400 acres. The maximum rise of the ground-water level, 7.58 feet, occurred in well 1213. Assuming 400 acres as the area of the base of an irregular cone with the height of eight feet at the apex, the volume of earth filled with water during the test would be about 46,464,000 cubic feet. The volume of water put into the ground was about 10,672,000 cubic feet, but about 0.5 cubic feet seeped from the reservoir back into Parleys Creek, leaving approximately 9,300,000 cubic feet of water to fill this volume of earth.

Therefore, for each cubic foot of water put into this area, there would be about five cubic feet of earth filled with water. If the 9,300,000 cubic feet of water had been placed evenly over the 400 acres it would have raised the water-table approximately 2.66 feet. From these approximate figures it can be seen that very large quantities of water can be placed in the ground without raising the water-table any great amount.

When the flow into the reservoir was discontinued, the water-level in the observation-wells began to decline, the lag in this decline in the wells at greater distances from the reservoir occurred in about the same relationship as when the water was turned in. This decline was rapid at first and gradually became slower. By the time the natural recharge of the creeks and irrigation occurred there was little change in the water-level between successive measurements. As shown by the curves, Figure 3, the water-level in the observation-wells became practically stationary, or assumed their normal trend, at a level about one foot higher than they were when the test began.

Table 3 gives the maximum increase in head in the observation-wells.

Table 3--Maximum increase in head, observation-wells, Test No. 1

Well No.	Depth to water at beginning in feet	Depth to water at end in feet	Total increase in feet
1213	36.24	28.66	7.58
1042	80.02	74.73	5.29
1108	13.03	9.52	3.51
1212	39.76	36.76	3.00
1109	8.83	5.03	3.80
1063	28.71	27.63	1.08
1062	118.28	117.51	0.77

Results on springs--Measurements were taken on 12 springs located along the spring flow-line, as shown in Figure 1. The flow from the springs under observation began to increase five or six days after water was turned into the reservoir and continued to increase until five or six days after the water was diverted as shown in Figure 4. The increased spring flow from the time the test began until they reached their maximum, is shown in Table 4.

Table 4--Increased spring flow, beginning of test to maximum, Test No. 1

Spring No.	Flow in gallons per minute		Percent increase
	At beginning	At end	
W-11	66	101	53
W-12	178	273	53
W-24	159	188	18
W-24-1	12	17	42
W-24-3	14	19	36
W-24-4	11	18	64
W-25	286	354	23
W-36	7	39	457
W-42	13	40	54
W-59	135	155	15
W-60	15	23	53
W-63	167	224	34

The total flow from these springs was increased about 36.5 per cent during the test. The springs dropped in flow soon after the water was diverted from the reservoir.

Conclusions--The data obtained indicated that artificial recharging of the ground-water basin is not only possible and practicable, but beneficial to owners of wells and springs.

The wells and springs measured during the test showed that the ground-water table had been raised and the spring flow increased. A higher water-table automatically means more water at a lower pumping cost for pumped wells and more water with a greater pressure for flowing wells.

The location of the test-area was so close to the zone of ground-water discharge that the storage-period of some of the water recharged is comparatively short. Water placed in storage by artificial recharge-methods over areas more remote from the zone of ground-water discharge--in this instance, over areas closer to the base of the mountains--will be retained in the ground longer. Because of the relatively slow movement of ground-water, it would probably appear at the zone of ground-water discharge several months and possibly a year or more after it was spread over the area recharged. Thus it is desirable to use spreading areas as high upon the ground-water recharge-area as is possible.

Test No. 2

An abandoned county gravel-pit located near the upper edge of the ground-water recharge-area was chosen for test No. 2. Water for this test was primarily obtained from Parleys Canyon, although some was obtained from Mill Creek. The canals and ditches used for conveying the water to this pit are used to carry irrigation-water during the season of irrigation. The pit is located on 33rd South Street about 4000 feet west of the base of the Wasatch Mountains (see Fig. 1). It has an area of about two acres and a depth of about 24 feet. Only a few wells have been developed near this pit and those available for measurement were shallow and probably receive their supply from a "perched" water-table. No wells penetrating the main ground-water table could be located near the pit.

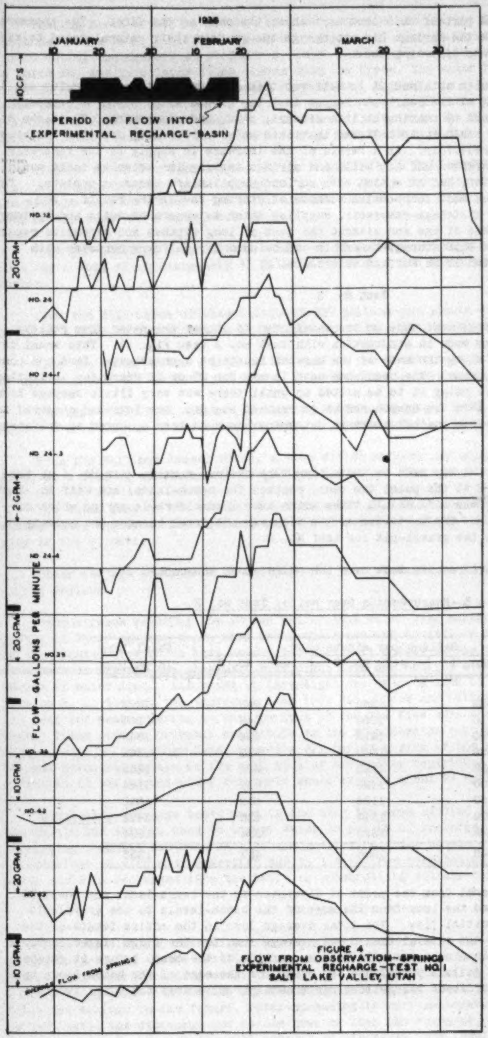


FIGURE 4
FLOW FROM OBSERVATION-SPRINGS
EXPERIMENTAL RECHARGE-TEST NO. 1
SALT LAKE VALLEY, UTAH

Procedure followed--Water was turned into the pit on March 9, 1936, and ran continuously until April 13, 1936, at which time it was diverted to supply water for irrigation. The average flow was about six second-feet, or a total of about 417 acre-feet, during this period. A similar test was made on the same and additional near-by pits between October 17 and December 31, 1936, and discontinued because of excessively cold weather making it impossible to get water through the ditch. The average flow was about three second-feet, or approximately 447 acre-feet, during the period. The observation-wells and springs were measured at frequent intervals, both before and after, as well as during the tests, in order to detect any possible movement of the water which might be caused by the experiment.

Results--The water-level in the pit during the first part of this experiment rose consistently at the rate of about six inches per day, until it reached a depth of about 12 feet. The depth then increased at the rate of about four inches per day until the water was turned out of the pit. The water-levels in the wells or the flow from the springs did not change their natural trend during this experiment. The water feeding these wells and springs was probably supplied from a perched water-table. The bottom of the pit was about 16 feet below the impervious clay-layer forming this perched water-table and the water run into the pit during this experiment naturally seeped to a lower water-table. This water, by the time it had reached the lower table, and traveled to the nearest deep well, had either spread over such a large area or had moved so slowly that any increased head could not be determined before the natural recharge began to raise the water-level in the wells.

The results in the second part of this test were about the same as the first. The pressure in the wells and the flow from the springs did not change enough from their natural trend to tell whether or not this recharge was affecting them.

Conclusion--From the results obtained it is believed that any amount of surplus water available, especially in the spring of the year, can be run into the ground-water basin by spreading it over the high bench-lands and by running it into ditches, pits, and reservoirs. By reason of the structure of the artesian basin it is believed that this water percolating downward contributes to the water in the artesian reservoir. As a result of the increase in supply to the reservoir, the artesian head would be increased and the wells and springs tapping the artesian basin would yield larger quantities of water, and at a time when surface supplies are becoming depleted. This is probably one of the best and most inexpensive methods of storing the spring runoff. The ground-water basin, acting as a storage-reservoir, supplies water to owners of wells and springs over the entire Valley at points of use and without the cost of long ditches and expensive regulating equipment. The water from this storage-reservoir can be used for culinary purposes with practically no fear of pollution as in surface reservoirs.

Test No. 3

The third test was a seepage test made on the canal used to convey the water from Parleys Creek to the gravel-pit and was made in conjunction with test No. 2 (see Fig. 1). This canal is about two miles long and runs along the base of the Wasatch Mountains over alluvial fans and cones where the materials are very porous. The canal has been in use for 20 or 25 years for irrigation-purposes and was assumed by all using it to be silted up until there was very little seepage loss. By measuring the stream as it left the canyon and as it reached the pit, the loss was observed to be so great that it was decided to install weirs to determine how much loss occurred in different sections of the canal.

Procedure followed--The test was made by installing three weirs, namely: Weir No. 1 at the intake of the canal; Weir No. 2 at the point the canal reaches the bench-lands; and Weir No. 3 at the gravel-pit. A close check was made on all three weirs over a considerable period which covered flows of different amounts. The variation of the flow was not great because the primary purpose was to convey water to the gravel-pit for test No. 2.

Results--Table 5 shows measurements made over the weirs along the canal.

Table 5--Measurements over weirs, Test No. 3

Date	Gallons per minute over			Total loss in gallons
	Weir No. 1	Weir No. 2	Weir No. 3	
1936				
Oct. 28	1838	1360	545	1293
30	1910	1360	545	1365
Nov. 5	1980	1412	438	1542
9	1838	1340	536	1302
19	1838	1193	573	1265
23	1700	1198	536	1164
25	1700	1198	438	1262
Dec. 1	1919	1340	438	1472
5	1838	1340	334	1504
Averages	1840	1305	487	1353

The average loss in the canal from the point of diversion to the bench-lands was about 29 per cent of the initial flow and the loss from the edge of the bench-levels to the gravel-pit was about 63 per cent of the initial flow. The total average loss in the entire length of the canal was about 74 per cent of the initial flow. The average loss for the flows listed in Table 5 would be about 550 gallons per minute per mile for the portion of the canal before it reaches the bench-lands, and about 800 gallons per minute per mile from the edge of the bench-lands to the gravel-pit, or an average of about 767 gallons per minute per mile over the total length of the canal.

A seepage test made on Big Cottonwood Creek near the area studied by Ward and McDonald [3] is as follows:

"During March of 1937 a seepage test was conducted on the Big Cottonwood Creek channel between Salt Lake City, Big Cottonwood Creek intake, and 62nd South Street. The water had been turned out of the channel on December 2, 1936, and turned back in the channel on March 8, 1937. On March 26, due to a large flush coming down the Creek, the water reached 62nd South Street, which is approximately two miles below the intake. During this time there had seeped into the creek bed 236 acre-feet of water, or an average flow of 6.62 cubic feet per second. Ordinarily a continuous flow of this amount could flow over the weir at the Big Cottonwood intake and would probably never reach as far down the creek as 62nd South Street."

Seepage tests on other ditches in the vicinity were made during 1934 to determine whether or not the seepage loss was great enough to justify lining them with concrete. In almost all cases the seepage loss was found to be large and the ditches were lined with concrete. The results of some of the tests were as follows:

From the Stillman Hussy Ditch, a loss of 67 gallons per minute over a length of about 5000 feet, or a loss of approximately 71 gallons per mile. The measurements were made by George F. Taylor.

From the Mill Creek ditches a loss of 359 gallons per minute over a length of about 7500 feet, or a loss of approximately 71 gallons per mile. The measurements were made by J. B. Cahoon.

From the Keller Ditch a loss of 157 gallons per minute over a length of 7200 feet, or a loss of approximately 115 gallons per mile. The measurements were made by Logan Wisseman.

The East Mill Creek ditches, including Brigham Young and Neff, a loss of 606 gallons per minute over a length of about 16,500 feet, or approximately a loss of 194 gallons per mile.

From the Stillman Russel Ditch, a loss of 105 gallons per minute over a length of about 6000 feet, or a loss of approximately 92 gallons per mile. The measurements were made by Hinckley.

The total loss in the ditches located in the area studied, and which were lined with concrete during 1934, was about 1250 gallons per minute per mile, the actual amount depending on the total flow in the ditches.

Other seepage tests made in and near the area studied are described by Leggette and Taylor [1] as follows:

"The streams entering the Jordan Valley lose water over portions of their channels below the mouths of their canyons where they cross the Provo and Bonneville benches. However, the length of the channel from which loss occurs is relatively short, as was shown by tests of the seepage loss made on some of the streams. Sufficient data are not yet available to estimate the total amount of water lost. According to investigations made in 1902 by E. R. Morgan and in 1925 and 1928 by R. C. Towler, the aggregate loss from Mill Creek and Cottonwood Creek was about nine cubic feet per second during certain periods of the low flow when the seepage investigations were made. These streams probably contribute to the ground-water supply at a more rapid rate at high stages, but, on the other hand, they are dry during a part of the year. If it were assumed that the six principal creeks on the east side of the Valley contribute like amounts, their total contribution to the ground-water reservoir would average about 27 cubic feet per second."

Conclusion--Seepage tests made in and near the area studied in this experiment indicate that the canals and ditches used to convey water to points of recharge, or for irrigation-use, act as recharging conduits the same as pits and reservoirs. The canals and ditches would not be quite as efficient as pits and reservoirs due to losses from evaporation and transpiration, but much water could be recharged into the basin by constructing ditches for this purpose.

General Conclusions

From the combined results of the experiments made, there appears to be no question that the ground-water basin can be artificially recharged by surface-runoff, and that the increased recharge from water spreading would increase the yield of the wells and springs accordingly. Also, that if the ground-water basins are to continue to supply the increasing demand for water from wells and springs in the future, water-spreading is very necessary. Water-spreading should not be left until the discharge has become greater than the recharge, but should be begun at once. With proper study of recharge with respect to discharge, the level of the water in the ground-water reservoir can be controlled until there would be practically no danger of drawing more water from the ground-water reservoir than the recharge will supply.

The ground-water reservoir, when compared with surface-reservoirs, for storing surplus water has many advantages. There is negligible loss by evaporation or transpiration, little danger of contamination, and in comparison with the acre-foot cost of most surface-reservoirs, the cost of underground storage is small.

Water-spreading has a definite place in the ground-water supply of the State of Utah, and a more complete study should be made at an early date to determine other problems and factors not encountered in this experiment.

Summary

In the first test about four cubic feet per second of water, for a period of 31 days, was run into an abandoned reservoir near the lower edge of the recharge-area. A rise in the water-table was measured over an area of about 400 acres. All observation-wells and springs in this area were increased in head and flow, respectively, by this experiment. The maximum rise of the water in the observation-wells was 7.58 feet. The flow from the springs was increased approximately 36 per cent over the original flow.

In the second test, about six cubic feet per second of water, for a period of 35 days, was run into an old gravel-pit near the upper edge of the recharge-area, or near the base of the Wasatch Mountains. The deep observation-wells were so far away, and the volume of earth to be filled so great, that any change in the level of the water-table could not be detected by the observations made.

In the third test the seepage losses from the supply-canals were found to be very high. The loss in the supply to the gravel-pit was about 74 per cent of the total flow over a distance of two miles. Other ditches and canals in the area had similar losses according to their size and location.

The results obtained in this experiment indicate that artificial recharging of the ground-water basin is probably more economical, more beneficial, and more practical than many surface-reservoirs for storing surplus runoff.

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GROUND-WATER FOR AIR-CONDITIONING ON LONG ISLAND, NEW YORK

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(Published with the approval of the Director, U. S. Geological Survey)

Introduction--During the last five years ground-water has been more and more extensively used for air-conditioning on Long Island, New York. The wide-spread occurrence of highly permeable water-bearing material and the relatively small cost of installation and operation of a ground-water, air-conditioning system has resulted in many such installations by theaters, restaurants, stores, and other establishments. Many companies likewise have taken advantage of these favorable factors and are utilizing low-temperature ground-water for cooling purposes in the manufacture of ice. This new use of ground-water has considerably increased the draft in the western end of Long Island where there had already been so much over-development that the water-table was below sea-level in an area of more than 40 square miles. In 1933 the State Legislature, recognizing the seriousness of this over-development, passed a law requiring that the approval of the State Water Power and Control Commission be secured before constructing a well with a capacity greater than 100,000 gallons a day. Since the passage of the law the policy of the Commission has been to require water pumped from new wells for cooling purposes to be returned to the ground. This requirement has resulted in the construction of many recharge-wells (locally called diffusion-wells) through which the warm water is returned to the ground.

A typical ground-water, air-conditioning system consists of an ordinary supply-well from which water with a temperature of about 55°F is pumped by a deep-well turbine-pump. After passing through a series of cooling coils, the water, with an increase in temperature of about 10 to 15°F, is piped to the recharge-well into which it is discharged below the water-level. Most of the systems operate about ten hours a day during the summer. The air-conditioning season normally begins about the first of June and ends about the end of September.

Table 1 gives estimates of the number of recharge-wells in operation on Long Island and the quantity of recharge for the last five years.

Table 1--Recharge of ground-water in Kings and Queens Counties, New York

Year	Estimated number of recharge-wells operating at end of year	Estimated rate of recharge in gallons per minute	Estimated quantity of recharge in gallons per day	
			Air-conditioning season	Non-air-conditioning season
1933	1	60	36,000	29,000
1934	12	2,135	1,200,000	45,000
1935	24	5,780	3,260,000	45,000
1936	67	23,245	17,835,000	6,814,000
1937	105	33,385	22,124,000	6,814,000

Thirteen of the recharge-wells operating in 1937 were in use during the entire year for cooling purposes in the manufacture of ice. Most of the other recharge-wells were used only during the air-conditioning season. During the summer some of the larger installations, both air-conditioning and ice-manufacturing, use as much as 1000 gallons a minute. It is apparent from Table 1 that the use of ground-water for cooling purposes on Long Island is rapidly increasing. This trend is further indicated by the fact that the Water Power and Control Commission during 1937 acted on more than 100 applications for wells to be used for cooling or air-conditioning.

Recharge-wells--A recharge-well may be defined as any well through which water is introduced into the ground. Its operation is similar to that of an ordinary supply-well except that the flow of water is in the opposite direction. When water is introduced into a recharge-well the head in the well is increased. Thus, in the area surrounding the well, a cone on the water-table is produced. This cone is somewhat similar in form to the cone of depression that exists around a pumped well except that the apex of the cone is above the water-table rather than below it. Where an air-conditioning recharge-well and a supply-well are so close to each other that their respective cones overlap, some of the warm water being discharged from the recharge-well will be drawn into the supply-well. The rapidity with which this takes place depends chiefly on the physical properties of the water-bearing material, the distance between the two wells, and the hydraulic gradient between the two wells.

Many types of recharge-wells have been constructed on Long Island since the first wells were drilled. Much progress has been made in improving methods of construction although none of the methods is completely successful everywhere. It is now realized that a great deal of care must be given to the details of the construction of a recharge-well. Some of the first recharge-wells were merely pits that ended above the water-table. Most of these were unsuccessful because the openings near the bottom of the well became clogged. The general practice at the present time is to finish the recharge-well below the water-table. The wells are generally finished with a section of large-diameter pipe into which slots have been cut. Recently several recharge-wells have been constructed with standard spiral-wound well-screens. Some of the contractors for wells have used the gravel-wall type of construction with considerable success. Many of the newer wells are subjected to a period of development similar to that used for supply-wells.

One of the most troublesome problems that arises in operating a recharge-well is a gradual clogging of the well-screen or perforated pipe. When this takes place the well is no longer capable of passing the required amount of water. This clogging probably results either from chemical incrustation, mechanical clogging by particles of silt or pipe-scale, or by a combination of both processes.

In those types of recharge-wells where the water is discharged above the water-level, at least three processes take place that tend to favor incrustation, namely: The pressure on the water is reduced as it leaves the end of the discharge pipe; some of the dissolved gases are released; and the water comes in contact with the oxygen of the air. All of these factors tend to precipitate some of the dissolved mineral matter in the water. The change in temperature that presumably takes place as the water leaves the recharge-well is probably not effective in causing incrustation. Recharge-wells in which the water is discharged below the water-level have generally been more successful than the type mentioned above, presumably because there is less opportunity for incrustation.

The presence of silt or other particles in the water introduced into the recharge-well is obviously objectionable. The coarser particles may sink to the bottom of the well and even fill the well to a level above the top of the screen. Other particles may clog the screen-openings, particularly if the screen used is of the type intended for a supply-well. Still other particles may pass through the screen and be deposited in the pore-spaces of the water-bearing material, thus decreasing its permeability. It is, therefore, very important that a supply-well be adequately developed to remove all sediment, if the well is intended for use in connection with a recharge-well. On casual observation a supply-well may appear to be pumping clear water after a moderate period of development, when in reality the water may still contain a small amount of silt. If the well is then put in operation in connection with a recharge-well, this small amount of silt will pass through the cooling coils, enter the recharge-well, and either clog the screen or the sand and gravel surrounding the screen. Even though the amount of silt in the water is small, the aggregate may constitute a considerable quantity. For example, if the water being pumped from a typical supply-well on Long Island contained only one ounce of silt in every 100 gallons, more than eleven tons of silt would enter the recharge-well in one air-conditioning season.

Ground-water temperature--In most areas where the natural ground-water level is not too near the surface, the temperature of the ground-water is fairly constant throughout the year. This temperature is generally within a few degrees of the average annual air-temperature. Prior to

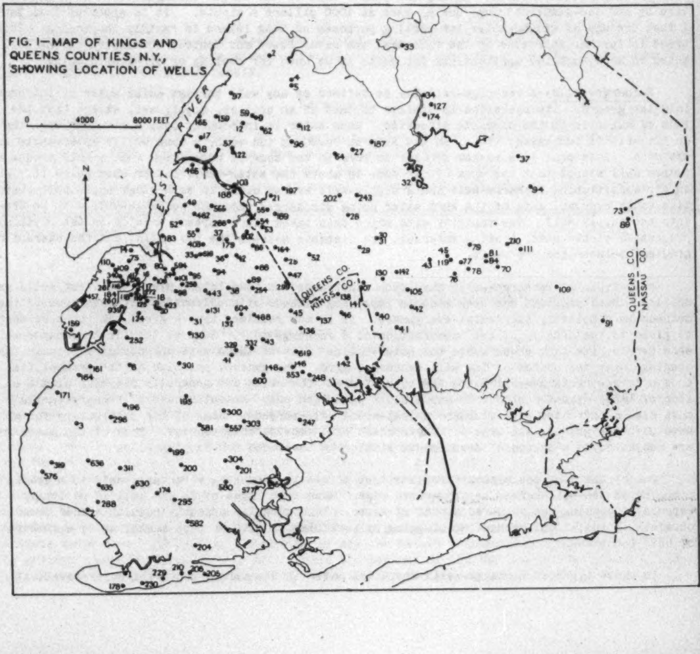

FIG. 1—MAP OF KINGS AND QUEENS COUNTIES, N.Y., SHOWING LOCATION OF WELLS

the introduction of warm water into a ground-water reservoir a condition of equilibrium exists between the amount of heat received from the Earth (and atmosphere) and the amount of heat lost to the atmosphere by radiation. With the introduction of warm water, this equilibrium is disturbed and a net rise of ground-water temperature would seem to be inevitable. The rate and amount of this rise of ground-water temperature depend on a large number of variable and complex factors. Obviously, the distribution of recharge-wells, the temperature of the returned water, and the quantity of such recharge are some of the most important factors. The physical properties of the water-bearing material, its permeability, its continuity both vertically and horizontally, likewise determine to a great extent the rapidity and amount of rise of temperature.

Anticipating a probable rise of ground-water temperature in the western end of Long Island as a result of the large quantities of warm water being returned to the ground, the Geological Survey in 1936 began a program of periodic observations of ground-water temperature. This work is being carried on as a part of the ground-water studies on Long Island that are being made in cooperation with the New York State Water Power and Control Commission and with Nassau and Suffolk counties. At the present time observations of ground-water temperature are being made at 192 supply-wells and at five recharge-wells (see Fig. 1). The temperature at 24 of the supply-wells and at three of the recharge-wells is being observed twice each month. Observations at the other wells are made three times a year, usually in April, in July and August, and in December. Some of the observation-wells are located near recharge-wells but many of them are in areas where no warm water is being returned to the ground.

The observations of temperature are made with calibrated 16-inch mercury thermometers graduated in tenths of a degree from 30 to 80°F. The thermometer is read by means of a magnifying tube that is held at right angles to the stem of the thermometer by a spring-clip. After the pump has been operated for a considerable time, a sample of water is drawn from a petcock on the pump discharge-pipe, in most cases less than five feet from the pump. The water-sample is taken in an ordinary one quart thermos bottle that has previously been thoroughly rinsed with the water to be sampled. The thermometer is then lowered into the thermos bottle and the temperature observed. Tests run to determine the extent to which the water-sample is insulated by the thermos bottle indicate that no discernible change in temperature takes place until about ten minutes have elapsed. The room-temperature is also observed but no correction is made for emergent stem, as this amounts to only about 0°.01 in most cases. The observations are believed to indicate the true temperature of water in the ground to within a few tenths of a degree. A few of the pumping plants are operated automatically and at the time of sampling it is not known how long the pump has been operating. Some of the observations at these wells may be considerably higher than the true temperature of the water in the ground.

Although the information so far obtained on Long Island covers a period of less than two years, certain general features are apparent. As the studies progress and the period of record becomes longer, slowly moving trends will doubtless become evident.

Tables 2 and 3 give pertinent data on some of the supply-wells and recharge-wells on Long Island.

Figure 2 shows temperature graphs for Well K131 (supply-well) and Well K314 (recharge-well). The wells are 300 feet apart and are located more than a mile from the nearest recharge-well. The two wells are operated throughout the year for cooling purposes in the manufacture of ice. The recharge-well was put in operation about December 1936, but observations of temperature were not started until March 1937. As might be expected, the temperature-graph for the recharge-well is more irregular than that of the supply-well. During 1937 an average of about 1,118,000 gallons a day of warm water was returned to the ground through the recharge-well. This resulted in a net rise of temperature of the water withdrawn from the supply-well of almost 9° during the year. A further rise doubtless may be expected as recharge of warm water continues.

The temperature-graph of Well K195 is shown in Figure 2. This well is operated throughout the year for cooling purposes in the manufacture of ice, but the warm water is not returned to the ground because the well had been in operation a number of years before the return of such water was required. However, about 1,200 feet to the north there are four recharge-wells located in an area of about 300 by 600 feet. These wells were operated only during the air-conditioning season of 1937, the estimated average combined recharge being about 456,000 gallons a day. Although the temperature-graph of Well K195 indicates a slight rise as a result of this recharge, the net change during the year was negligible.

Figure 3 shows the location of recharge-wells and some of the supply-wells in the area of most concentrated development in 1937. It is apparent from this map that a rise of temperature

Table 2--Supply-well data

Well No.	Altitude of bottom of well below sea-level in feet	Estimated average daily yield during 1937 in gallons per day		Distance to nearest recharge well in feet	Net change of ground-water temperature during 1937 air conditioning season in °F
		Air-conditioning season	Non-air-conditioning season		
K11	61	20,000	20,000	550	+0.2
K16	61	504,000	0	550	-0.4
K17	65	576,000	0	200	0.0
K18	66	180,000	0	1,100	-0.8
K21	74	336,000	336,000	3,200	+0.3
K106	69	180,000	180,000	2,600	+0.9
K110	67	1,000,000	540,000	1,800	--
K116	48	180,000	180,000	500	--
K118	63	780,000	390,000	600	0.0
K125	60	72,000	0	2,000	--
K131	93	1,440,000	1,440,000	300	+0.2
K184	65	210,000	0	300	-2.5
K195	39	1,000,000	1,000,000	1,000	-0.3
K260	69	126,000	0	400	+6.8
K261	25	270,000	0	800	+0.3
K272	57	210,000	0	150	+1.5
K276	72	528,000	288,000	100	--
K340	72	195,000	0	150	+2.9
K939	118	360,000	360,000	3,000	0.0

Table 3--Recharge-well data

Well No.	Altitude of bottom of well above (+) or below (-) sea-level in feet	Estimated average daily recharge during 1937 in gallons per day	
		Air-conditioning season	Non-air-conditioning season
K314	-108	1,118,000	1,118,000
K901	-55	210,000	0
K905	-38	120,000	0
K906	-30	108,000	0
K932	-103	390,000	195,000
K933	-103	390,000	195,000
K934	-26	126,000	0
K935	-55	528,000	288,000
K936	-10	264,000	0
K937	-23	240,000	0
K938	+6	195,000	0
K942	-24	150,000	0

in any one supply-well may be the result of recharge from a number of adjacent wells. The average recharge from all wells in this area during the 1937 air-conditioning season is estimated to be about 2,700,000 gallons a day. During the remainder of the year the recharge was about 700,000 gallons a day.

The rise of temperature shown by the graphs for wells K260 and K340 in Figure 2 is fairly typical for supply-wells in this area. These two wells began operation a few weeks before the first observation shown in Figure 2. The decline in water-temperature of Well K184 during June and July 1937 took place when a nearby recharge-well was shut down for repairs.

Well K118 is about 600 feet from the nearest recharge-well. The temperature-graph of this Well shows a net rise of water-temperature of about one-half degree during 1937.

Wells K106 and K939 are each more than one-half mile from the center of the group of recharge-wells shown in Figure 3. The temperature-graphs of these two supply-wells show the effect of the recharge during the air-conditioning season (see Fig. 2). During the year there was a slight net rise of water-temperature in both these wells. However, until a longer period of record is available it should not be definitely concluded that the rise of temperature has spread radially to a distance of more than one-half mile from the center of recharge.

There are many unexplained temperature-relations shown by the data so far obtained on Long Island. Some of these doubtless will be better understood as the period of record becomes longer. However, there are certain factors that will always be difficult to evaluate.

The temperature of water from some of the supply-wells has shown no rise even though they are located relatively near a recharge-well. At least in some cases this is because, contrary to regulations, no warm water has been returned through the recharge-well, the well having become clogged, or the warm water discharged to the sewer to prevent excessive temperature rise of supply-well water.

From a study of the available well-logs it is apparent that many of the supply-wells end in

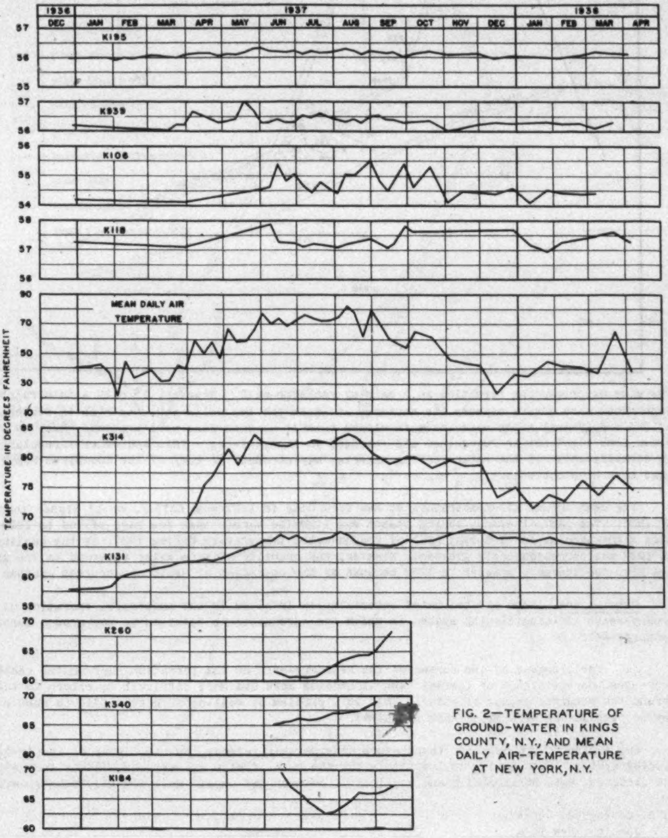

FIG. 2—TEMPERATURE OF GROUND-WATER IN KINGS COUNTY, N.Y., AND MEAN DAILY AIR-TEMPERATURE AT NEW YORK, N.Y.

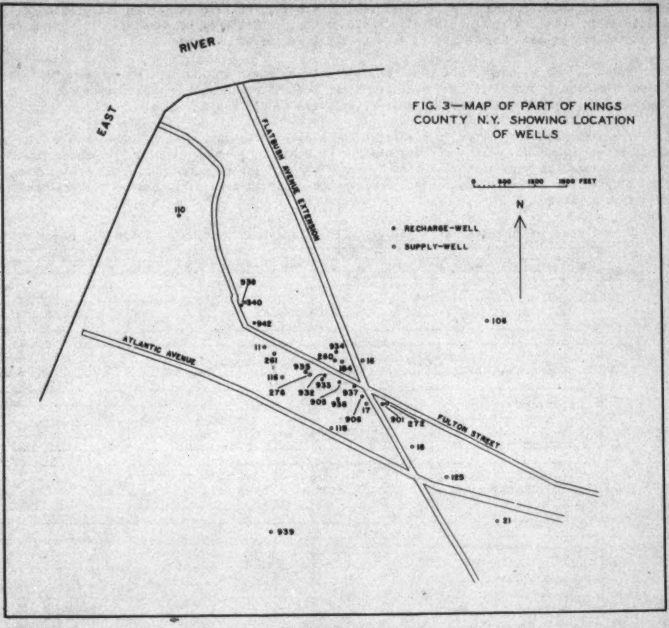
RIVER
EAST
FLATBUSH AVENUE EXTENSION
ATLANTIC AVENUE
FULTON STREET
0 600 1200 1800 FEET
N
• RECHARGE-WELL
• SUPPLY-WELL
110
938
940
942
11
261
116
276
935
932
933
905
936
937
934
260
184
16
17
906
118
901
272
18
125
106
21
939

FIG. 3—MAP OF PART OF KINGS COUNTY N.Y. SHOWING LOCATION OF WELLS

the same water-bearing formation as a near-by recharge-well. However, if only a thin relatively impervious zone occurs between the horizons at which the two wells end, the rise in temperature of the supply-well water would be much slower than if no such zone were present. Such an impervious zone might not be recognized and recorded in the well-log. This and other irregularities in stratification of the water-bearing material may account for many of the anomalous results that have been obtained.

The mean annual air-temperature at New York City in 1937 was 53°.9 F, or 1° higher than that of 1936. The 1937 air-conditioning season was likewise warmer than the same period in 1936. This might account for a slight rise of ground-water temperature during 1937, as the cooling load in 1937 was correspondingly greater, However, the quantity of warm water returned to the ground was also considerably greater in 1937 because of the operation of newly constructed systems.

Conclusions--There are two chief uncertainties involved in the successful operation of a ground-water air-conditioning system in which the warm water is returned to the ground through a recharge-well.

(1) The clogging of the screen of the recharge-well or the formation surrounding the screen, decreases the efficiency of the well and it becomes more and more difficult to return to the ground the required amount of water. This is a problem of well-construction that to some extent may be overcome as more experience is gained.

(2) A rise of ground-water temperature considerably reduces the efficiency of an air-conditioning system. Such a rise is inevitable but the rate of rise may be decreased by increasing the distance, both horizontally and vertically, between the supply-well and the recharge-well.

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GROUND-WATER UNDERFLOW IN CROTON VALLEY, NEW YORK: A COMPARISON OF FIELD- AND LABORATORY-METHODS

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Introduction--The area under consideration lies wholly within the narrow valley of the Croton River between New Croton Dam and Quaker Bridge (see Fig. 1). New Croton Dam is a masonry structure of the gravity-type, which was completed during the early part of the present century. The reservoir has a capacity of 30 billion gallons and is part of the municipal supply-system of the City of New York. Quaker Bridge is about two miles above the point of confluence of Croton and Hudson rivers. The maximum width of the floor of the Valley is about 500 feet. The old bedrock valley is cut to a depth of about 100 feet below the present stream-level and is filled with

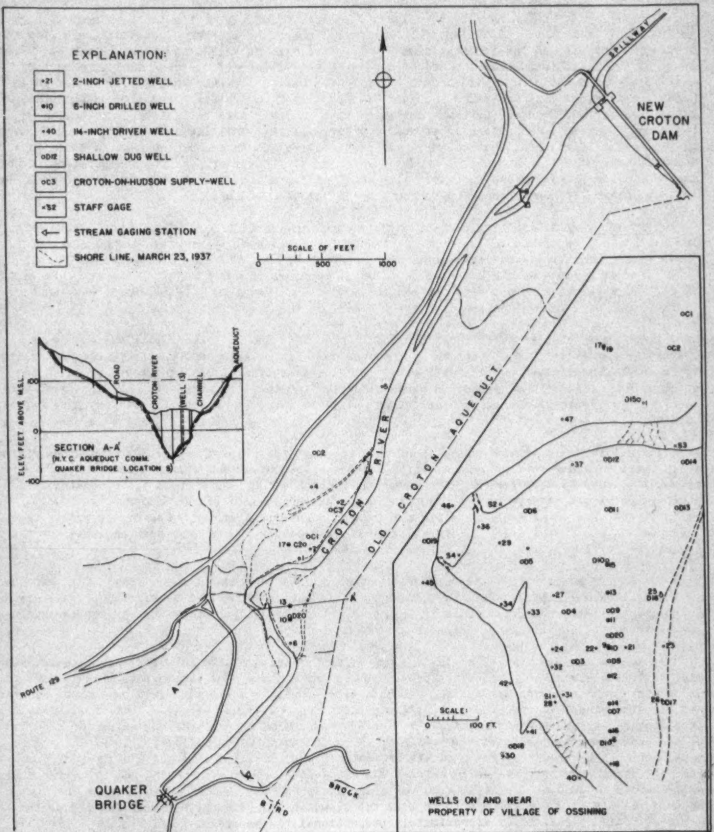
EXPLANATION:
•21 2-INCH JETTED WELL
•10 6-INCH DRILLED WELL
•40 14-INCH DRIVEN WELL
oD12 SHALLOW DUG WELL
oC3 CROTON-ON-HUDSON SUPPLY-WELL
×52 STAFF GAGE
STREAM GAGING STATION
SHORE LINE, MARCH 23, 1937
SCALE OF FEET
0
500
1000
NEW CROTON DAM
SPILLWAY
CROTON RIVER
OLD CROTON AQUEDUCT
ROUTE 129
QUAKER BRIDGE
BIRD BROOK
A
A'
ELEV. FEET ABOVE M.S.L.
100
0
-100
ROAD
CROTON RIVER
WELL 13
CHANNEL
AQUEDUCT
SECTION A-A'
(N.Y.C. AQUEDUCT COMM. QUAKER BRIDGE LOCATION 9)
SCALE:
0
100 FT.
WELLS ON AND NEAR PROPERTY OF VILLAGE OF OSSINING

FIG. 1—MAP OF CROTON VALLEY, NEW YORK, BETWEEN NEW CROTON DAM AND QUAKER BRIDGE, SHOWING LOCATION OF WELLS

unconsolidated deposits of silt, sand, and gravel. Although the valley-fill is quite irregular in composition, it may be divided in general into three zones, an upper zone of relatively permeable water-bearing material, an intermediate zone of relatively impermeable material which is effective as a confining layer, and a lower zone of relatively permeable material. At low flow Croton River consists of a series of shallow ponds and ranges in width from about 30 to 50 feet.

The public water-supply of the Village of Croton-on-Hudson is obtained from three shallow wells near the Croton River about 4,500 feet downstream from the New Croton Dam. These wells, (C1, C2, and C3 in Fig. 1) are of the caisson-type, ranging in depth from eight to 20 feet. They are inter-connected by open-joint tile and pumped by suction. Ordinarily Croton pumps about eight to ten hours a day at a rate ranging from 500 to 1,100 gallons a minute. Over the period of record the minimum pumpage on any one day was 230,000 gallons, and the maximum, 1,049,000 gallons. The average daily pumpage is 471,000 gallons.

The Village of Ossining for some time has desired to obtain an additional supply to augment the existing inadequate supply, and for that reason purchased property just downstream from the Croton property and on the opposite side of the river. In July 1933 an investigation was undertaken by the Geological Survey in cooperation with the Village of Ossining to determine the available ground-water supply in Croton Valley. The results of this work are contained in an unpublished report [see reference 1 at end of paper] by D. G. Thompson and A. W. Harrington, dated December 1935, in which it was concluded that in order more definitely to establish geologic and hydrologic conditions an extensive program of test-drilling and pumping would be necessary. It was recommended that the Slichter electrolytic and Thiem pumping methods be employed to determine the underflow and that corroborative laboratory-tests of porosity and permeability be made on samples of water-bearing material. Accordingly, an additional cooperative agreement was entered into by the Geological Survey and the Village of Ossining.

Test-wells--Test-drilling operations were begun in August 1936. Eight 6-inch wells extending along the axis of the Valley were drilled by the cable-tool, percussion-method. Six of these wells encountered bed-rock, at an average depth of 90 feet. The casings of the 6-inch wells were perforated with slots. In addition, eleven 2-inch wells ranging in depth from 14 to 78 feet were jetted. These wells are located on radial lines from Well 10, which was intended for a pumped well in conjunction with Thiem's test on the deeper material.

A system of shallow wells consisting of 19 dug wells and 15 driven wells was also installed. One of the dug wells, D20, intended as a pumped well for Thiem's test on the shallow material, is 38 inches in diameter and 13 feet deep. With the exception of D2, the other dug wells are eight or nine inches in diameter and range in depth from three to ten feet. All are cased with perforated galvanized-iron pipe. The driven wells are 1 1/4 inches in diameter and have 2-foot screens.

Water-level data--These wells, on or near the Ossining property, together with nine original wells, were included in a program of water-level measurements, thus giving a total of 62 observation-wells. Weekly measurements have been made in each of the wells since their construction. Water-stage recorders have been in continuous operation on wells 10, 17, and D15, and during various intervals on each of the wells six inches or more in diameter. Records of weekly tape-measurements and lowest daily water-levels from the recorder charts have been published by the Geological Survey [2].

The water-levels in all the wells respond to changes in stage of the stream. The response in the shallow wells is direct. In the deeper wells the response is less direct, probably owing in part to the effect of the change in load on the confining layer occasioned by the change in stream-level and accompanying change in ground-water level.

As the storage-facilities of the Village of Croton are adequate only for about a day's supply, their wells must be pumped every day. It was anticipated that pumping tests would have to be conducted while Croton was pumping and that water-level measurements would have to be corrected accordingly. Thus, in order to determine the extent and magnitude of the effect of withdrawal from Croton's wells, water-stage recorders were installed on each of the dug wells for a period of at least one week. With the exception of D15, D18, and D19, these observation-wells are located on the side of the stream opposite Croton's wells, and all are affected by Croton's pumping. By comparing the differences between low water-level and the preceding and following high water-levels in each of the wells with the differences between the corresponding low and high water-levels in Well D15, it was found that the drawdown and recovery produced in each of the wells by Croton's pumping is approximately proportional to the drawdown and recovery in Well D15. Well D15 was thus chosen as a reference-well because of its proximity to the Croton supply-wells. A daily fluctuation of about 1.5 feet is observed in D15 when Croton pumps 500,000 gallons during a period of eight hours each day.

Values of the ratio of the drawdown or recovery in the various wells to that in D15, termed here the "response-ratio", are given in Table 5. The periods of observation chosen were those in which little or no rainfall occurred and during which the stage of the stream remained constant or nearly so.

Water-levels in the deep wells were also found to respond to Croton's pumping, the response being due at least in part to the effect of change in load on the confining layer produced by dewatering the material above. A very slight barometric effect was observed in each of the 6-inch wells with the exception of 15 and 17.

Stream-flow data--In conjunction with this investigation three stream-gaging stations were established July 28, 1933, and have been maintained under the direction of A. W. Harrington of the Geological Survey. Two of these stations are on Croton River, one just below New Croton Dam and one at Quaker Bridge. The third station is on Bird Brook, a tributary which enters Croton River just above Quaker Bridge. Stream-flow data from these stations have been published by the Geological Survey [3].

When the dam is not overflowing, the flow of Croton River at the upper station is usually less than two second-feet. The overflow from the dam is usually several hundred second-feet. The maximum mean daily discharge observed since installation of the gaging-stations and prior to September 30, 1936, was 4,350 second-feet (on March 19, 1936). The minimum mean daily discharge at the upper station was 0.84 second-feet (on October 15 and 16, 1935).

Laboratory-determination of permeability--During drilling operations unwashed samples of of water-bearing material were taken from the sand-pump at every few feet and at each change in formation. Of the 151 samples taken, 115 were retained for determinations of permeability, which were made in a portable permeameter of the discharging type. This apparatus consists of a percolation-cylinder and a manometer connected in the form of a U-tube. The sample of material to be tested is placed in the percolation-cylinder, being supported by a screen in the bottom of the cylinder. Water is introduced into the graduated glass tube which extends above the top of the percolation-cylinder and thus serves as a source of supply as well as provides a means of measuring the discharge. As the water flows upward through the material and discharges at a constant head over the edge of the cylinder, the head in the manometer declines, the rate of decline diminishing with time. By measuring the time elapsed between interceptions of the meniscus by any two graduations on the tube the permeability may be determined.

The values of coefficient of permeability (defined as the flow in gallons a day through a square foot of cross-section under unity gradient) thus determined range from 3.9 to 36,000 and average 2,050. The average of the coefficients of permeability of the samples taken from the seven 6-inch wells excluding Well 17 is 1,290.

The test-section of the New York City Aqueduct Commission at "Quaker Bridge Location S" is shown as section A-A' in Figure 1. This test-drilling was done in conjunction with exploratory work prior to construction of New Croton Dam. No logs of the borings are available. From published sections [4] only the depth and general character of the bed-rock can be ascertained. As nearly as could be determined from available maps, this section was located as shown in Figure 1, crossing the Ossining property near the present site of Well 13.

In order to facilitate computations of the effective permeability, the cross-section of the Valley below ground-level was replaced by an equivalent triagular section having a width of 435 feet at elevation 40.0, and a depth of 102.5 feet. It was arbitrarily assumed that material having a permeability less than 100 is effective as a confining layer. On the basis of this assumption the confining layer is absent in wells 12 and 14, although a negative artesian head is observed in these wells as in the other deep wells.

The effective cross-section area of the lower aquifer was assumed to be the area represented by material which is below the confining layer and which has a permeability greater than 100. The permeability of such material was weighted according to the area of cross-section, it being assumed that each sample is representative of a layer of the thickness indicated by the log, running the full width of the section at that depth. Table 1 gives the effective area of cross-section and weighted average permeability based on the logs of the five wells in which the arbitrarily defined confining layer occurs. The mean of the weighted average permeabilities is 2,290 and the average of the effective areas of the five sections is 8,650 square feet.

Table 1--Effective cross-section area and weighted average permeability of lower aquifer

Well	Effective area, square feet	Permeability, weighted average
10	7,009	948
11	12,880	196
13	9,505	6,080
15	5,890	1,680
16	7,967	2,280
Averages	8,650	2,290

From contours plotted on the piezometric surface the natural hydraulic gradient in the artesian aquifer was found to be approximately 35 feet to the mile. Thus, the underflow through the lower aquifer is

$$Q = PIA = 2,290 \times 35/5,280 \times 8,650$$
$$= 132,000 \text{ gallons a day}$$

where P is the coefficient of permeability, I is the hydraulic gradient, and A is the cross-sectional area in square feet.

The saturated thickness of the upper aquifer was determined from the logs of the 6-inch wells and from water-level measurements made in the shallow wells on the same line. The lower limit of the aquifer was taken as the top of the confining layer arbitrarily defined above. The water-level measurements were made September 8, 1937, at a time when the stage of Croton River according to staff gage S 1 near-by was but 0.05 foot above the minimum stage. The thickness of the aquifer thus defined varies from well to well and averages 15.7 feet, as shown in Table 2. On the basis of the equivalent triangular section the cross-section area of the saturated aquifer is found to be 5,870 square feet and the average width of the section 374 feet.

Table 2--Saturated thickness of upper aquifer

Well No.	Elevation in feet above mean sea-level			
	Ground level	Water-level (in upper aquifer)	Top of confining layer	Thickness of saturated aquifer in feet
10	42.0	33.47	18.0	15.5
11	41.6	33.78	29.6	4.2
13	41.2	33.97	29.2	4.8
15	40.8	34.16	3.8	30.4
16	40.0	32.39	9.0	23.4
Averages		33.5		15.7

Table 3 gives the permeability and porosity of samples of water-bearing material from the shallow wells. Permeabilities were determined with the portable apparatus described above; porosities were determined by V. C. Fishel in the Hydrologic Laboratory of the Geological Survey.

Table 3--Samples of water-bearing material from shallow wells

Sample	Depth in feet	Permeability	Porosity in per cent
D4-C	3.5	4,800	34.2
D5-A	-	5,500	----
D5-B	4	6,900	27.1
D8-A	9	950	24.3
D8-C	10	-----	27.8
D8-D	11	-----	29.3
D9-A	6	195	41.4
D9-B	5	2,880	----
D9-C	9	4,200	23.4
D10-A	9	2,350	30.5
D20-D	10	3,970	25.4
D20-H	3	1,800	33.8
Averages		3,350	29.7

From contours plotted on the water-table the natural hydraulic gradient was found to be approximately 25 feet to the mile. Thus the underflow through the upper aquifer is

$$Q = PIA = 3,350 \times 25/5280 \times 5,870$$
$$= 93,000 \text{ gallons a day}$$

Slichter method--Slichter [5] devised a method of measuring ground-water velocity by observing the rate of movement of an electrolyte from one well to another. The electrolyte (usually ammonium chloride) is introduced into one well and its movement toward a down-gradient well traced by means of electric circuits. In order to determine the direction of the velocity ordinarily three or more receiving wells are employed. Two circuits are used for each receiving well, namely, one between the casings of the receiving and salt wells--known as the "casing circuit"--and the other between the casing of the receiving well and an insulated electrode inside the receiving well--known as the "electrode-circuit." By means of a battery and an ammeter the conductivity of these circuits may be measured.

A series of Slichter tests was run using Well D3 as salt well with four receiving wells located as shown in Figure 2. Well D3 is eight inches in diameter and 9.9 feet deep. The receiving wells are 1 1/4-inch driven wells 12.5 feet deep and have 2-foot screens. Well D3 was salted by pouring ten quarts of saturated ammonium-chloride solution through a length of garden hose, the lower end of which was submerged in the bottom of the well. This procedure was found necessary in order to insure the movement of the salt through the bottom of the well into the formation. The disturbance in head thus produced was evaluated by comparing velocities between wells (a) and (d) with velocities between D3 and (a) and between D3 and (d).

Typical curves showing the time variation of the current in the circuits of wells (a) and (d) are given in Figure 2. Of the two curves the electrode-curve is the more significant. The initial decline in the electrode-current is attributed to polarization. As the salt reaches the receiving well, the conductivity of the electrode-circuit increases. The point of inflection of the curve is the point at which the time-rate of increase in conductivity is greatest, and is therefore considered to represent the average time of arrival of the salt. Table 4 gives the results of tests I to VI on Well D3. Test I was run before well (d) was driven, giving but one combination of wells from which to determine the ground-water velocity. For some unknown reason no results were obtained in test IV. In each of the other four tests ground-water velocities were determined for three combinations of wells.

In computing the value of permeability from measurements of ground-water velocity, the gradient and the effective porosity must be known. The gradient was determined by plotting contours for water-level measurements made in nearby shallow wells. From Table 3 the average of the porosity of ten samples taken from the shallow water-bearing material is 29.7 per cent. Thus, assuming the effective porosity to be equal to the actual porosity, the permeability is

$$P = Q/IA = (Q/pA)(p/I) = 7.48\ v(p/I) = 2.22\ v/I$$

where p is the effective porosity and v is the average ground-water velocity.

The average value of permeability determined by the Slichter method is 3,550. Using the values of natural hydraulic gradient and area of cross-section determined above, the underflow through the upper aquifer according to the Slichter method is found to be

$$Q = PIA = 3{,}550 \times 25/5280 \times 5{,}870 = 99{,}000 \text{ gallons a day}$$

In spite of the fact that an attempt was made to employ the identical technique in salting D3 each time, discrepancies arose in values of velocity from the four tests. A comparison of the data shows that the discrepancy is greatest for the combination of wells D3 and (a), and is least for the combination of wells (a) and (d). This indicates that the disturbance in head,

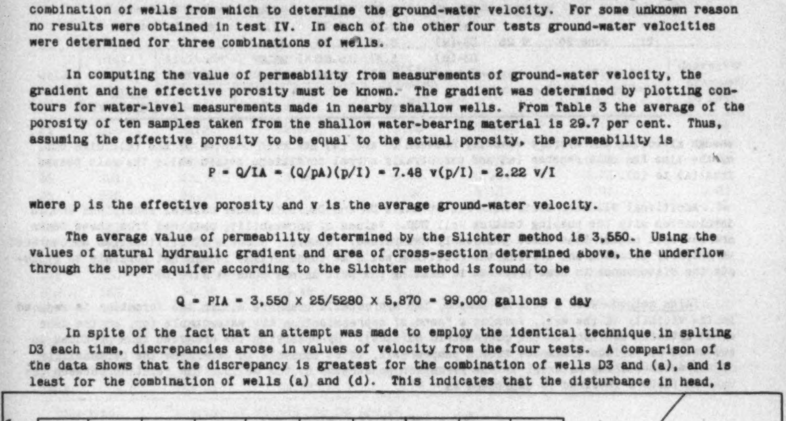

FIG. 2—DETERMINATION OF GROUND-WATER VELOCITY BY SLICHTER METHOD, WELL D3, TEST II, JUNE 17, 1937

Table 4--Results of Slichter tests on Well D3

Test	Date	Test begun	Wells	Dis- tance	Time	Veloc- ity	Gradi- ent	Permea- bility
	1937	h m		feet	hour	ft/day	ft/mile	
I	June 16	14 45	D3-(a)	3.42	2.42	33.9	64.3	....
II	June 17	10 25	D3-(a)	3.42	4.83	17.0		
			D3-(d)	5.87	8.09	17.4		
			(a)-(d)	2.45	3.26	18.0	68.1	3,100
III	June 21	9 10	D3-(a)	3.42	3.58	22.9		
			D3-(d)	5.87	6.59	21.4		
			(a)-(d)	2.45	3.01	19.5	65.6	3,500
IV	June 22	7 47					41.3	
V	June 29	9 55	D3-(a)	3.42	2.33	35.2		
			D3-(d)	5.87	5.08	27.8		
			(a)-(d)	2.45	2.75	21.4	63.3	4,000
VI	June 30	9 25	D3-(a)	3.42	2.50	32.8		
			D3-(d)	5.87	5.50	25.6		
			(a)-(d)	2.45	3.00	19.6	63.3	3,600
				Average				3,550

though affecting the average velocity between D3 and (a) and even between D3 and (d), dies out by the time the salt reaches (a) and essentially normal conditions obtain while the salt passes from (a) to (d).

Additional Slichter tests were run at wells D8 and D9, both under natural conditions and in combination with the pumping test on Well D20. Values of permeability obtained from these tests are somewhat higher than those previously determined. However, because of difficulties encountered in properly locating and driving the receiving wells at these sites, it was not possible to evaluate the disturbance in head produced in salting the well as was done at Site D3.

Thiem method--When a well is pumped, the hydrostatic pressure within the formation is reduced in the vicinity of the well, forming a "cone of depression" on the water-table (or, in the case of an artesian aquifer, on the piezometric surface). By observing the drawdown thus produced in two wells at various distances from a pumped well, it is possible to determine the permeability of the water-bearing material in the formation penetrated by the wells. The relation between the various factors involved is expressed as

$$P = 528\ q\ \log_{10}(r_2/r_1)/m(s_1 - s_2)$$

where P is the coefficient of permeability, q is the pumping rate in gallons a minute, m is the thickness of the aquifer in feet, s_1 and s_2 are the drawdowns at two observation wells r_1 and r_2 feet, respectively, from the pumped well also expressed in feet. The above equation, known as Thiem's equation, has been derived and the method of its application discussed in detail elsewhere [6].

A Thiem pumping test of 24 hours' duration was run on the shallow water-bearing material beginning 5:30 am, September 8, 1937. Well D20 was pumped at a rate of 106 gallons a minute with a 4-inch centrifugal pump. The pump discharged into the open end of a 6-inch pipe line through which the water flowed by gravity to a calibrated tank by means of which the yield of the pumped well was determined. The discharge was emptied into the stream below the pond opposite the Ossining property in order to preclude its affecting water-levels during the test.

Water-levels in 34 shallow wells were measured at frequent intervals during the pumping period and during the recovery period following. In order to determine the effect of pumping from the shallow formation on water-levels in the deep wells, tape-measurements were made also in the deep wells. A response of as much as 0.2 foot was observed in the deep wells in the vicinity of Well D20, probably being due in part to change in load on the confining layer. Table 5 gives the drawdown in each of the shallow observation-wells as determined from differences in water-levels before and after pumping Well D20 for 24 hours. It is assumed that the net drawdown of 0.09 foot observed in Well D15 was due to variations from day to day in Croton's pumping and that a corresponding decline in water-level was produced by the same cause in each of the shallow wells, the amount of decline in any well being proportional to the "response-ratio" of that well. Values of observed drawdown were corrected accordingly.

In order to determine the amount of underflow by the Thiem method it is not necessary to know the thickness of the aquifer. The value of the product of the permeability P by the thickness of the aquifer m may be determined from Thiem's equation as

$$Pm = 528\ q\ \log_{10}(r_2/r_1)\ /\ (s_1 - s_2)$$

This quantity, termed by Theis [7] the "coefficient of transmissibility" of the aquifer, multiplied by the width of the aquifer and by the gradient gives the total underflow. That is

$$Q = (Pm)\ w\ I$$

The width of the aquifer, w, may be determined with a reasonable degree of accuracy. Thus, uncertainties involved in determining the average thickness of the aquifer may be avoided and the reliability of the method improved.

Table 5--Drawdown in shallow observation-wells during pumping test on Well D20

Well No.	Dist. from D20 in feet	Altitude of water-level in feet above mean sea-level		Observed drawdown in feet	Response-ratio	Correction for Croton's effect	Corrected drawdown in feet
		Before pumping	After pumping 24 hours				
D1	212	32.23	32.15	0.08	0.028	----	0.08
D3	84.7	32.91	32.65	0.26	0.056	0.01	0.25
D4	99	33.66	32.96	0.70	0.085	0.01	0.69
D5	224	33.71	33.55	0.16	0.104	0.01	0.15
D6	296	34.36	34.31	0.05	0.107	0.01	0.04
D7	151	32.70	32.32	0.38	0.029	----	0.38
D8	51.1	33.49	32.30	1.19	0.053	----	1.19
D9	51.7	33.88	33.08	0.80	0.086	0.01	0.79
D10	150	34.20	33.70	0.50	0.221	0.02	0.48
D11	250	34.55	34.31	0.24	0.169	0.02	0.22
D12	350	34.52	34.48	0.04	0.098	0.01	0.03
D13	287	35.04	34.80	0.24	0.242	0.02	0.22
D14	376	35.39	35.22	0.17	0.273	0.02	0.15
D15	475	35.70	35.61	0.09	1.000[a]	0.09	0
D16	134	34.25	33.69	0.56	0.094	0.01	0.55
D17	172	33.68	33.37	0.31	0.014	----	0.31
D20a	1.5	33.61	29.83	3.78	0.070[b]	0.01	3.77

[a] Assumed. [b] Average of values for D8 and D9.

Values of the coefficient of transmissibility were computed for all possible combinations of wells. The average of all positive values using the observed drawdown is 93,000, and using the corrected drawdown, 81,300. Experience has shown, however, that combinations of wells on radial lines directly up- and down-gradient from the pumped well give the most reliable results. Table 6 gives values computed for such combinations for both observed and corrected drawdowns.

Table 6--Coefficient of transmissibility of upper aquifer

Well No.	Using observed drawdown	Using corrected drawdown
D9-D10	86,300	83,500
D9-D11	68,400	67,200
D9-D12	61,200	61,200
D10-D11	47,800	47,800
D10-D12	44,800	45,800
D11-D12	40,900	43,000
D8-D7	32,500	32,500
D8-D1	31,200	31,200
D7-D1	27,200	27,200
Average	48,900	48,800

The average width of the aquifer was found to be 374 feet. Thus, using the value of gradient used above, the total underflow through the upper aquifer according to the Thiem method is

$$Q = 48{,}800 \times 374 \times 35/5280 = 86{,}000 \text{ gallons a day}$$

In addition to the Thiem test run on the shallow material, three such tests were later run on the deeper material. Wells 10, 12, and 14 were pumped in turn as shown in Table 7.

These wells were pumped by means of a 4-inch turbine pump driven by a gasoline engine. As before, the discharge was measured volumetrically. Water-level measurements were made in seven 6-inch wells and in eleven 2-inch wells. Occasional measurements of water-levels in the shallow wells failed to show the effect of pumping from the deep material. Values of drawdown as determined from differences in water-levels before and after pumping Well 10 for 24 hours are given in Table 8.

Table 7--Thiem tests on wells 10, 12, and 14

Well No.	Began pumping	Length of pumping period	Discharge in gallons per minute
	h m	h m	
10	Sept. 21, 1937, 5 30	24 00	76
12	Sept. 23, 1937, 5 30	12 30	56
14	Sept. 24, 1937, 16 00	14 07	105

Table 8--Drawdown in observation wells during pumping test on well 10

Well No.	Distance from Well 10 in feet	Altitude of water-level feet above mean sea-level		Observed drawdown in feet
		Before pumping	After pumping	
6	184	33.77	31.01	2.76
9	0.6	34.85	27.34	7.51
11	55.6	34.87	31.40	3.47
12	54.8	33.88	30.79	3.09
13	110	35.17	30.93	4.24
14	110	33.84	30.91	2.93
15	165	35.17	35.14	0.03
16	165	33.78	30.89	2.89
21	35.4	34.90	28.82	6.08
22	34.9	33.92	28.70	5.22
23	110	35.87	34.71	1.16
24	109	34.36	33.77	0.59
25	151	36.04	35.76	0.28
26	150	35.67	34.19	1.48
27	150	35.00	34.52	0.48
28	150	33.99	33.22	0.77
29	300	36.03	35.02	1.01
30	300	33.61	33.21	0.40

Table 9--Coefficient of transmissibility of lower aquifer

Pumped well	Observation-wells	Coefficient of transmissibility
10	12 - 14	76,000
	12 - 16	96,000
	12 - 6	64,000
	14 - 16	176,000
	14 - 6	53,000
	16 - 6	15,000
12	14 - 16	68,000
	14 - 6	42,000
	16 - 6	16,000
	10 - 11	10,000
	10 - 13	30,000
	11 - 13	----
14	16 - 6	60,000
	12 - 10	3,000
	12 - 11	5,000
	12 - 13	6,000
	10 - 11	24,000
	10 - 13	22,000
	11 - 13	20,000
	Average	44,000

Applying Thiem's equation to these data, values of the coefficient of transmissibility were computed. No attempt was made to correct for the effect of Croton's pumping as this effect was small compared with that due to change in stage of the stream. (The dam was overflowing at the time.) The effect of the latter, however, was found to be essentially the same in each of the deep wells and since Thiem's equation involves differences between drawdowns in two wells, no correction was needed. The mean values of coefficients of transmissibility for all possible combinations of wells in the three tests are 16,400, 22,900, and 14,100 for wells 10, 12, and 14, respectively. More reasonable values are those obtained by using only those combinations of wells up- and down-gradient from the pumped well, as given in Table 9.

The average width of the artesian aquifer is 135 feet. Thus, using the proper value of hydraulic gradient, the total underflow through the lower aquifer based on the results of the three Thiem tests is

$$Q = 44,000 \times 135 \times 35/5280$$
$$= 39,000 \text{ gallons a day}$$

Theis method--It has been shown [7, p. 522] that the coefficient of transmissibility of an aquifer may be determined from observations of the rate of recovery of the water level in a pumped well. The equation in notation consistent with that used here is

$$Pm = 264\ q\ \log_{10}(t/t')/s'$$

where q is the yield of the well in gallons a minute before shut-down, s' is the residual drawdown, or the difference between the static level and the recovering water-level, in feet, t is the time since pumping started, and t' is the time since pumping stopped.

In Figure 3 are plotted the water-level measurements made in Well D20a, a 1 1/4-inch driven well just outside the casing of Well D20, after the pump of D20 was shut down. The recovery curve drawn through these points is to be corrected for the effect of withdrawal from Croton's wells. From a study of recorder-charts taken from

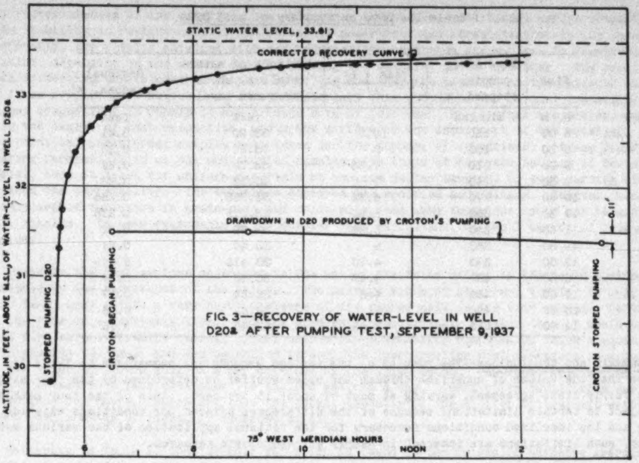

FIG. 3—RECOVERY OF WATER-LEVEL IN WELL D20a AFTER PUMPING TEST, SEPTEMBER 9,1937

wells D8 and D9 near D20 it was found that the drawdown in those wells produced by Croton's pumping may be approximated by two straight lines--one a horizontal line representing a "lag" of 2 1/2 hours, the other representing drawdown at a constant rate. The pump on Well D20 was shut down at 5:30 am, September 9, 1937. Croton began pumping that day at 6:30 am, and quit pumping at 3:30 pm, producing a drawdown of 1.56 feet in Well D15. The response ratio for well D20a was found to be 0.070 (Table 5). Thus the drawdown in this Well due to Croton's pumping was 1.56 x 0.070 = 0.11 foot, beginning at 9:00 am and ending at 3:30 pm, as drawn in Figure 3.

Values of t/t' and s' determined for points taken at half-hour intervals on the corrected recovery curve are given in Table 10. During the first part of the recovery-period the assumptions on which this method is based are not satisfied, therefore only the last part of the recovery-curve is used. The pumping period was 24 hours, or 1,440 minutes; therefore the time in minutes since pumping started is (1,440 + t'), where t' is the time in minutes since pumping stopped. The elevation of the true static water-level in D20a was assumed to be 33.61. Thus the residual drawdown is s' = (33.61 - s), where s is the elevation of the recovering water-level at time t'.

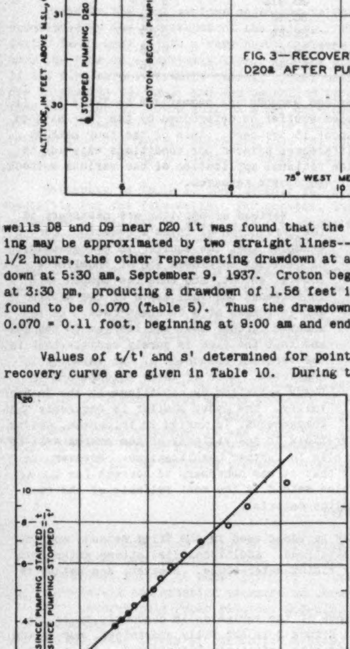

FIG. 4—APPLICATION OF THEIS METHOD TO RECOVERY IN WELL D20a

The data given in Table 10 are plotted on semi-logarithmic coordinates in Figure 4. The coefficient of transmissibility is determined from the slope of the straight line drawn through the points as plotted. Well D20 was pumped at a rate of 106 gallons a minute. Thus

$$Pm = 264 \times 106 \times 1/0.505 = 55{,}000$$

and the total underflow through the upper aquifer according to the Theis method is

$$Q = (Pm)\ w\ I = 55{,}000 \times 374 \times 25/5280$$
$$= 97{,}000 \text{ gallons a day}$$

Table 10--Data on recovery of well D20a

Time	Time since pumping stopped, t'	$\left(\frac{1440 + t'}{t'}\right)$	Altitude of water-level, s, above mean sea-level	Residual drawdown, s'
h m	minutes		feet	feet
8 00	150	10.6	33.20	0.41
8 30	180	9	33.26	0.35
9 00	210	7.86	33.29	0.32
9 30	240	7	33.33	0.28
10 00	270	6.34	33.355	0.255
10 30	300	5.80	33.375	0.235
11 00	330	5.36	33.39	0.22
11 30	360	5	33.40	0.21
12 00	390	4.70	33.415	0.195
12 30	420	4.43	33.43	0.18
13 00	450	4.2	33.44	0.17
13 30	480	4	33.45	0.16
14 00	510	3.82	33.46	0.15

Summary and conclusions--The results of the various methods are summarized in Table 11. It is seen that the values of underflow through the upper aquifer as determined by the four methods are in fairly close agreement, varying at most by about 15 per cent. Each of the four methods is subject to certain limitations because of the differences between the conditions existing in nature and the idealized conditions necessary for the rational application of the various methods. However, such limitations are inherent in nearly all hydrologic research.

Table 11--Ground-water underflow in Croton Valley measured on property of Village of Ossining

Aquifer	Method	Underflow in gallons a day	
Upper	Laboratory	93,000	
	Slichter	99,000	
	Thiem	86,000	86,000
	Theis	97,000	
Lower	Laboratory	132,000	132,000
	(Thiem	39,000)	
	Total		218,000

Various assumptions are necessary to the derivation of Thiem's equation, for example, that the aquifer is homogeneous, of constant thickness and of indefinite lateral extent, and that the pumped well is screened the full depth of the aquifer and is pumped at a constant rate. It is assumed further that equilibrium is reached and that the flow is purely radial--that is, the vertical component of the velocity is neglected. None of these assumptions is fully satisfied by conditions in the Croton Valley. The upper aquifer is decidedly non-homogeneous, is varied in thickness, and is confined laterally by bed-rock walls. Also the water-table in the vicinity of the pumped well is bounded on nearly three sides by the stream, giving rise to further complications. However, in spite of these limitations, and in spite of the fact that it was necessary to correct for the effect of Croton's pumping, it is believed that the Thiem method is the most reliable of the four methods in determining the underflow through the shallow material.

The Theis method is based on assumptions similar to those used in the Thiem method, and is therefore subject in varying degrees to the same limitations. Additional limitations arise from the disturbing effect of the capillary fringe on the rising water-table. However, the method is justified by its simplicity and convenience.

In regard to the Slichter method, the significance of the variation in conductivity of the electric circuits illustrated by the curves drawn in Figure 2 is not fully understood, nor is the relation between the effective porosity and actual porosity definitely established. It is likely, however, that in the case of the material involved here the difference between the actual and effective porosities is not large. It is believed that the uncertainties arising from disturbance in natural conditions have been properly eliminated and the method thus improved. However, as conclusive results were obtained from the Slichter method at only one site and as the composition of the water-bearing material is known to vary laterally as well as vertically, these results can serve merely as a check.

The determination of underflow through the shallow material by the laboratory-method is based on the permeabilities of ten samples taken at or near the water-table in six of the shallow dug wells. To begin with, uncertainties arise in this method because the samples are not always truly representative of the material from which they were taken. Additional errors arise because of the inability to restore natural conditions of assortment and stratification in the laboratory. Consequently, the results obtained with any permeability-apparatus are subject to considerable variation, depending on the method used in placing the material in the cylinder. The usual procedure in testing non-volumetric samples is to jar and tamp the material in the cylinder so as to make it as compact as possible. In certain materials this procedure may produce essentially the original compaction; in others it may produce more or less than the original compaction, depending on the degree of that compaction, and on the uniformity and assortment of the material, etc. Where possible, undisturbed samples were taken for the purpose of comparison. It was found, by comparing permeabilities of the undisturbed samples with those of repacked samples of the same material, that at least for uniform materials at shallow depths tamping, or even jarring alone, decreased the permeability. The technique employed was modified accordingly. However, because of considerable variation in grain-size and uniformity, the same technique could not be applied to all samples. To make reasonable allowance for such variations, no small amount of judgment is needed.

Probably the most serious objection to the use of the Thiem method in the lower aquifer is occasioned by the narrowness of the Valley. The average width of the lower aquifer is only 135 feet. Thus, only within a very short distance of the pumped well is the flow radial. Farther away the flow is essentially linear and the gradient is accordingly greater than it would be if the flow were everywhere radial. Thus, values of permeability computed by Thiem's equation are in general too small and the underflow through the lower aquifer must therefore be greater than the value of 39,000 gallons a day obtained by the Thiem method. In fact, Well 14 was actually pumped at a rate equivalent to 150,000 gallons a day. Comparative equilibrium was reached in a few hours, indicating that the normal capacity of the aquifer to transmit water had not been greatly exceeded, if exceeded at all.

Referring to Table 1, considerable variation is found in the values of weighted average permeability for the five wells. For example, the weighted average permeability of the section represented by the log of Well 11 is 196, compared with an average of 2,290. It might be argued that the underflow cannot be greater than that through the section of minimum permeability. However, it is likely that if those wells had been located on a line crossing the Valley, as great or greater variation in permeability would have been encountered. The assumption made in determining the effective cross-sectional area and the weighted average permeability was merely one of convenience. Regardless of the arbitrary character of such an assumption, the value thus obtained by averaging a large number of permeability-determinations should approach within reason the true value of effective permeability.

Using the values of underflow through the upper aquifer as determined by the Thiem method and through the lower aquifer by the laboratory-method, it is concluded that the total ground-water underflow in Croton Valley at the property of the Village of Ossining is about 200,000 gallons a day. By comparing Croton's pumpage with the difference in the flow of Croton River at the upper and lower gaging-stations, it was found that virtually 100 per cent of Croton's supply is drawn from the River, either directly or indirectly. Thus, it is concluded that the flow of 200,000 gallons a day at the Ossining property is exclusive of the amount pumped by Croton farther upstream.

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DETERMINATION OF INFILTRATION-RATES FROM SURFACE-RUNOFF

L. K. Sherman

Infiltration-rates in this paper refer to full-soil capacity-rates. They occur only during rain-intensities which produce surface-runoff.

Infiltration-rates cannot be determined by subtracting runoff from rainfall. When the storage-equation is applied, at any time during the progress of effective rain, we have two unknown quantities. One is a part of outflow, represented by infiltration, the other is storage, represented by surface-detention or the volume of water upon the ground-surface at a given time.

The whole problem is, how to determine this volume of water called "surface-detention". A simple illustration will show the solution.

The Soil Conservation Service has furnished me the records of two sprinkler-experiment runs. They were made at Lincoln, Nebraska, Plot No. 2, on October 21 and November 5, 1937. All of the data is shown in Table 1. The columns "Mass-runoff" were computed by the author.

Table 1--Preliminary tests, series A, Plot 2, Lincoln, Nebraska

Particulars regarding runs		
Run No.	10	22
Date 1937	Oct. 21	Nov. 5
Duration in minutes	60	60
Amount applied in inches.	1.21	2.50
Average intensity in inches/hour. .	1.21	2.50
Initial soil-moisture in per cent		
0-6 inches.	35	36
6-12 inches.	27	29
12-24 inches.	21	28

Run No. 10			Run No. 22			Run No. 22		
After start	Run-off	Mass-runoff	After start	Run-off	Mass-runoff	After start	Run-off	Mass-runoff
min	in/hr	inch	min	in/hr	inch	min	in/hr	inch
0	----	----	0	----	----	38	0.86	.401
17.5	a	.0000	5	a	.0000	40	0.89	.431
19.5	0.06	.0010	7	0.06	.0010	42	1.02	.462
20.5	0.12	.0025	9	0.30	.0065	44	0.98	.496
21.5	0.14	.0047	10	0.44	.0127	46	1.05	.529
22.5	0.10	.0067	12	0.64	.0304	48	1.10	.566
25	0.24	.0104	14	0.64	.0520	50	1.03	.601
27	0.15	.0167	16	0.69	.0742	52	1.07	.635
31	0.15	.0265	18	0.69	.0973	54	1.08	.672
33	0.195	.0325	20	0.72	.1220	56	0.99	.706
40	0.18	.0544	22	0.82	.1478	58	0.91	.755
45	0.16	.0685	24	0.97	.1770	60	0.91[b]	.768
50	0.17	.0824	26	1.09	.2121	60.5	0.78	.792
60	0.18[b]	.1114	28	1.03	.2472	61	0.63	.780
62	0.08	.1156	30	0.85	.2781	62	0.34	.788
62.5	0.17	.1170	32	0.98	.3088	62.5	0.25	.792
64	0.10	.1200	34	0.96	.3420	63	0.22	.794
67	0.03	.1230	36	0.89	.372	65	0.05	.797
		.1234						.800

[a] Runoff started. [b] Application stopped.

Note--Antecedent rain in inches: Oct. 15, 1.09; Oct. 16, 0.21; Oct. 17, 0.17; Oct. 18, 0.01.

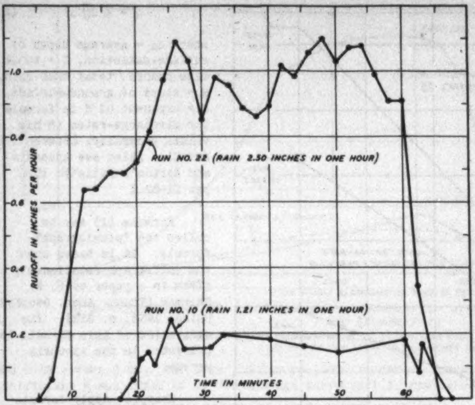

FIG. 1—HYDROGRAPHS FROM SPRINKLER-EXPERIMENTS, PLOT NO. 2, LINCOLN, NEBRASKA, OCTOBER 21 AND NOVEMBER 5, 1937

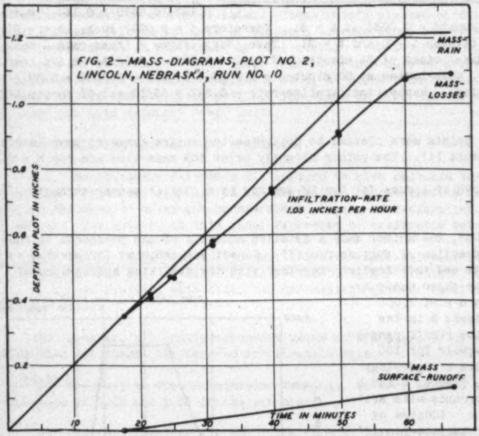

FIG. 2—MASS-DIAGRAMS, PLOT NO. 2, LINCOLN, NEBRASKA, RUN NO. 10

Plot No. 2 is described as follows: "Area 6.6 feet wide by 33 feet long, or 1/200 of an acre. Slope 4.6 per cent; the soil, a sandy silt-loam, deep and well drained; Carrington series of glacial origin; vegetation somewhat sparse grass and weeds, clipped to about one-half inch height."

Figure 1 shows the hydrographs of surface-runoff for the two runs, as plotted from the original data. Run No. 10 had a rain-rate of 1.21 inches per hour for 60 minutes. Run No. 22 had a rain-rate of 2.50 inches per hour, also for 60 minutes. Note that the heavy rain became effective five minutes after the start, and that the lighter rain became effective 17.5 minutes after the start. These are the periods of initial loss. Incidentally note that the times of termination of both hydrographs are practically identical--65 and 67 minutes. Note the abrupt termination of these hydrographs of surface-runoff.

Figure 2 is a mass-diagram of Run No. 10. The upper graph shows the mass-rainfall at a uniform rate of 1.21 inches per hour. The bottom graph shows the accumulated volume of runoff, as observed, from Table 1. The middle line is the curve of mass-losses. Mass-losses are coincident with the line of rainfall during the initial-loss period. Initial losses, prior to runoff, are made up of water used to wet the surface of the ground and vegetation, and water used to fill surface-pockets or irregularities. Some infiltration also takes place.

When these initial losses are satisfied, then the line of losses begins to depart from the line of rainfall. There are two fixed points on the loss line. One, at the beginning of runoff, at 0.35 and 17.5 minutes. The other point is at the end of the surface-runoff period, or 1.087 at time 67 minutes. All of the intervening points on the line between 17.5 and 67 minutes, are affected by runoff in storage or surface-detention.

We will now determine the rate of infiltration, say, for the 19 minutes between time 31 and 50 minutes. First find the value of surface-detention d at time 31. For this purpose, either of two following formulas are available.

$$d_a = [\text{Mass-rain, } \underline{\text{during runoff}}\text{, to } t - \text{runoff to } t(1/C)]C^{1/M} \qquad (1)$$

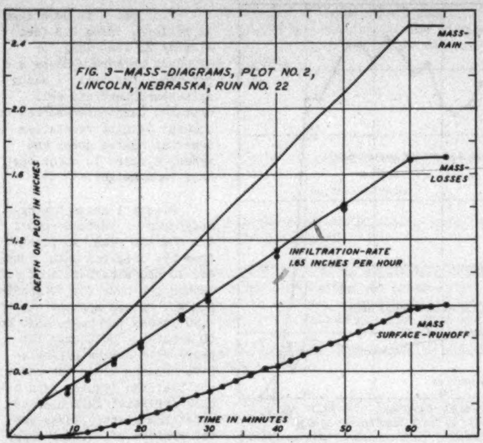
FIG. 3—MASS-DIAGRAMS, PLOT NO. 2, LINCOLN, NEBRASKA, RUN NO. 22

$$q = K\, d_c^M\, S^m \qquad (2)$$

where d_a = average depth of surface-detention, C = total mass-runoff / total mass-rain, S = slope of ground-surface, M = exponent of d in formula for discharge-rates (M has values, generally, between 1.5 and 2). [Also see Appendix and Horton's Bulletin 101, pp. 21-32.]

Formula (1) may be called the "pluviograph"-formula. It is based upon the unit-graph relationship given in a paper by L. K. Sherman [Trans. Amer. Geophys. Union, 1936, p. 312]. The derivation of this formula is presented in the Appendix hereto.

Example--Apply formula (1) to the data of run 10 for t = 31 minutes. Then C = 0.1234/0.8570 = 0.144. Mass-rain = 0.2722 at t = 31. Mass-runoff = 0.0265 at t = 31. Therefore d = 0.0242 inch, at t = 31. Now apply the storage equation between t = 0 and t = 31. Then: Mass-losses = (Mass-rain – mass-runoff – surface-detention). Mass-losses at 31 minutes = 0.6251 - 0.0265 - 0.0242 = 0.574 inch. By the same procedure at t = 50, mass-losses at 50 minutes = 1.0083 - 0.0824 - 0.0232 = 0.901 inch. The difference is 0.327 inch. Hence, infiltration-rate = 0.327 × 60/19 = 1.03 inches per hour.

In a similar manner, other points were plotted to determine the entire curve of mass-losses. We used M = 1.5 in applying formula (1). The points slightly below the mass-line are for M = 2.

Figure 3 shows the mass-curve of losses for Run 22 derived in a similar manner through formula (1).

Check on formula (1)--In 1933, the author made a detailed analysis of the phenomena of run-off in a paper entitled "The hydraulics of surface-runoff". Runoff was computed for several variations of given rates of rainfall and infiltration, together with the resulting hydrographs of runoff. Figure 4 taken from this paper shows hydrographs of surface-runoff from a plot 1,000 feet long and one foot wide. Figure 5 is the same situation, excepting that the runoff passes through a detention storage-reservoir for 100 feet at the outlet. No hypotheses of hydrology or of the unit-graph were used. Figures 4 and 5 are simply facts derived in accordance with well-known standard hydraulic practice. [Copies of this paper on "The hydraulics of surface-runoff" are filed with the libraries of the American Geophysical Union in Washington and with the library of the Associated Engineering Societies in New York.]

In order to check formula (1), which is predicated on the unit-graph hypothesis, the author applied the formula to the hydrographs of Figures 4 and 5 in the same manner as we did herein to the Lincoln, Nebraska, hydrographs, given in Figure 1. We found the following: For time 60 minutes (3600 seconds), infiltration-rate 0.93 versus 0.95

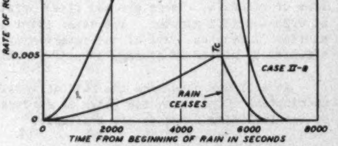
FIG. 4—HYDROGRAPHS OF RUNOFF
(CASE II-a, RAIN 5200 SECONDS AT 0.864 INCH PER HOUR, r=0.65 INCH PER HOUR; CASE II-b, RAIN 5200 SECONDS AT 1.728 INCHES PER HOUR, r=0.95 INCH PER HOUR)

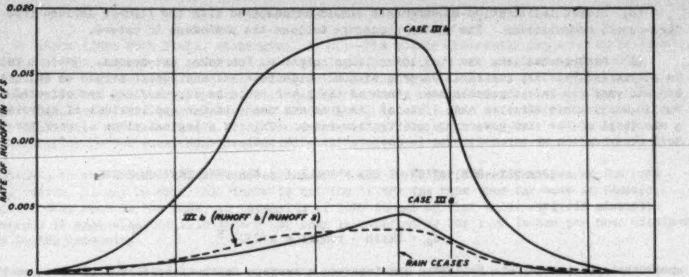

FIG. 5—HYDROGRAPHS OF RUNOFF FROM 1100 SQUARE FEET WITH DETENTION-RESERVOIR OF 100 SQUARE FEET

(CASE IIIa, RAIN 5200 SECONDS AT 0.864 INCH PER HOUR, f=0.65 INCH PER HOUR; CASE IIIb, RAIN 5200 SECONDS AT 1.728 INCHES PER HOUR, f=0.93 INCHES PER HOUR)

and 0.68 versus 0.65. For time 1.44 hours (5200 seconds), mass-losses of 1.42 versus 1.34. The coefficient M was taken as 2. When M was taken at 1.5, the agreement was not as close.

Comparison of formulas (1) and (2)--The author has also applied the values of d to formula (2). This gave a value of K = 29 for Run 22 at Lincoln, Nebraska, and established the relation of surface-detention to runoff-rates. These points plotted on diagram Figure 8 of Horton's Bulletin 101, all fell to the left of Horton's average line, but were within the limits of his St. Louis points shown on his Figure 8. The average line for Run 22 passes through the point q = 0.98 inch per hour and d = 0.13 inch. Run 10 points have an average maximum rate of only 0.17 inch per hour with corresponding value of d_a of 0.088 inch. This extremely small rate of runoff is below all of the St. Louis points. The value of K for Run 10 did not agree with K for Run 22 where the same exponent M was used.

The following comments are presented:

(1) The pluviograph-formula (1) appears to give reliable values of surface-detention. It furnishes a practical approach to the derivation of the surface-runoff formula (2). It can be used on basins with mixed and undetermined slopes, soil-characteristics, and coefficients of roughness. The author, at this time, expresses no preference between formulas (1) or (2).

(2) The lines of mass-losses must leave the mass-rainfall line at the time of initial surface-runoff. The line of mass-losses should be either straight, for uniform infiltration-rates, or else become flatter, with time, for decreasing rates of infiltration--provided correct values of M are used.

(3) However, a slightly erroneous value of M appears to give a parallel mass-loss line and hence does not change the derived infiltration-rate.

(4) The soil at Plot 2, Lincoln, Nebraska, is very porous, as shown by the high infiltration-rates of 1.05 and 1.65 inches per hour.

(5) The infiltration-rate on Plot 2, Run 22, was definitely 60 per cent greater than was the infiltration-rate from Run 10, despite the fact that the initial soil-moisture conditions were practically identical. Does this mean that infiltration-rates increase with increased intensity of precipitation? More light is needed on this matter.

(6) With experimental plots and some other small areas, the observed hydrographs are the hydrographs of surface-runoff. Its abrupt termination is typical.

(7) On large basins, the hydrograph of stream-flow reflects the effect of channel-storage. This is indicated by the large tail. The element of base-flow may also be present. Such hydrographs cannot be used directly for determining infiltration-rates. They require rectification into surface-runoff graphs. This can be done by Horton's channel-inflow method.

(8) Direct infiltration-measurements should be compared with the figures derived from surface-runoff computations. The latter procedure follows the phenomena in nature.

(9) Experiments like the runs 10 and 22 of Lincoln, Nebraska, are needed. Uniform rainfall is not necessary. All available records of continuous rainfall and runoff should be collected and analyzed for infiltration-rates. Much of the labor could be standardized and effected through Public Works Administration help. One of the greatest needs in the applications of hydrology is a knowledge of the laws governing infiltration-rates. This is a logical piece of work for the Soil Conservation Research-Service in cooperation with soil-experts.

APPENDIX--DERIVATION OF THE FORMULA (1) FOR INFILTRATION-RATES

Formula (1) is

$$d_a = (\text{Rain} - \text{runoff} \times 1/C)C^{1/M}$$

where the symbols have the following meanings: d_a = average depth of surfâce-detention, on the basin, at a given time t; rain = accumulated depth of effective rainfall to time t (that is, beginning of this rainfall is taken at the time of observed beginning of surface-runoff); runoff = accumulated depth of measured surface-runoff to time t; C = total depth of measured surface-runoff divided by total depth of effective rainfall; M = Exponent in the runoff-formula $q = Kd^M$; $1/M = 0.67$ to 0.5, or possibly less; t = any given time starting at the beginning of any rainfall.

When the storage equation, Inflow = (Outflow - storage), is applied to rainfall-runoff data on a plot of ground, there are two unknown quantities, namely, "storage" as surface-detention d, and a part of "outflow" represented by infiltration.

In order to solve the equation for infiltration, it is first necessary to find the values of surface-detention d by formula (1).

Following is the derivation of this formula:

If, during the period of surface-runoff, no infiltration took place, then the area (volume) of the hydrograph of runoff would be (1/C) times as great as the area of the observed hydrograph of surface-runoff. Also, under the hypothesis of the unit-graph, this hydrograph with no infiltration-losses would have ordinates of rate of runoff (1/C) times as great as the similar ordinates of the observed hydrograph. It would be Bernard's pluviograph.

In a relatively shallow stream such as overland flow, the hydraulic radius may be taken as the depth d. The rate of flow q will vary as d^M. Let q be the observed rate of overland flow at time t and d be the corresponding depth of surface-detention. Let q_p and d_p be flow and depth of the pluviograph at the same time t. Then

$$q/q_p = d^M/\,d_p^M \qquad (1)$$

With uniform rainfall over the plot there is a fixed relation between d measured at the outlet on the channel-bank and the average value of d for the entire plot. The relationship given in (1) therefore applies to average depth of surface-detention.

In equation (1) q is known from the runoff data.

$$q_p = 1/C \times q \qquad (2)$$

d_p^M is the volume of storage (surface-detention) on the plot at time t with no infiltration-losses. It is equal to the net or effective rainfall up to time t minus the accumulated pluviograph runoff up to time t. Hence

$$d_p^M = [\text{Rain} - (1/C)\text{Runoff}]^M \qquad (3)$$

Substitute (2) and (3) in equation (1), then since

$$q/q_p = \text{runoff}/\text{rain} = C \qquad (4)$$

$$d^M = [\text{Rain} - (1/C)\text{runoff}]^M \times C \text{ or}$$

$$d = [\text{Rain} - (1/C)\text{runoff}]C^{1/M} \qquad (5)$$

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DISCUSSION

H. R. LEACH (5536 30th Place, Washington, D. C.)--The author apparently neglects to include the initial losses which occur prior to the beginning of runoff. These amounted in Run 10 to 0.35 inch and in Runn 22 to 0.14 inch, accounted for partly by infiltration during the period until runoff started and partly by the filling of permanently retained depression-storage. In the first instance this is over one-quarter of the total rainfall and, were the plot-results directly applicable to even a small area, would materially reduce the peak-rate of runoff for any concentration-period exceeding 37 minutes.

Relative to the inquiry in the conclusions as to whether high intensity increases the rate of infiltration, it can be said that there is nothing in the few runs thus far made at Lincoln to indicate that such is the case, an intensity of 2.60 inches on Plot 2 having given an infiltration-rate of approximately 1.15 inches per hour in contrast to the 1.65 inches per hour obtained for a 2.5-inch intensity.

The results of plot-tests must be used with caution when applied to larger areas. One of the greatest problems in connection with such tests relates to their use in predicting the performance of natural watersheds of areas even so small as two or three acres when subjected to natural storm-rainfall.

SOME CHANNEL-STORAGE STUDIES AND THEIR APPLICATION TO THE DETERMINATION OF INFILTRATION

W. B. Langbein

Results of studies of recent major floods have emphasized the practical value of analyses of the relation between rainfall and resultant runoff. Analyses that are directed toward appraising the respective values of the soil and the river-channel as agencies for disposing of potential flood-waters are especially pertinent.

If evaporation-losses from non-infiltrated water may be neglected as inconsequential, as during prolonged storms of winter, the difference between rainfall and the associated runoff may be ascribed to such edaphic influences represented by infiltration, interception, and surface-storage. The problem in the analyses is therefore one of correlating rainfall and resultant runoff. The procedures described in this paper resulted from studies of the rainfall and runoff relations of the Ohio River flood of January 1937 [see 1 of References at end of paper].

Methods of computing mean areal precipitation are generally well known and will not be discussed here. The accuracy of the computed mean areal precipitation, by whatever method chosen, is dependent upon the adequacy of the number of rain-gages per unit of area and their distribution with respect to the basin in question to define the geographical variation of precipitation. The segregation of daily observations of precipitation into separate storm-periods is generally not difficult. However, such separate storm-periods as may be recognized in the record of precipitation, frequently may be identified only with great difficulty upon the hydrographs of resultant stream-flow, largely due to the modulating effects of channel-storage.

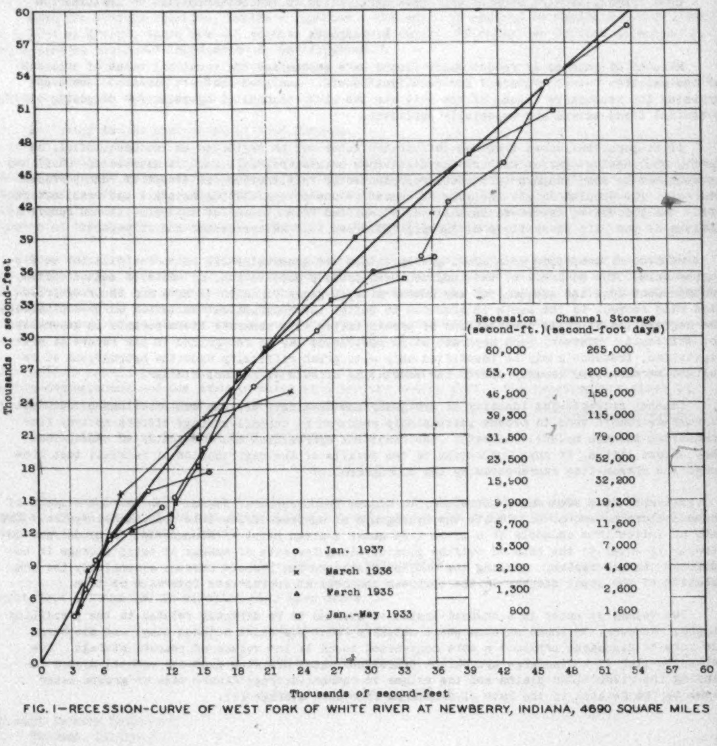
Channel storage--The identity of irregular and seemingly erratic contributions of rainfall to surface-runoff tend to become increasingly obscured by channel-storage effects as they flow downstream through natural channels. The resultant hydrographs are frequently so smooth that they afford little, if any, indication of the details of the distribution of rainfall that produced the stream-flow represented by the hydrograph.

Attempts have been made, therefore, to adjust hydrographs of stream-flow for the effects of channel-storage, and so to compute the hydrograph of surface-inflow into the channel-system. The rate of inflow into channels at a given time above a given point of measurement of discharge, obviously is equal to the rate of outflow plus or minus the rate of change of total storage in the upstream channel-system. Knowing the outflow-discharge the problem becomes essentially the computation of the total storage in the upstream channels at appropriate intervals of time.

The volume of water in a channel-system is assumed to be directly related to the prevailing stages. However, in those streams where definite relations exist between stage and discharge, the rate of discharge provides a more convenient index to the volume of channel-storage. The recession-limb of a discharge hydrograph represents water draining from the channel-system including the river-flood plains and the volume in channel-storage (above base or ground-water flow) may be related to the rate of discharge from such storage [2].

In small basins, the discharge at the outlet point may serve as a satisfactory index of the stages prevailing upstream. However, in large basins, particularly those of drainage-area over 500 square miles, one gage at the outlet of the basin will not generally serve as a sufficient index. For example, at the beginning of a storm, the gage at the outlet may show but negligible rise, yet the channels upstream may be considerably swollen, and moreover, in the larger basins, after the storm has ended, the lower may be at peak and yet the stations upstream may have long passed their peak. Furthermore, floods over large basins are not of uniform intensity. To evaluate these possibilities adequately, it is essential to make use of upstream gages, by using each gage only as an index of channel-storage in the area intervening between it and other gages upstream. Naturally the accuracy obtainable largely depends on the adequacy of the number of gaging stations available for this purpose. Through the use of upstream gages, weight may be given to floods which affect only a part of the basin or to the variable flood-intensities over the basin.

In the preparation of the discharge-channel-storage relationship in a given basin, study must be made of past floods to obtain recession-hydrographs that are free from contemporaneous inflow from surface-water. It has been found that complete recession-hydrographs meeting this requirement are rare. This experience led to the adoption of the following procedure for determining a normal recession-graph or graph free from complications of inflow. When two or more days of apparently normal recession have been found, one day's discharge has been plotted as ordinate against the following day's discharge as abscissa on rectangular coordinates, as illustrated on Figure 1. (Base or ground-water discharge must of course be deducted.) Lines have been drawn connecting points on the same recession-graph. These points and lines were then used as a basis



Recession (second-ft.)	Channel Storage (second-foot days)
60,000	265,000
53,700	208,000
46,800	158,000
39,300	115,000
31,500	79,000
23,500	52,000
15,900	32,200
9,900	19,300
5,700	11,600
3,300	7,000
2,100	4,400
1,300	2,600
800	1,600

FIG. 1—RECESSION-CURVE OF WEST FORK OF WHITE RIVER AT NEWBERRY, INDIANA, 4690 SQUARE MILES

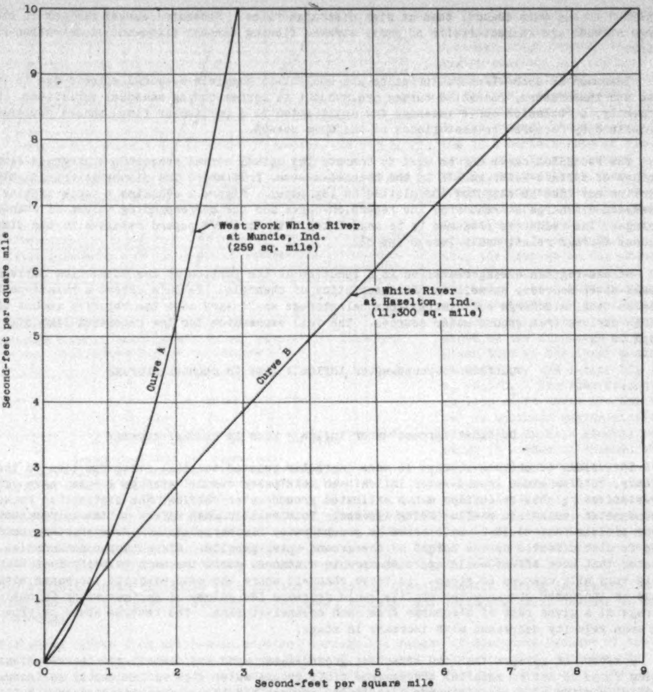


FIG. 2—TYPICAL RECESSION-CURVES

for drawing a curve which must pass through the origin, which curve has been termed the recession-curve. In drawing the recession-curve, greater weight is given to these points which show the most rapid rate of draining out, that is, those which plot to the left on Figure 1 and which show a tendency to define a curve. Care is exercised, however, to disregard those singular points which plot so far to the left as to be indicative of drainage from partial-area flood-rises.

The shape of the recession-curve is indicative of the channel-storage characteristics of the particular basin. A straight-line recession-curve indicates that the recession-hydrograph is of exponential form, $q = ae^{-bt}$ where a and b are constants, q is discharge, and t is time, in which event the curve of the discharge-channel-storage relation is a straight line at a 45° slope on logarithmic paper, and the volume of channel-storage bears a constant ratio to the discharge-rate. The recession-curves of small upstream basins frequently are steep at high discharge-rates but rapidly flatten as they approach the origin (see curve A of Fig. 2). This characteristic of these curves is due to the high velocities peculiar to these streams and efficient channel-conveyance at high rates of discharge incident to steep slopes, whereas at low rates of discharge there are numerous pools to be drained eliminating the influence of slope and with channel-conveyance relatively poor provided these pools are not drowned out by a high base or ground-water flow. The recession-curves of large basins, on the other hand, appear to be steeper at low discharge-rates than at higher discharge-rates. The upper end of the curve cannot exceed a limiting 45° slope (see curve B of Fig. 2). The lower reaches of large streams have wide overflow areas which provide large volumes of channel-storage but do not add proportionately to the rate of discharge inasmuch as overflow areas do not flow at efficient rates of discharge. Large rivers, however, flow with relatively greater velocity at low discharge-rates when the river is confined to the main channel than at high discharge-rates. Recession-curves similar to those of large streams are characteristic of small streams flowing through fixed-outlet detention-reservoirs.

Inasmuch as channel-characteristics are subject to possible seasonal effects due to vegetation and temperature, recession-curves are subject to corresponding seasonal variations. Consequently, a recession-curve intended for application to a particular flood should preferably be defined by recourse to past floods of the same season.

The recession-curve may be used to compute the actual normal recession-hydrograph from storage of surface-water runoff in the channel-system, from which the discharge-channel-storage relation may then be computed and plotted on log paper. Figure 1 contains a table listing daily recession-discharge as read from the recession-curve and the corresponding volume of channel-storage. The reader is presumed to be acquainted with Horton's papers relative to the discharge-channel-storage relationship [see 2 and 3].

Stream-regimen during recession is a function of the portion of the total flow derived from ground-water sources, as well as characteristics of channels. To this extent a relationship between total discharge and upstream channel-storage would vary with the relative amount of discharge derived from ground-water sources. The full expression for the recession-limb of a hydrograph is

Outflow = Ground-water inflow + loss in channel-storage

or

Outflow - ground-water inflow = Loss in channel-storage

The volume of channel-storage is more correctly related to total discharge than to the difference, outflow minus ground-water inflow. In this paper channel-storage volumes have been nevertheless related to outflow minus estimated ground-water outflow, the distinction between ground-water inflow and outflow being ignored. This relationship refers to the volume above the river-profile that would be maintained by ground-water discharge alone. However, this relationship is also affected by the height of the ground-water profile. Elementary consideration indicates that such effect would approach zero in a channel where the mean velocity does not sensibly vary with changes in stage. In those channels where the mean velocity increases with the stage an increased ground-water profile would decrease the volume of surface-water channel-storage at a given rate of discharge from such channel-storage. The reverse would be true where the mean velocity decreases with increase in stage.

However, it appears that the effect of ground-water upon the channel-storage relationship during flood is not of material consequence since ground-water flow is frequently an inconsequential portion of flood-discharge. At least, errors from this source do not approach those from other sources.

Figure 3 presents a comparative study of the computed discharge-channel-storage relations at the several stations in the White River Basin. This Figure shows the channel-storage in second-foot-days per square mile, against drainage-area at the several stations, when the rate of outflow is 1, 5, and 10 second-feet per square mile. As might be predicted the upward trend in unit channel-storage in a downstream direction is very marked. Channel-storage-relation graphs for stations on the same stream can be adjusted to conform with the mean trend and thus made consistent with one another and accidental errors eliminated. It may be observed that the trend-lines show appreciable divergence in a downstream direction, in such a way that at low flows (1 csm) the dendrition shows less tendency to increase downstreamward than at high flows (10 csm). This observation confirms the deductions regarding comparative channel-capacities made in connection with discussion of the recession-curves. Moreover, this observation seems to demonstrate that the average slopes of the graph of discharge-channel-storage relation--the value of M in $Q_c = K_{s_c} S_c^M$ [2]--are not the same at all stations on the same river-system, but are greater at upstream stations than at downstream stations. For example, the value of M for the West Fork of the White River at Muncie, Indiana, is about 1.2 and for the White River at Hazelton, Indiana, is about 0.7.

To compute the daily volumes of channel-storage during a flood-rise in the channel-system of a large basin, the daily storage is first computed at all most upstream gaging-stations by simple application of the curve of discharge-channel-storage relation to the mean daily net discharge, that is, observed discharge minus estimated base or ground-water flow. To the volumes

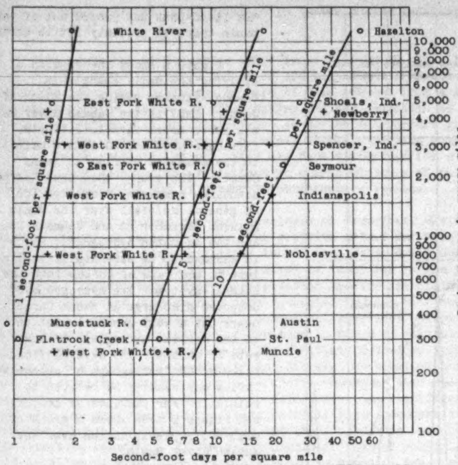


FIG. 3—COMPARISON OF DISCHARGE CHANNEL-STORAGE RELATIONS AT INDICATED GAGING STATIONS OF THE WHITE RIVER BASIN

at the upper station are respectively added the volumes in the drainage-areas between the upper gaging-stations and the next lower gaging-station, computed as follows. Assuming that at two upstream gaging-stations A and B, the channel-storage corresponding to a certain rate of flow per square mile is represented by A_s and B_s, respectively, and at the lower gaging-station, C, the channel-storage corresponding to the same rate of flow per square mile is equal to C_s, then the storage in the channels below gaging stations A and B and above C at a certain time may be approximated by multiplying the channel-storage, as determined by the discharge at the given time at the lower gaging-station C by the factor $(C_s - A_s - B_s)/C_s$. The same reasoning applies where there are any number of upstream gaging-stations, the factor in more general terms being [1 - (Sum of channel storage at all upstream gaging-stations at same unit-rate of discharge divided by channel-storage at lower gaging-station at the same unit-rate of discharge]. The volume of channel-storage in the intervening area above station C, as thus computed, is added to the volume above the upper stations to obtain a value of the channel-storage above station C that is considered more correct than can be obtained by the use of the discharge-hydrograph of station C alone.

The computation is continued from station to station downstreamward adding increments in intervening storage, bringing in storage on tributary streams, and including daily volumes in artificial storage.

The above factor does not remain constant through all ranges of discharge because of the non-parallelism of the trend-lines shown on Figure 3, and previously discussed. However, by evaluating the factor at the mean unit-rate of discharge (second feet per square mile) prevailing during the given flood, a value will be obtained that may be used with confidence through the flood in question.

Although an approximation, this procedure affords a method of computing volumes of flood-water in channel-storage with respect to time and place. The part of the volumes of flood-water measured by the described method are illustrated on Figure 4. Since it omits that volume above the normal recession-profile extended upstream from the stage at each gaging station, the method inherently gives values of the channel-storage during rising stages that are low, although where a number of gaging stations may be used the error is probably not great. During recession after

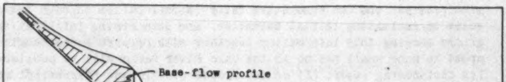


FIG. 4—RIVER-PROFILE DURING FLOOD SHOWING VOLUMES ACCOUNTED FOR BY PROPOSED COMPUTING PROCEDURE

the flood-peak has passed out of the basin there is probably little error.

Figure 5 shows the results of applying the daily changes in channel-storage during the flood of January 1937 in the Wabash River Basin above Mount Carmel, Illinois, (drainage-area 28,600 square miles) to the measured outflow at Mount Carmel. The graphs show the observed discharge resulting from a period of general rainfall over the basin between December 26 and January 25 and the adjusted hydrograph, which is assumed to be essentially equivalent to the rate of inflow into the channel-system. The hydrograph of observed discharge at Mount Carmel bears but a very general relation to the derived hydrograph of the rate of inflow. The several flood-rises of the hydrograph of channel-inflow may readily be related to rainfall. For purposes of comparison Figure 5 also shows a graph of mean daily precipitation over the Wabash River Basin.

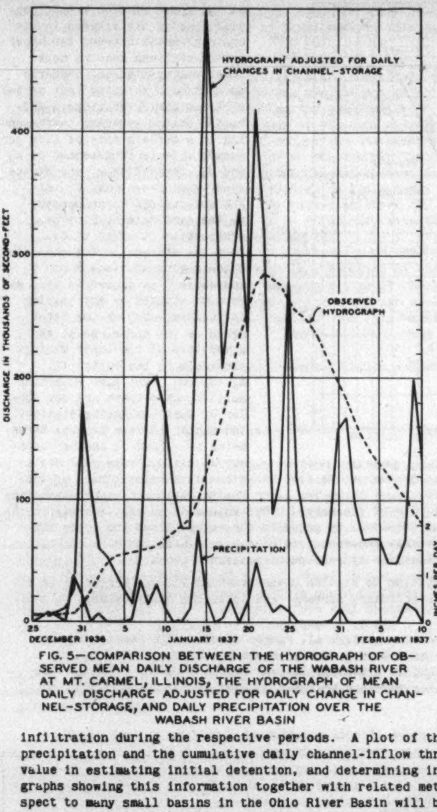


FIG. 5—COMPARISON BETWEEN THE HYDROGRAPH OF OBSERVED MEAN DAILY DISCHARGE OF THE WABASH RIVER AT MT. CARMEL, ILLINOIS, THE HYDROGRAPH OF MEAN DAILY DISCHARGE ADJUSTED FOR DAILY CHANGE IN CHANNEL-STORAGE, AND DAILY PRECIPITATION OVER THE WABASH RIVER BASIN

To compute the average infiltration over the basin during the storm-period, the total storm-precipitation is divided into as many reasonably distinct storm-periods as may be recognized, and the associated runoff computed on the basis of the hydrograph of computed inflow into the channel-system. Unless sufficient time has elapsed between storm-periods to permit water in transit over ground-surfaces to reach the stream-channels, the difference between the rainfall and runoff must be corrected for such surface-detention to obtain the total infiltration during the respective periods. A plot of the difference between cumulative daily precipitation and the cumulative daily channel-inflow through the storm-period is of essential value in estimating initial detention, and determining infiltration during the storm. Several graphs showing this information together with related meteorologic and hydrologic data with respect to many small basins in the Ohio River Basin will be published by the Geological Survey in its forthcoming report [1] on the floods of Ohio and Mississippi rivers, January to February, 1937.

The infiltration is a function of the rainfall-supply and of the ground-surface conditions. When the total infiltration is known and sufficient knowledge of characteristics of rainfall are available, the limiting capacity of the soil to absorb water under the given conditions, termed the infiltration-capacity, may be computed. Many methods are available for this purpose. The following is intended to present a procedure of computation adapted to ready computation over wide areas, during prolonged storms, and where the usually limited data only are available.

Computation of the infiltration-capacity--Doctor Horton [4] has described a method of computing the average infiltration-capacity over a drainage-basin during widespread rain-storms where the runoff is known and the rates of rainfall are available as furnished by a recording rain-gage. The procedure described by Horton is directly related and largely conforms to the theory that surface-runoff occurs only when rain falls at a rate in excess of the capacity of the soil to absorb water under the conditions existing at the time or the infiltration-capacity. Aided by appropriate curves described by Horton, a value of the infiltration-capacity is found by trial such that the total rainfall in excess of the infiltration-capacity equals the known surface-runoff.

Some practical difficulties arise in an attempt to apply Horton's procedure to compute the infiltration-capacity over a number of drainage-basins, affected by the same widespread storm since any individual basin may be more or less removed from a recording rain-gage. Recording rain-gage records, today are still sparse and the selection of a base-gage to apply to a given basin is involved with uncertainties. In Horton's procedure greater emphasis was apparently placed on the characteristics of rainfall at the recording gages than on the geographical variation of these characteristics. Need has been recognized for a technique which would give more weight to the geographical variations in characteristics of rainfall and which would be capable of ready application and of producing satisfactory results.

A systematic method has been developed as described herein, which conforms to the theory of surface-runoff as defined above. The proposed method has been applied to the determination of the approximate infiltration-capacities in the Ohio River Basin during the storms of December 1936 to January 1937 [1]. The total infiltration as described in the previous section is equal to the difference between total storm-precipitation and the associated surface-runoff provided that changes in surface-storage have been accounted for and that evaporation-losses from non-infiltrated water may be neglected as inconsequential. After computing the total infiltration a suitable statistical criterion of duration in hours of rainfall has been derived so that division of the known total infiltration by the duration defined by this criterion, will yield the correct value of the infiltration-capacity and satisfy the equation that the total rainfall in excess of the infiltration-capacity = surface-runoff. Such duration will be termed herein the duration of significant rainfall and the corresponding rate, the significant rate of rainfall. It is apparent that the duration of significant rainfall lies somewhere between the total period of rainfall and the duration of rainfall-excess and its location in this range bears a relation to the infiltration-capacity. If the relationship of rate of rainfall and duration were a straight line, the duration of significant rainfall would be equal to the mean of the total storm-duration and the duration of rainfall at rates in excess of the infiltration-capacity. Also for a straight-line relationship the duration of significant rainfall would equal the duration of rainfall at a rate equal to one-half the infiltration-capacity. Either of these relations would afford a rough approximate method for determining duration of significant rainfall. But the relationship of rate and duration of rainfall is rarely a straight line, but rather is generally exponential in form. Consequently, some adjustments are necessary to arrive at a satisfactory approximation. Six methods have been derived by empirical processes and the results compared. These methods fall into two categories, each related to the two approximate methods indicated above. Three methods are in the first category, A, based upon the combination of two durations, one in terms of the total rainfall-duration and the second in terms of the duration at the infiltration-capacity. Three are in the second category, B, based upon a single duration between the two durations just mentioned, but expressed in terms of the infiltration-capacity.

Method	Formula for criterion
A-1	$t = (t_T + t_f)/2$
A-2	$t = (t_{.01} + t_f)/2$
A-3	$t = 0.42t_{.01} + 0.58t_f$
B-1	$t = t_{.5f}$ = Duration of rainfall at 0.5 of infiltration-capacity
B-2	$t = t_{.6f}$ = Duration of rainfall at 0.6 of infiltration-capacity
B-3	$t = t_{0.4f+0.007}$ = Duration of rainfall at 0.4 of infiltration-capacity +0.007 inch per hour

In these formulas, t is the duration in hours of significant rainfall, t_T is duration of rainfall-intensities per hour equal to or in excess of "trace", $t_{.01}$ is the duration of rainfall equal to or greater than 0.01 inch per hour, and t_f is the duration of rainfall equal to or greater than the infiltration-capacity.

The six methods listed above have been tested by trial within a range of 0.01 to 0.10 inch per hour in infiltration-capacity with 13 automatic rainfall-records. The results have been averaged and are compared on Table 1 which shows the relation between the values of the infiltration-capacity computed by the method indicated and the value of the infiltration-capacity computed by the exact method. Methods A-3 and B-3 apparently give the best results.

Table 1--Comparison of computed value of the infiltration-capacity with correct values

Method	Average ratio of computed value to correct value of infiltration-capacity	Mean deviation of computed values from correct values of infiltration-capacity
	per cent	per cent
A-1	89	11
A-2	96	4
A-3	100	4
B-1	98	7
B-2	105	9
B-3	100	4

Inasmuch as these approximate methods are expressed in terms of the unknown infiltration-capacity, their application requires the use of successive approximations, although such approximations tend rapidly to converge upon the desired result.

The first step for computing the infiltration-capacity of a given basin is the preparation of a map of the region upon which are plotted at the points representing recording rain-gage sites, the durations of rainfall during the given storm, at rates equal to or in excess of one-half of the estimated average infiltration-capacity of the basin. At the same points there are likewise plotted the average total precipitation divided by the duration of rainfall as determined above, which quotient has been termed the precipitation-ratio. Two systems of smooth lines are now drawn upon this map, one being lines of equal duration of significant rainfall and the other being lines of equal ratios of total precipitation to duration of significant rainfall. These lines are so drawn that the product of the two factors at strategic points of intersection equals the known storm-precipitation, at such points. Figure 6 shows a map of the Ohio River Basin, showing a system of these lines for the storm of December 27, 1936, to January 5, 1937. It may be observed that the two systems of lines approximate a trajectory.

Two values of the duration of significant rainfall at any point can be determined from this map by reading off the duration directly as well as the average rate at the same point. The storm-precipitation at the given point, as determined from an isohyetal chart or other method is divided by the precipitation-ratio as read from the map as above and thus the second value of the duration is found. The average of the two may be used as the proper value of the duration of rainfall, equal to or in excess of the selected rate of precipitation. If the lines are properly drawn so that their product at all points is equal to the precipitation, the two values so derived will be equal. This suggested method for determining the duration of rainfall has an advantage in that it gives appropriate weight to varying precipitation over the basin as well as to any meteorologic features that one may wish to consider in drawing the lines of equal duration.

It may be observed on Figure 6 that the precipitation-ratio varied relatively less over the basin than the duration of significant rainfall. The former varied from a minimum of 0.073 inch per hour to a maximum of 0.205 inch per hour, whereas the latter varied between nine and 90 hours.

Precipitation at any point is equal to the product of the duration and the precipitation-ratio and at stations having different storm-precipitation, one or both of these factors must be different during the given storm. Figure 6 indicates that the rate-factor is subject to less variation than duration but that higher precipitation, nevertheless, is accompanied by a higher average precipitation-ratio as well as a higher duration. These observations have been confirmed by a separate study of total storm-rainfall, rate and duration experience at 43 recording rain-gage stations in the Ohio River Basin, during the storm of January 20 to 25, 1937. The average rainfall-experience during this storm may be summed up as follows: Stations with one inch of precipitation had an average precipitation-ratio of 0.10 inch per hour and a duration of significant rainfall intensity of 10 hours, whereas stations with 10 inches of precipitation had an average precipitation-ratio of about 0.15 inch per hour and a duration of 67 hours. In this example precipitation increased ten times, while the average precipitation-ratio increased 1.5 times and the duration 6.7 times.

As stated, Figure 6 related to the duration of rainfall at rates equal to or in excess of one half of the estimated infiltration-capacity. Duration at other rates within a limited range may be computed in the following manner: The rate-duration relationship is generally of the exponential form, $d = aK^{-r}$ where a and K are constants and d is the duration of rainfall at rates equal to or in excess of rate r. Therefore, if the duration d at a given intensity r is known, then the duration d_{r+1} at an intensity $(r+1)$ is $K \times d_r$. Therefore $K = d_{r+1}/d_r$. The value of K at a given recording rain-gage station during a storm is best expressed as the ratio of the dura-

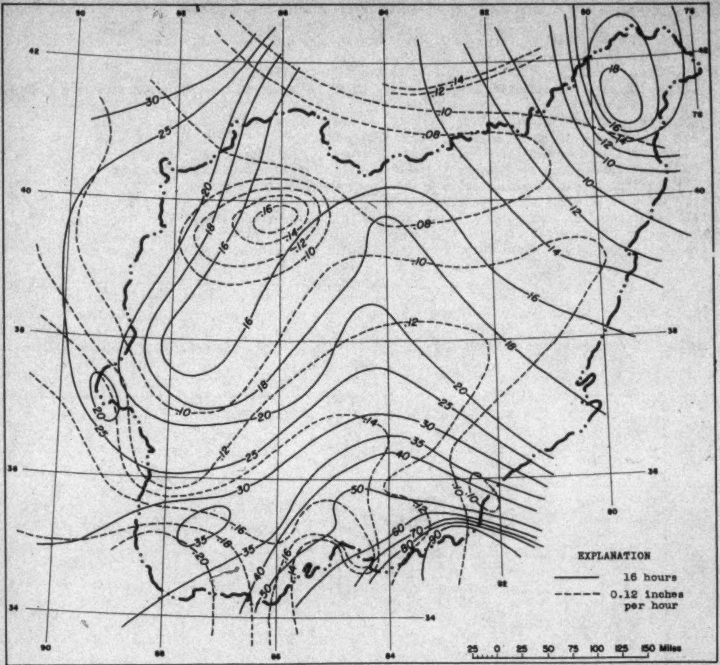


FIG. 6—MAP OF OHIO RIVER BASIN SHOWING LINES OF EQUAL DURATION OF RAINFALL AT RATES EQUAL TO OR GREATER THAN 0.03 INCH PER HOUR (SIGNIFICANT RAINFALL) AND LINES OF EQUAL RATIOS OF TOTAL PRECIPITATION TO DURATION OF SIGNIFICANT RAINFALL, DECEMBER 26, 1936 TO JANUARY 5, 1937

tion at intensities of intervals of 0.01 inch per hour and is most easily computed by the formula

$$K = \sqrt[n]{d_{r+n}/d_r}$$

where n is the number of units of 0.01 inch per hour between (r + n) and r. Although only two points on the rate-and-duration relationship need be known, it is desirable to select three points, one at the duration used to prepare Figure 6, and one 0.03 inch per hour greater and another 0.03 inch per hour less. In this way two values of K are computed and there is no necessity for extracting any root greater than the third.

Example--At a certain gage the duration of rainfall at an intensity of 0.02 inch per hour or more was 40 hours, the duration at 0.05 inch per hour was 27 hours, and the duration at 0.08 inch per hour was 9 hours. The two values of K are then $K = \sqrt[3]{27/40} = 0.77$ and $K = \sqrt[3]{19/27} = 0.79$, the average being 0.78.

A map is then prepared upon which is plotted the value of K at recording-gage records and lines of equal value of K drawn upon it (see Fig. 7).

Sufficient data are now available for computing the approximate infiltration-capacity for any basin in the area studied provided the precipitation and direct runoff are known.

Example--For Greenville Creek Basin above Bradford, Ohio, we have:

(1) Total precipitation, December 27, 1936, to January 5, 1937 = 1.6 inches
(2) Total direct runoff = 0.45 inch
(3) Difference [(1) - (2)] = total infiltration = 1.15 inches
(4) Duration of precipitation at rates equal to or in excess of 0.03 inch per hour from Figure 5 is 16.5 hours
(5) Precipitation-ratio from Figure 5 is 0.082 inch per hour
(6) Second value of duration is 1.6/0.082 = 19.5 hours
(7) Average duration is (16.5 + 19.5)/2 = 18 hours
(8) Approximate value of infiltration-capacity f = 1.15/18 = 0.064 inch per hour
(9) Significant rate (method B-3) = 0.4f + 0.007 = 0.033 inch per hour
(10) Value of K from Figure 7 is 0.78
(11) Duration of precipitation at 0.04 inch per hour or greater is 18 × 0.78 = 14 hours
(12) Therefore, duration of precipitation at the significant rate of 0.033 is 16.8 hours
(13) Second and final value of infiltration-capacity is 1.15/16.8 = 0.069 inch per hour

References

[1] Geol. Sur. Water-Supply Paper 838, Floods of Ohio and Mississippi rivers, January to February 1937.

[2] R. E. Horton, Natural stream channel-storage, Trans. Amer. Geophys. Union, 17th annual meeting, pp. 406-415, 1936.

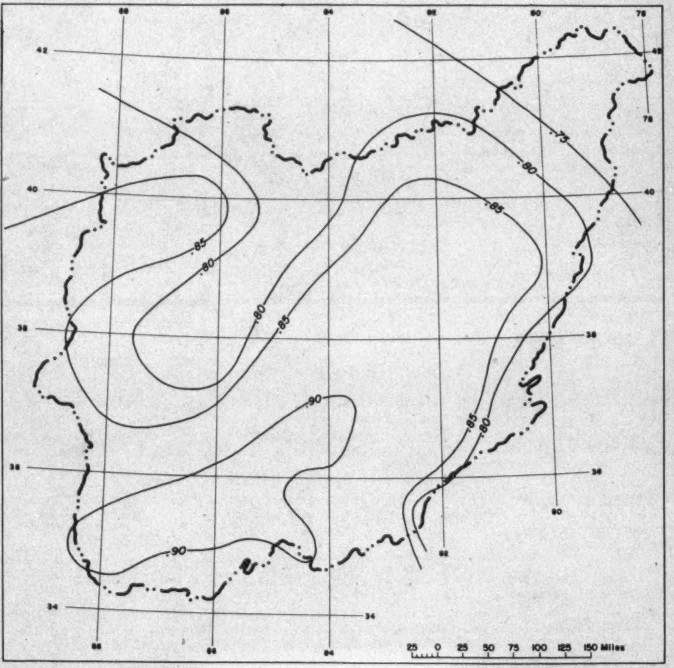


FIG. 7—VALUES OF K IN OHIO RIVER BASIN DURING STORM OF DECEMBER 26,1936 TO JANUARY 5,1937

[3] R. E. Horton, Natural stream channel-storage, Trans. Amer. Geophys. Union, 18th annual meeting, pp. 440-456, 1937.
[4] R. E. Horton, Determination of infiltration-capacity for large drainage-areas, Trans. Amer. Geophys. Union, 18th annual meeting, pp. 371-385, 1937

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DISCUSSION

L. L. HARROLD (1425 Van Buren Street, N.W., Washington, D. C.)--The paper presented by the author serves a very worthy purpose in that it again brings before the attention of the hydrologists and hydraulic engineers the importance of channel-storage in studies of rainfall and runoff.

The author is correct in the statement that the upper end of the recession-curve cannot exceed a limiting slope of 45°. This means that the difference in the discharge from day to day is decreasing and the hydrograph of stream-flow has a concave shape. In other words the recession-curve is that portion of the hydrograph which occurs after the point of inflection on the recession-side of the runoff-graph.

In determining the recession-curve only that portion of the recession-side of the runoff-graph should be used which is beyond the point of inflection. On Figure 1 of the author's paper many points indicate that they are to the left of the point of inflection due to the fact that the difference increases from day to day. The fact that differences in the daily flows as tabulated oh Figure 1 increase until the flow has fallen to 25,000 cfs indicates that the average recession-graph from 60,000 second-feet down to 25,000 second-feet (Fig. 1) has a slope to the right of a limiting 45°-line and that the author has given weight to that portion of the recession-side of the runoff-graph which occurs prior to the point of inflection.

The author has presented a good method for arriving at a better figure for the channel-storage for large watershed than that which would have been computed by using only one discharge-channel-storage relation. A word of caution, however, should have been made so that the figures of channel-storage in second-feet-days per square mile (Fig. 3) would not be used in the correction-factor. If these figures are used, it is possible to arrive at a negative storage-difference between stations on the stream-system. Storage in units of second-feet-days instead of second-feet-days per square mile should be used in the correction-factor in obtaining the difference in channel-storage between stations.

The writer computed the difference in storage between the West Fork of White River at Newberry, Indiana, and the East Fork of White River at Shoals, Indiana, and the White River at Hazleton, Indiana, using first a flow of ten csm, and then one csm, as a basis for computing the correction-factor. The difference in the total storage at White River at Hazleton as computed by the use of two different flows amounted to less than one per cent.

WALTER B. LANGBEIN--Mr. Harrold points out that in determining the normal recession-graph only that portion of the runoff-hydrograph which follows the point of inflection should be used. It is assumed that reference is made to the presentation in "Surface-runoff phenomena" by R. E. Horton [Pub. 101, Horton Hydrological Laboratory, February 1, 1935] for support of this statement. The theory as described by Horton is that inflow into channels from surface-runoff occurs previous to the time of the point of inflection, and that only after the point of inflection has been passed does the hydrograph represent drainage from channel-storage alone. The point of inflection in this connection is of hydrologic significance, marking the transition from the hydrograph of discharge as maintained by inflow into the channels which is concave downwards to that portion of the hydrograph representing simple channel-storage drainage and which Horton describes as always being concave upwards.

The significance of the point of inflection insofar as it may be indicative of the time of cessation of surface-water inflow is apparently lost when large basins are considered since the relationship between singular points of the distribution of rainfall and the resultant hydrograph of runoff are largely obscured. Consequently, the theory which attaches this significance to the point of inflection is applicable only to small basins. This restricted application is necessary for another important reason, namely, the geometrical form of the recession-limb representing drainage of channel-storage of large basins is not necessarily concave upwards as it almost always is for small basins.

Channel-characteristics vary widely between the range of low water and extreme flood-stages such as encountered in recent flood-experiences. These variable channel-characteristics as they relate to conveyance and corresponding storage-volumes are necessarily reflected in the recession-limb.

Some discussion has already been given of this relationship. Although it may be impossible to write a single expression for the discharge and channel-storage volume for the entire range of stages experienced in a given river, it may be possible within a restricted range to write

$$Q_c = KS_c^M \text{ and } Q_c = dS_c/dt$$

with notation as given in [2] and [3].

The exponent M is related to the conveyance and the physical dimensions of the tributary channel-system of the given basin. Values of M greater than unity seem to be characteristic of channel-systems in which all water moves downstreamward at efficient rates of discharge. Increasing volumes of impounded water either in lakes or over wide flood-plains reduce the mean velocity and result in lowered values of exponent M. It is physically possible for all real positive values of M to be encountered. The value of M determines the geometrical nature of the recession-limb of the hydrograph. Five types or forms of recession hydrograph-limbs may be recognized in relation to the value of M. These types and their geometrical characteristics are as follows:

Type A: $M > 1$--
(1) Mean velocity, (Q_c/S_c), increases with rise in stage
(2) Recession-curve concave upwards
(3) Recession hydrograph-limb concave upwards

Type B: $M = 1$--
(1) Mean velocity, (Q_c/S_c), is a constant
(2) Recession-curve is a straight line through the origin
(3) Recession hydrograph-limb, concave upwards, plots as straight line on semi-logarithmic paper

Type C: $1 > M > 1/2$--
(1) Mean velocity, Q_c/S_c, decreases with rise in stage
(2) Recession-curve concave downwards, as $Q \rightarrow 0$, the curve becomes asymptotic to the vertical, and as discharge increases the curve approaches a 45°-slope
(3) Recession hydrograph-limb concave upwards

Type D: $M = 1/2$--
(1) Product of mean velocity and discharge is a constant, $S_c \times Q_c =$ constant, mean velocity, Q_c/S_c, decreases with increase in stage
(2) Recession-curve is a straight line at 45°-slope but does not pass through origin.
(3) Recession hydrograph-limb is a straight line.

Type E: $0 < M < 1/2$--
(1) Mean velocity, Q_c/S_c, decreases with increase in stage
(2) Recession-curve is concave upwards approaching a 45°-slope as discharge increases
(3) Recession hydrograph-limb is concave downwards

It should be noted that the recession hydrograph-limb of Type E is concave downwards. This Type is not at all impossible. R. E. Horton [2] gives a table listing values of M which includes several medium-sized basins having recession-limbs approaching Type E.

The slopes of recession-curves of types A and B are steeper than 45° throughout their range, but it may be noted that the upper limits of recession-curves of types C, D, and E with increasing discharge each approach a limiting 45°-slope. This fact has served as the basis for the statement to which Harrold refers in the second paragraph of his discussion. Except for Type E the slope of the recession-curve is never less than 45°.

Very rarely does the recession-curve of a given basin fit the same one of these types throughout its range, although many small basins are of Type A and many approach Type B. The author has found but few recession-curves of Type E. However, it is desired to point out that such curves within a limited range are distinct possibilities and particularly so in the case of large rivers with wide overflow- or lake-areas. The upper end of the recession-curves of such rivers frequently are of Type E. The recession-curve of the Stillwater River below the Englewood Retarding Basin at Englewood, Ohio, is an example of this Type. The drainage-area above the gaging station is 646 square miles, but the fixed-outlet retarding-basin provides an artificial increase in storage so that the recession-hydrographs are of Type E.

When the level of the reservoir is below the top of the outlet conduits, the natural drainage-characteristics of the drainage-basin are not modified and the recession-curve in the range of discharge below the outlet top is of Type A. The value of M in this range is approximately 1.2. However, when the level of the pool is above the top of the outlet conduits, the value of M is reduced to 0.35, making the recession-hydrographs of Type E and concave downwards. Hence, when the reservoir drains, the recession-limb of the hydrograph is concave downwards until the level of the top of the fixed-outlet conduits is reached, when the recession-hydrograph becomes concave upwards. This example is given in preference to a natural river-basin because the discharge-channel-storage relationship derived as previously explained in this paper and whose characteristics have just been discussed, was checked by addition of the known volumes of water in the reservoir at a given discharge at the gaging station and volumes of channel-storage above a gaging station on the Stillwater River above the Reservoir at Pleasant Hill, Ohio, (drainage-area 502 square miles). The recession-curve at the latter place is of Type A, the value of M being approximately 1.1.

The upper end of the recession-curve shown on Figure 1, to which Harrold refers, is of Type E. It is not considered essential to the purpose of this paper to support the accuracy of the particular curve shown on Figure 1; subsequent data may support it or not. However, its accuracy cannot be challenged without adequate study of the hydraulic characteristics of the tributary basin. The fact that the value of M for high discharge-values is less than 0.5 is not in itself sufficient evidence to discredit it. Mr. Harrold's remarks concerning Figure 3 are well taken. Volumes of channel-storage in a given problem should be reckoned in second-foot-days and not in terms of unit channel-storage as shown on Figure 3. This figure is presented to demonstrate variations in unit channel-storage and to suggest a method for adjustment of discharge-channel-storage graphs.

SYNTHETIC UNIT-GRAPHS

Franklin F. Snyder

Synopsis--This paper presents a method, mostly empirical, of deriving synthetic unit-graphs. It is of assistance in the study and analysis of runoff-characteristics of drainage-areas of from 10 to 10,000 square miles for which stream-flow records may or may not be available. No attempt has been made to eliminate the use or need of judgment and experience in such studies.

The "lag" or time from center of mass of rainfall to peak of runoff is the principal drainage-basin characteristic used in deriving the synthetic unit-graphs. An approximate method of determining the "lag" is given for use on areas with no stage-records available. The peak-rate of the unit-graph is expressed as a function of the "lag".

A distribution-graph is also determined by means of the lag. Knowing the peak-rate and the distribution-graph for the area in question, a unit-graph can then be constructed. An example of this procedure is given for the French Broad River Basin above Dandridge, Tennessee.

The additional use of the lag-characteristic in flood-forecasting is described and in conclusion, the limitations of the method are discussed.

Introduction--It is assumed that the reader is familiar with the basic theory and practical application of unit-graphs. If not, reference is made to them [see 1, 2, and 3 of "References" at end of paper]. As used by the author, a unit-graph is the discharge-graph of one inch of surface-runoff from a given area for a typical or specified type of storm of some unit of duration. The basic assumptions of the unit-graph method are that for such storms the time-duration of discharge of surface-runoff is a constant for any basin and the ordinates are proportional to the total amount of surface-runoff and distributed according to the unit-graph shape. A distribution-graph expresses the unit-graph by giving the discharge-volume of successive equal time-periods as a percentage of the total volume.

The procedure herein described for deriving unit-graphs was developed mainly for studying and forecasting stream-flow on areas with no immediately available records of runoff. However, it has been of material assistance in the study of areas for which discharge or gage-height records are available.

True unit-graphs are not directly obtainable from published mean daily discharges, although distribution-graphs based on 24-hour periods can be worked up and serve as an aid in outlining the unit-graph.

Mean daily discharges may indicate a single smooth runoff-graph when actually two closely spaced rises may have occurred. Actual drainage-basin characteristics and irregularities may also be concealed. In addition, the fortuitous synchronization of runoff with the calendar day will affect the results. Some of these difficulties are reduced if recording rainfall-records are available but, until recently, this has not generally been the case.

Even with continuous records of river-stages available, and this is now the case at the majority of river-measurement stations, the investigator who is trying to derive unit-graphs for the first time is often discouraged. The usual procedure is to obtain unit-graphs for all stream-rises that are fairly well isolated and seemingly the result of storms of about the same duration. When these unit-graphs are compared, the variations may often seem too great for substantiation of the unit-graph method.

These differences are mainly explainable by variations in the lengths of the surface-runoff-producing storms, unequal distribution of rainfall, differences in storm-paths, and runoff from snow or ice if the area is so affected.

None of the conflicting results may be safely disregarded until the reasons for the discrepancies are determined. Likewise, the unit-graph desired may be one for a special or typical type of storm rather than one of uniform intensity and distribution of rainfall, especially if the basin is one on which the latter very seldom occurs.

The selection of a unit of duration is the next step. In studying small areas this may need to be as short as six or even one hour. The problem then is to develop a unit-graph for use with data of rainfall for one or six hours from the available unit-graphs based on storms of from probably six to 36 hours in duration. This can be done by trial and error or by systematic break-down of a unit-graph of known storm-duration into a unit-graph for use with rainfall-data of half the original duration. With care this process can be repeated until the desired graph is obtained. An idea of how to make this break-down can be obtained by graphically combining two of the smaller duration unit-graphs (for example, say 12 hours) staggered 12 hours and getting the resultant 24-hour unit-graph by adding the ordinates and dividing by two. One can then see how to carry out the reverse process.

The procedure for deriving synthetic unit-graphs presented in this paper can be used to advantage in obtaining a trial unit-graph. Regardless of how they are obtained, all unit-graphs should be tested by application to actual storms.

Determination of the "lag"-characteristic of any drainage-area--The term "lag" as introduced by W. W. Horner and F. L. Flynt in their study of rainfall and runoff from urban areas [4] was defined as the time-difference in phase between salient features of curves of rainfall and runoff or as the time from center of mass of rainfall to center of mass of runoff. As used in this paper, the term "lag" is defined as the time between center of mass of surface-runoff-producing rain of a specified type of storm and the occurrence of resulting peak-discharge at the location being studied. It is assumed that the lag is constant for a particular drainage-basin. The above definition of lag is consistent with the use of different values of lag on the same area depending on the type of storm.

The lag for any area is obtained either through available records of rainfall and streamflow or by special field-observation for a few storms. In any case an approximate determination of lag can be made from information readily available.

The factors usually considered as affecting unit-graph shapes are area, shape of basin, topography, channel-slopes, stream-density, and channel-storage. In this study which is based mainly on records of streams located in the Appalachian Highlands, topography, channel-slopes, and stream-density have been eliminated.

Topography was experimentally expressed as different factors, none of which correlated so as to improve the determination of lag. The fact that surface-runoff ceases soon after end of rainfall and is in a channel of some sort within a short time may explain the failure, especially as the size of the area increases.

Channel-slopes likewise failed to be of much help, although provision has been made so that a coefficient can be varied when changing from one general area to another with great differences in channel-slopes.

Channel-storage is generally not obtainable from available maps, although approximate values

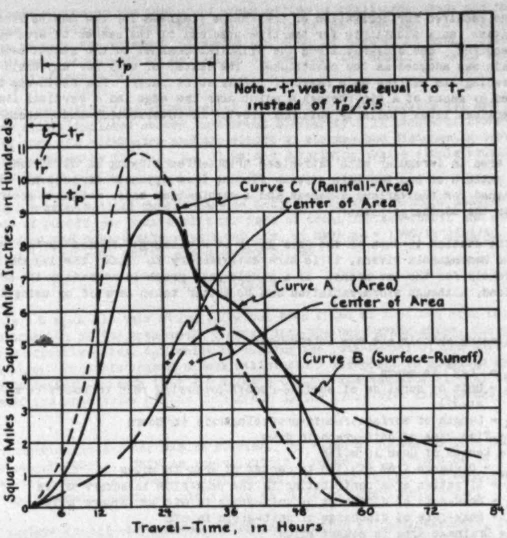

FIG. 1 - SHAPE-DIAGRAMS - FRENCH BROAD RIVER BASIN ABOVE DANDRIDGE, TENNESSEE

can be derived from stream-flow records. To date a readily obtainable factor to represent the effect of storage on lag has not been developed so its influence was also thrown into the coefficient since channel-slopes and storage are closely related. This leaves size and shape to be considered.

Width of drainage-basin is recognized as one measure of peak-flow to be expected. An additional factor to complete the shape-picture is the distance from the river-measurement station to the center of area. Let us now consider what is meant by width and shape of a drainage-basin.

One way to express the effective shape or width of a drainage-basin is to locate contours of travel-time above the point being studied. These contours can be located by hydraulic computations for steady flow using the Manning or Kutter formula. The results when plotted with time as abscissa and increments of area between time-contours as ordinates express the shape of a drainage-basin by giving the increment of the area that is at any particular travel-time distant from the station. An approximate shape may be obtained by eliminating consideration of shapes and slopes of channel and locating contours of distance from the station and then plotting drainage-area or width (increment of area divided by increment of distance from gage) against distance from the station. None of the above diagrams will show the effect of channel-storage or flood-wave travel.

Curve A of Figure 1 is an area-shape diagram for the French Broad River above Dandridge, Tennessee, based on six-hour travel-time contours and curve B is the six-hour unit-graph for the area. If the unit-graph shape for the area were to be that of curve A, the lag for a six-hour storm would be equal to the time-distance to the maximum ordinate less three hours (6/2). However, with the flood-wave action counteracted by the storage-effect that is always present in natural drainage-basins the time-lag will always be greater. In addition, the effects of flood-wave and storage are such that the time-abscissa of the center of area of curve A is a better index of the time of peak-flow than is the time-abscissa of the peak-ordinate of curve A. However, for the example of Figure 1, the time-abscissa of the center of area of curve A is also less than the time of peak-flow of the unit-graph.

The time required for derivation of area-shape diagrams for the basins studied would have been prohibitive, so a substitute for the time-abscissa of the center of area of area-shape diagrams was required. The distance along the principal channel to the actual center of area of the drainage-basin was adopted as the substitute. The center of area for any basin is readily obtained by tracing its outline on a sheet of fairly stiff paper. The basin-map is then cut out and suspended by means of a pin through a point near the edge and a vertical line determined. Two other vertical lines similarly obtained provide an intersection and a check at the center of area.

If the area is irregular with fair-sized tributaries flowing in directions at variance with the general pattern or has two main streams flowing from opposite directions, the center of area must be obtained for the various subareas and a combination made, weighted according to area and distance from the discharge-point.

Where the station is such as the Ohio River at Pittsburgh, formed by the junction of the Allegheny and Monongahela rivers, it is more satisfactory to obtain the lag and also the unit-graphs separately for the two areas. If a single unit-graph is necessary the separate graphs can be combined, although storm-variation can be better taken care of by using the graphs separately.

The following symbols are now introduced:

t_p = "Lag" in hours
t_r = Unit of duration of surface-runoff-producing rain in hours (= $t_p/5.5$ in this study)
t_R = Length of surface-runoff-producing rain in hours
T = Time-base of unit-graph in days
L = Length of area in miles
L_{ca} = Distance from station to center of area in miles
a_p = Effective area contributing to the peak-flow in square miles
q_p = Peak-rate of discharge of unit-graph in cfs per square mile
Q_p = Peak-rate of discharge of unit-graph in cfs
A = Drainage-area in square miles
C_t and C_p = coefficients depending on units and drainage-basin characteristics

The addition of an accent (') to the above symbols does not change the definition except to indicate that it refers to a hypothetical runoff-shape as expressed by an area-shape diagram. The unit of duration, t_r, was made a fixed part of t_p to satisfy the requirement of a practical relation between the unit of time used and drainage-basin characteristics.

The use of the expression "surface-runoff-producing rain", in the definitions given above is necessary to eliminate storms or parts of stormsthat cause practically no surface-runoff.

L_{ca} should be the actual distance on the river from the station to the center of area. If the center of area does not fall on the main channel, the distance should be measured along the main channel to a point opposite the center of area. L should be measured in the same manner until past the center of area after which less and less attention need be paid to minor sinuosities of the stream.

From the study of a large number of basins it was found that t_p varied with $L_{ca}^{0.6}$. The significance of the exponent 0.6 can not be definitely stated at this time as there are a number of possible explanations. Since L_{ca} is an approximation of the actual factor desired the relation is made more stable by introducing L which can be more definitely determined. Then

$$t_p = C_t(L_{ca}L)^{0.3} \tag{1}$$

The value of C_t varied from about 1.8 to 2.2 with an average of 2 for the areas studied which are mainly in the Appalachian Highlands. The coefficient C_t takes care of differences in slopes and storage and did not vary greatly over areas studied, all of which were of somewhat similar characteristics.

The value of t_p thus obtained is the lag for a storm of uniform intensity and distribution. For storms of unequal areal distribution, a rainfall-area-shape diagram can be constructed on the same basis as curve A of Figure 1, but plotting square-mile-inches as ordinates instead of square miles. Curve C, Figure 1, is the rainfall-area-shape diagram for a storm that has two inches of precipitation at the outlet and grades off uniformly to no rainfall on the remote headwater-areas.

Then the time-difference between the center of areas of the rainfall-area-shape and the area-shape diagram is the approximate correction to be applied to the time-lag. A final adjustment would still have to be made for movement of storms up, down, or across the area. This correction consists of an additional adjustment to be applied to the lag, the amount of which would depend mainly on judgment and observed data.

Determination of peak-flow (per square mile) of unit-graph--Practically all hydrographs of surface-runoff from single isolated storms have great similarity. If length of storm, amount of runoff, and size of area are eliminated and the effect of storage and distance to center of area of the basin are represented by the lag, t_p, it is not surprising that a simple relation exists between q_p, t_p, and C_p.

First, taking the hypothetical runoff-case as indicated by an area-shape diagram and since q'_p is the peak rate of runoff per square mile per inch of total surface-runoff and since one inch of runoff per hour from one square mile equals 640 cfs, we have $q'_p = (640/tr')(a'_p/A)$. Then, if C'_p equals $(t'_p/t'_r)(a'_p/A)$, $q'_p = C'_p\ 640/t'_p$ and dropping primes

$$q_p = C_p\ 640/t_p \tag{2}$$

C_p now equals 5.5 a_p/A (in this study t_p/t_r has been fixed at 5.5) but with the flood-wave and storage-transformation of the area-shape diagram into the unit-graph shape, the physical significance of the effective area, a_p, which is causing the peak-runoff of the unit-graph is practically eliminated. For this reason a more satisfactory viewpoint is obtained by introducing a drainage-basin factor, K, such that $K = a_p/a'_p = C_p/C'_p$. The quantity (1 - K) then expresses the net reduction of the peak-value of the unit-graph by the flood-wave and storage-effect of the drainage-basin.

As developed empirically for the basins studied, the value of q_p varied from $360/t_p$ to $440/t_p$ with an average value of $400/t_p$. This is equivalent to a range in C_p from 0.56 to 0.69. As a rule C_p approached its larger value when C_t approached its lower value. The equation $q_p = C_p\ 640/t_p$, being an equilateral hyperbola, plots as a 45°-line on logarithmic paper.

Duration of surface-runoff--An expression for the duration of surface-runoff, T, is necessary in constructing Figure 2 which gives values of a distribution-graph for any basin as a function of the lag. This diagram is somewhat similar to that published by Bernard [2] which was based on drainage-basin features. The empirical expression

$$T = [3 + 3\ (t_p/24)] \tag{3}$$

was adopted to define a practical value of T. Other values could be used without difficulty and the best duration depends on the method adopted for handling base-flow.

It will be noted that for the smallest area considered (about ten square miles) the value of T will be at least three days. This is the time required for the stream-discharge from a unit-storm to rise and recede to a normal ground-water depletion. Since this is a longer period than can be explained by the effect of channel-storage, it appears that the additional time required must be due to ground-storage or delay. In other words, part of the water which has been included in unit-graphs is not really surface-runoff as usually defined (storm-water flowing over the surface of the ground into open drainage-channels.)

This particular feature has provided difficulties for the author and other investigators for several years. The temporary ground-water flow or better still "subsurface storm-flow" has been defined by C. R. Hursh [5] as "that portion of the storm-flow which infiltrates into the surface soil, but moves away from the area through the upper soil-horizons at a rate much in excess of normal ground-water seepage". It should be added that as pointed out in Water-Supply Paper 772 [3, p. 113], subsurface storm-flow may reach stream-channels with a responsiveness that is only somewhat less pronounced than that which characterizes surface-runoff.

At the present stage of development the most practical procedure is to include most of the subsurface storm-flow in a unit-graph along with the true surface-runoff. This is the result obtained when the ground-water flow is separated from the flood-runoff by means of a normal ground-water depletion-curve. The subsurface storm-flow appears mainly in the recession-side of a unit-graph and necessitates a longer time base than would ordinarily seem necessary. The effect, so far as it influences unit-graph procedure, decreases in importance as size of area increases.

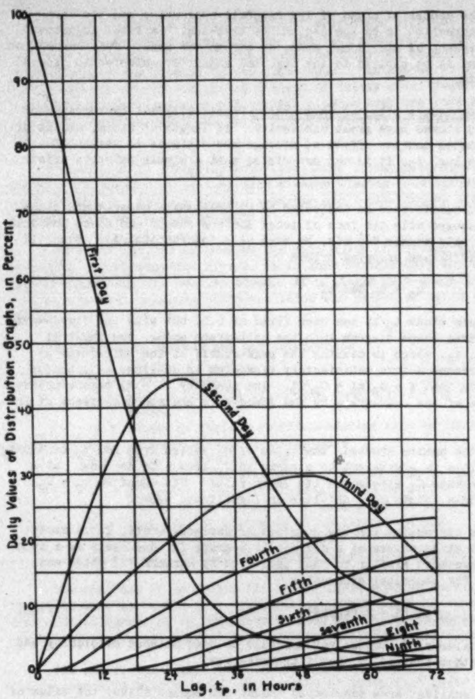

FIG. 2 - RELATION OF BASIN-LAG AND DISTRIBUTION-GRAPH

The above procedure will prove satisfactory for most ordinary storms, since it now appears that up to a certain amount of total runoff there is a fairly consistent relation between amounts of surface-runoff, subsurface storm-flow, and ground-water recharge.

Difficulties arise in very intense storms on areas as large as 3,000 square miles. In the same way that it appears probable that most ground-water aquifers have a maximum storage and discharge [6], it is also likely that there is a maximum amount of subsurface storm-water storage and flow. Thus in very large floods due to the maximum being reached or a limit set by existing infiltration-capacity, a greater percentage of the total flood-runoff occurs as true surface-runoff as compared to subsurface storm-flow than is usually the case. This gives a unit-graph with an earlier and higher peak and less volume in the recession-side than is the case in ordinary floods.

In forecasting very large floods having a flood-runoff in excess of a certain amount, the excess or true surface-runoff can be distributed by means of an additional unit-graph.

Example of procedure--There is now available the lag, t_p, which plus $t_r/2$ gives the time from beginning of surface-runoff to the peak of the unit-graph. The rate, q_p, multiplied by the area gives the peak-value of the unit-graph, Q_p, and Figure 2 gives daily values of the distribution-graph percentages.

It should be pointed out here that the distribution-graphs as represented by Figure 2 are averages for typical areas and should be used merely as guides in drawing unit-graphs. If necessary, variations should be made according to the shape-diagram of the area being studied. An example is now given for the French Broad River above Dandridge, Tennessee, whose drainage area is 4,450 square miles.

From a 1/500,000-scale map the length, L, was measured as 130 miles with a map-measurer. The value of L_{ca} was determined as 67 miles with no adjustments since the stream pattern is uniform. Then: $t_p = 2\ (67 \times 130)^{0.3} = 30.4$ hours; $q_p = 400/30.4 = 13.2$ cfs per inch per square mile; $Q_p = 4450 \times 13.2 = 58{,}700$ cfs; $t_r = 30.4/5.5 = 5.5$ hours (use nearest whole value, 6); $T = [3 + 3\ (30.4/24)] = 6.8$ days

From Figure 2 the distribution-graph percentages for a lag-value of 30.4 are 11.1, 42.6, 23.7, 12.5, 6.4, 3.0, and 0.7. Figure 3 shows the synthetic unit-graph which satisfies the above requirements and whose volume equals 120,000 sec-ft-days (1 inch on 4450 square miles).

Also on Figure 2 is a unit-graph derived from the storm of January 18 to 19, 1936. This storm consisted of four short periods of intense rainfall and lasted 18 hours. The surface-run-

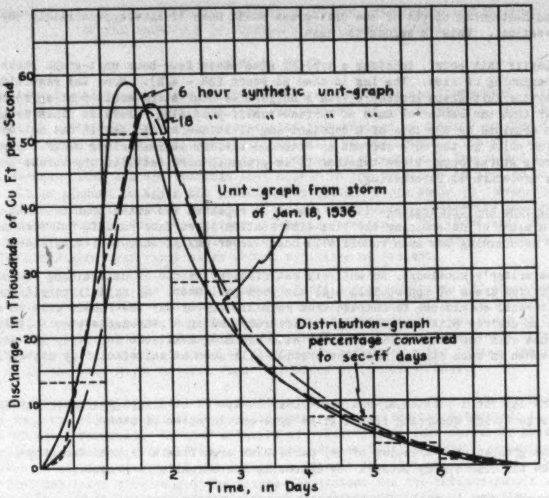

FIG. 3 - SYNTHETIC UNIT-GRAPHS — FRENCH BROAD RIVER BASIN ABOVE DANDRIDGE, TENNESSEE

off was 1.43 inches. An 18-hour synthetic unit-graph derived from the six-hour synthetic unit-graph is shown for comparison.

Use of procedure in flood-forecasting--In forecasting flood-flows and stages the flow at the point being forecast is usually known up to the time any particular forecast is to be issued, thus outlining the beginning of the rise. If the time of peaking and peak-flow are known it is usually easy to sketch in the intervening rise. Assuming a knowledge of the rainfall and expected amount of total surface-runoff, the time and value of the peak-flow can be closely approximated by knowledge of the expected lag and use of a modified equation (2) which is for one inch of surface-runoff per square mile. By expected lag is meant the lag for a specified type of storm adjusted for actual distribution of rainfall and storm-movement. On any area for which forecasts are being made the value and variation of the lag under different conditions would soon be known from observation even if original values were estimated.

The time-interval, t_r, as used in this paper is about the correct time-interval for collection of data that is necessary for satisfactory forecasting. Actual lengths of storms are not apt to be equal to t_r so equation (2) is modified to form

$$q_{pR} = C_p 640/[t_p + (t_R - t_r)/4] \qquad (2a)$$

where the variables and coefficients have the same general meaning as before except that the addition of the subscript "R" indicates the length of rain is other than t_r.

Based on the idea of time-contours as expressed by the area-shape curve of Figure 1, expression (2a) would read $q_{pR} = (c_{pR}640/t_p)$ where $C_{pR} = 5.5\ a_{pR}/A$ and a_{pR} equals the area contributing to the peak-flow and corresponding to the actual storm-duration. However, due to the lack of sufficient area-shape diagrams it was thought best to vary the denominator of the expression rather than the numerator to take care of various duration storms. Equation (2a) is an attempt to do this.

According to the original assumptions, the time of peaking will still be distant from the center of mass of surface-runoff-producing rain by an amount equal to the lag. This assumption differs slightly from the unit-graph procedure. For it to hold in the unit-graph method, the ascending and descending slopes of the unit-graph would have to make approximately the same angle with the vertical. This is seldom the case.

To clarify this point, consider a typical area whose four-hour unit-graph peaks in 26 hours from the beginning of rise. The lag is then 24 hours (26 - 4/2). Now, the resulting hydrograph obtained by the unit-graph procedure from a uniform 24-hour storm would peak approximately 27 hours later than the center of mass of surface-runoff-producing rainfall. This is three hours later than obtained by the idea of a constant lag of 24 hours. So far it has not been possible to determine which is the more correct as extended uniform storms seldom occur. When two or more separate storms occur close together it is probably more satisfactory to use the regular unit-graph procedure in forecasting.

Limitations and conclusions--(1) It cannot be repeated too often that the unit-graph procedure is a means of determining the time-distribution of surface-runoff, but does not solve the problem of determining how much runoff will occur under any given set of conditions.

To the writer's knowledge, no entirely satisfactory method of determining the amount of surface-runoff from areas of appreciable size has been published. By satisfactory is meant a procedure that would enable one to compute from rainfall day by day the runoff from an area for as many years as desired with a high degree of accuracy and in a reasonable time. It is believed, however, that with the increase in accurate data becoming available for study and with the interest being shown in such studies that the problem will soon be satisfactorily solved and demonstrated.

(2) The lag for a particular drainage-basin is apt to be slightly greater for small floods than for large floods occurring from a given type and duration of storm.

(3) The greater the variation of any particular area from a typical fan-shape, the greater is apt to be the discrepancy between the synthetic and actual unit-graphs.

(4) Extreme care must be used in applying unit-graphs satisfactory for ordinary storms of less than two inches surface-runoff to storms having three or more inches surface-runoff.

(5) The equations and coefficients given are based mainly on the fairly mountainous Appalachian Highlands and may need considerable adjustment to take care of streams in the relatively flat middle west.

(6) It is believed that the procedure presented in this paper will be of material assistance to others as it has been to the author in studies of flood-control, flood-routing [7], and flood-forecasting. The unit-graphs on which the results are based are not presented herein as it is felt that anyone planning to use a unit-graph for any particular area should himself work up the various storms available and become acquainted with the limitations and difficulties of the particular area involved.

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MODIFICATION OF THE INDEX-AREA PRINCIPLE AND THE ANTICIPATED APPLICATION OF THE PRINCIPLE TO MUSKINGUM RIVER FLOOD-CONTROL

Waldo E. Smith

Introduction--The basic principle of the index-area was first set forth by H. R. Leach, H. L. Cook, and Robert E. Horton in 1933 [see 1 of "References" at end of paper]. The fact that the index-area idea as applied in that case gave results that checked as closely as they did seemed to establish that there must be come fundamental validity in the method. The work of the Muskingum Watershed Conservancy District has afforded opportunity to work with the method and to attempt to eliminate certain flaws and shortcomings that appear in the principle as applied in the paper referred to. It is planned to apply the principle on quite a large scale in forecasting the volume of flood-water that will need to be handled in the flood-control reservoirs and the channels of the Muskingum Watershed. Some expeditious scheme is essential. The index-area method combined with the distribution-graph as evolved by L. K. Sherman, Merrill Bernard, and others [2] seems to offer advantages over every other method available at present.

Review of previous work--The principle as set forth and demonstrated [1] showed principally that a close relationship exists between the volume-coefficient [3] of runoff for Ralston Creek, a watershed of three square miles, and the contiguous watershed of the Iowa River at Iowa City, a watershed of 3230 square miles. Inasmuch as the runoff from the small watershed occurred very soon after the rain fell, it was shown to be possible to determine quite closely the resultant volume of flow in the Iowa River several days in advance of its occurrence.

Modifications in principle--The chief modification as evolved for work on the Muskingum Watershed is that the volume-coefficient-relationship curve for the two watersheds is developed on the basis of equal rainfall. Further a family of volume-coefficient curves is developed for the index-area to accommodate differences in rainfall between the two areas. Another difference is that instead of working with total storm-flow an attempt in all cases is made to separate all ground-flow from the total flow to get the volume-coefficient for the surface-runoff alone. This then permits the use of the distribution-graph for determining the flow at any time.

Establishment of the index-area--Home Creek Watershed near New Philadelphia, which is the index-area, lies south of the region of the continental ice-sheet, and while the bottom lands are alluvial materials from the hill sides, these are a small part of the total area, as the map (Fig. 1) of the area shows. The soil of the hills is materials in situ from the decomposition of shales and sandstones chiefly which underlie the soil at shallow depth. There are nearly 300 feet of relief. The land is used chiefly for farming operations, with about 26 per cent in cultivated fields, 51 per cent in meadow, pasture and orchard, 19 per cent in woodland, and four per cent in mining operations, farm-yards and miscellaneous. Drainage from a number of coal mines contributes to the flow from the watershed. This is noticeable chiefly in times of low flow. In the fall of 1936, the U. S. Geological Survey cooperated with the Conservancy District in the construction of the recording gaging-station consisting of a seven-foot gage-house, a Columbus deep-notch control weir, and a foot-bridge for taking measurements in times of high flow, on the grounds of the Tuscarawas County Infirmary. The drainage-area is 1.64 square miles. The Soil Conservation Service through its Section of Watershed and Hydrologic Studies supplied the four standard eight-inch cylindrical rain-gages that have been in use since the gaging station was established. In October 1937, the Section of Climatic and Physiographic Factors of Erosion of the Soil Conservation Service through its Muskingum Climatic Research Center supplied four recording rain-gages which furnish much needed information on the details of the precipitation.

Basic work--Work is in progress to relate this index-area to as many watersheds of the Muskingum drainage-system and increments to watersheds as possible. Most of these are neither inclusive of nor adjacent to the index-area (see Fig. 2). The area was selected because of its proximity to headquarters for effective operation, the resemblance in topography and cover that it bore to the watershed as a whole, and other factors. It is very unlike many of the sub-watersheds with which it is hoped to establish relationship. By using the modifications of the method outlined, it is believed that physical and geologic similarities are much less important, and that proximity is important only to the end that the index-area and the watershed to which it is correlated will receive the same storms, and that they will be of like duration.

Volume-coefficients of runoff were determined for all of the periods available for which runoff occurred, including light, moderate, and heavy rains for both brief intervals and longer ones. Most of the periods in the available data are for a day or less and they represent a fairly complete range of conditions of soil and vegetation. One of the surprising things found was the rapid increase in ground- or base-flow that occurred which may be due, it has been suggested, to

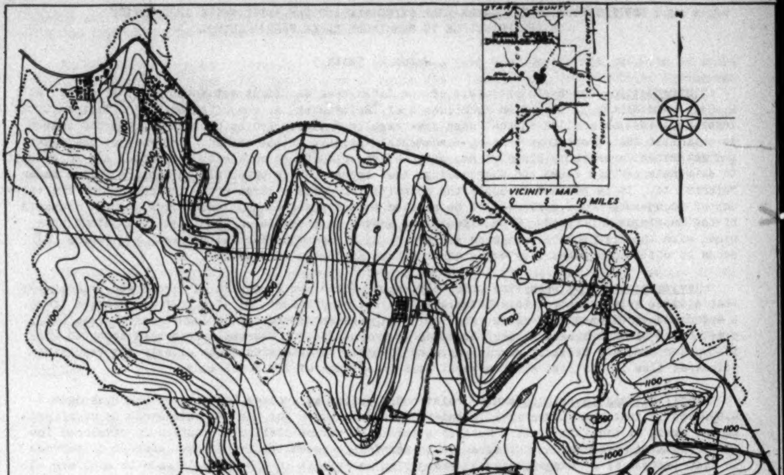

VICINITY MAP
0 5 10 MILES
COUNTY INFIRMERY
RECORDING STREAM-GAGE
ZERO-GAGE = 872.49
DRAINAGE-AREA = 1049.634
LEGEND
RAIN-GAGE ●
INTERMITTENT STREAM
FIELD LINE
SCALE OF FEET
0 400 800
1100
1000
900
1075
Creek

FIG. 1—TOPOGRAPHIC MAP, HOME CREEK DRAINAGE-AREA, TUSCARAWAS COUNTY, GOSHEN TOWNSHIP, T8N-R1W, U.S.M.S., 2ND & 3RD QRS. (CONTOUR-INTERVAL 20 FEET)

the shallow soil on the hills and to a high water-table (as indicated by swampy areas) in the valleys. No one of the storms investigated failed to show increase in ground-flow.

The values were all plotted, using volume-coefficient as ordinate and amount of rainfall as abscissa. The two outer curves (Fig. 3) were then drawn as envelope-curves. Because of the fact that neither maximum nor minimum runoff-conditions probably occurred within the brief period of record, rather generous allowance was made. The other curves were drawn to conform to the general pattern indicated by the first two curves. These then are the family of curves for the determination of the volume-coefficient of runoff for precipitation in amount other than the amount that fell on the index-area, that is, for the amounts of the same storm on the correlated watersheds.

While the conditions represented by intermediate curves are relatively unimportant--at least at the outset--their shape is important. They may probably be best corrected and interpreted

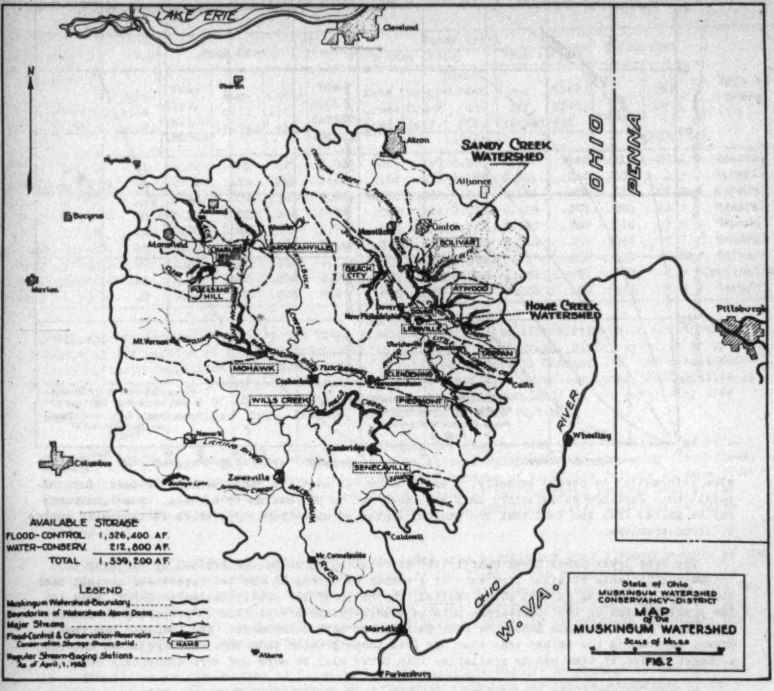
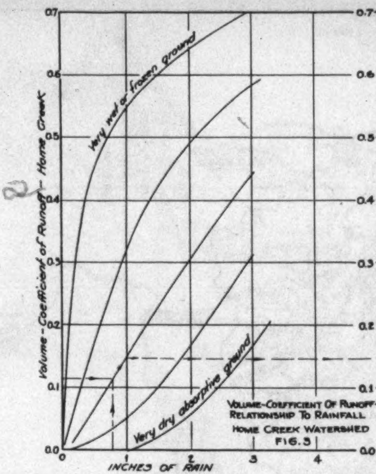

LAKE ERIE
Cleveland
N
Oberlin
Akron
SANDY CREEK WATERSHED
Alliance
OHIO
PENNA
Plymouth
Bucyrus
Ashland
Wooster
Massillon
Canton
Mansfield
CHARLES MILL
MOHICANVILLE
BOLIVAR
BEACH CITY
Marion
PLEASANT HILL
ATWOOD
Dover
New Philadelphia
HOME CREEK WATERSHED
Pittsburgh
LEESVILLE
Mt. Vernon
Uhrichsville
Dennison
TAPPAN
MOHAWK
Coshocton
CLENDENING
Cadiz
WILLS CREEK
PIEDMONT
RIVER
Wheeling
Newark
LICKING RIVER
Cambridge
Columbus
SENECAVILLE
Zanesville
MUSKINGUM
Caldwell
Mc Connelsville
RIVER
OHIO
W. VA.
Marietta
Parkersburg
AVAILABLE STORAGE:
FLOOD-CONTROL 1,326,400 AF.
WATER-CONSERV. 212,800 AF.
TOTAL 1,539,200 AF.
LEGEND
Muskingum Watershed-Boundary
Boundaries of Watersheds Above Dams
Major Streams
Flood-Control & Conservation-Reservoirs
Conservation Storage Shown Solid.
NAME
Regular Stream-Gaging Stations As of April, 1, 1938
State of Ohio
MUSKINGUM WATERSHED CONSERVANCY-DISTRICT
MAP of the MUSKINGUM WATERSHED
Scale of Miles

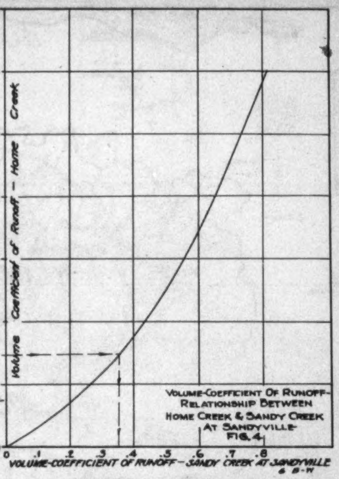
FIG. 2

through use. Since the preparation of the curves shown, further use chiefly in connection with the storms of March 13 to 16, 1938, has indicated that all except possibly the uppermost one should turn more sharply to the right especially when the rainfall is in excess of two inches. Lack of sufficient points based on high precipitation has made the definition of the right-hand part of the curves uncertain. It may ultimately be desirable to work up a family of curves for each season and to interpret intermediate conditions. Present data are insufficient for this purpose, however.

The results of these studies to the present as they relate to Sandy Creek Watershed above Sandy Creek Watershed above Sandyville on the northeastern headwaters of the Watershed (see Fig. 2) demonstrate the technique and tend to establish the validity of the index-area principle as applied. The area of Sandy Creek Watershed above Sandyville is 481 square miles. The northern portion of this Watershed is in the glaciated area, the southern part in the outwash region. There is a recording gage on this stream established by the United States Geological Survey and the State of Ohio prior to the organization of the Conservancy District. The rating curve is quite satisfactory and the data used on stream-flow approached the optimum in validity. Since July 1, 1937, the records of the Muskingum Climatic Research Center have furnished much more adequate precipitation-data than were formerly available. For the general storms in winter, a small number of stations will suffice for the satisfactory determination of the average precipitation for quite a large area. Storm-periods of the showery type, especially common in summer and of the order of the storm of March 15 to 16 later referred to, require many stations to give a satisfactory average, and there are instances in which the four-mile spacing of the gages of the Muskingum Climatic Research Center appears to be too great. In flood-control operation,



VOLUME-COEFFICIENT OF RUNOFF-RELATIONSHIP TO RAINFALL HOME CREEK WATERSHED FIG. 3



VOLUME-COEFFICIENT OF RUNOFF-RELATIONSHIP BETWEEN HOME CREEK & SANDY CREEK AT SANDYVILLE FIG. 4

when information is needed promptly, a small number of stations must furnish the basis for computations. Just how satisfactory this will work out to be remains to be seen. Some assurance may be gained from the fact that the general winter or warm-front-type storm is the chief source of flood-problems.

The Home Creek-Sandy Creek coefficient-correlation curve was determined by plotting the volume-coefficients obtained from each of a number of storms on the two watersheds against each other, and indicating at each point plotted, the ratio of the precipitation on Sandy Creek to the precipitation on the index-area--much as stadia-points are plotted in preparing a contour-map. Each storm used had about the same duration on both watersheds. The isopleth was then drawn, separating the values less than one from those greater than one. As correlated data for a longer period of time become available, this curve will be more and more accurately defined.

Example of use--Take, for illustration, the rain that fell on March 5, 1938. The average precipitation on Home Creek Watershed was 0.78 inch between 9 in the morning and 8 in the evening. The computed runoff above base-flow for Home Creek gave a volume-coefficient of runoff of 0.115. This happened to fall on the middle curve of the family (Fig. 3).

A same storm-period resulted in a total of 0.98 inch of rain on Sandy Creek Watershed. Had the precipitation been 0.98 inch on Home Creek, the trend of the family of curves indicates that the runoff-coefficient would have been 0.146 (follow the dashed line). To obtain the coefficient for Sandy Creek enter the coefficient-correlation curve (Fig. 4, follow the dashed line) and it is found that the corresponding volume-coefficient for Sandy Creek is 0.350, resulting in a runoff of 0.343 inch. The distribution-graph for Sandy Creek could then have been applied to this volume of flow which, added to the base- or ground-flow, would have given the hydrograph. Because the chief concern of this work at this time has been the testing of the index-area principle, this was not actually done. The record for Sandy Creek indicates an actual runoff of 0.353 inch, or 0.010 more than computed by the index-area method. This checks out more closely than can be expected regularly as Table 1 shows. Application of the method to the smaller storms, the runoff from which is often less than the base-flow, offers opportunity for a large percentage of error. It is to be expected, also, that practice will improve the technique as well as the curves.

The precipitation-data on Home Creek Watershed are available within an hour of the cessation of rainfall, and the runoff-record is complete in about seven hours additional. Computations make the volume-coefficient available in about an hour. To gain the seven hours needed to complete the runoff-record for the index-area, the distribution-graph method may be applied. The pluvia-

Table 1--Estimated and actual runoff from Sandy Creek at Sandyville (estimate based on index-area method)

Rainfall			Home Creek			Sandy Creek						Difference		Type of storm
						From Home Creek				From record				
Date														
From	To	Duration hours	Precipitation inches	Runoff, inches	Coefficient	Precipitation inches	Home Creek coef.	Sandy Creek coef.	Sandy Creek runoff, inches	Runoff, inches	Coefficient	In inches	In per cent of runoff	
12/31/37	1/1/38	21	0.32	.020	.063	0.35	.065	.190	0.067	0.086	.246	-.019	22.1	General
1/30/38	1/30/38	15	0.74	.189	.255	0.44	.154	.370	0.163	0.161	.366	.002	1.3	General
2/6/38	2/6/38	10	0.44	.073	.166	0.68	.235	.482	0.328	0.264	.388	.064	24.2	General
2/18/38	2/20/38	45	0.53	.022	.042	0.64	.050	.154	0.099	0.129	.293	-.030	23.3	General
3/5/38	3/5/38	11	0.78	.090	.115	0.98	.148	.350	0.343	0.353	.360	-.010	2.9	General
3/13/38	3/14/38	36	1.58	.366	.232	1.59	.235	.480	0.763	0.715	.450	.048	6.7	General
3/15/38	3/16/38	27	1.55	.555	.358	1.17	.300	.577	0.652	0.752	.643	-.100	13.3	Showers
3/13/38	3/16/38	120	3.13	.945	.302	2.76	.247	.498	1.375	1.467	.531	-.092	6.3	Combination
3/23/38	3/23/38	20	0.52	.039	.075	0.68	.098	.260	0.177	0.135	.199	.045	33.3	General

graph can be produced as soon as the total precipitation and its time-distribution are known. Comparing the portion of the hydrograph available with the pluviagraph, either directly or by plotting their respective summations of flow as outlined by Merrill Bernard [4], the volume-coefficient of runoff can probably be determined within a short time after the total precipitation is known, or that portion of the total with which one will concern himself at any particular time during a protracted storm. This, however, was not done in this instance.

Snow-cover--It is characteristic of this region for snow to melt under conditions of rainy weather, and at other times that it melts, it is relatively unimportant in producing flood-flows. No great accumulation usually occurs during the winter because of alternate periods of snow-cover and bare ground. When the snow melts under the influence of falling rain, records kept of the snow accumulation supply the necessary information for the additional water that must be taken into account.

Comment on other methods--This work on the index-area studies has gone forward because of the advantages that this procedure has over any other in the light of the expedition required. It has been realized full well the disrepute with which many engineers and hydrologists now hold the volume-coefficient method of approach. Especially is the disrepute great among some of the proponents of the infiltration-capacity method of estimating runoff and agronomists and others who work in terms of field moisture-deficiency.

The office of the Studies of Climatic and Physiographic Factors of Erosion of the Soil Conservation Service has under development an interesting technique for the determination of surface-runoff involving field moisture-deficiency, saturation-deficiency, infiltration-rate, time-contours, or isochrones and valley-storage, which the author has been permitted to review. It is possible that this may develop into a useful analytical method for situations offering adequate precipitation and other basic data, but is doubtful if the required expedition can ever be attained to be of use on flood-forecast and work of operation.

It has been said that the volume-coefficient is an abstraction and thus can not be estimated rationally while infiltration-capacity is physical and concrete, permitting judgment in selection. In defense of the coefficient-method I would like to point out that volume-coefficient is inches per inch, and is abstract only because numerator and denominator are of the same units. The unit inches per inch in reality is no less concrete than inches per hour, which is used in infiltration-capacity studies.

It should be noted here that on Home Creek Watershed it has been found that runoff occurs before the intensity of precipitation has reached infiltration-capacity as determined under conditions of considerable rainfall-excess. In other words, as intensity increases, infiltration-rate increases, runoff occurring at both intensities. This has been observed in other studies also and has been explained that the ground becomes more absorptive as it becomes moist or that infiltration-capacity increases somewhat with intensity because of lack of coverage of the drops of rain under lower intensities. It appears that the change in some cases is likely due to lack of uniformity of infiltration-capacity or rate over the area. For example, assume it has been found that the infiltration-capacity is 0.25 inch per hour for a given area under given conditions when there is considerable rainfall-excess. A rain occurs when the area is under the given condition, of intensity of 0.25 inch per hour. There will be runoff from the watershed. That is because the infiltration-capacity of 0.25 is an average only. If the area is half swamp or water-surface having zero infiltration-capacity and half having an infiltration-capacity of 0.50, the resulting runoff would be substantial. The ratios given will hardly hold, but they serve to illustrate the point. Care should always be taken to avoid confusing infiltration-capacity, field moisture-deficiency, and other terms relating to soil or soil-condition on a plot which is quite homogeneous, or at a point, with the same items for a drainage-area which is usually a complex composite of many soil-conditions and aspects.

It is realized full well the advantages of the infiltration-capacity and field-moisture-deficiency methods of approach and likewise their deficiencies, one of which latter is lack of expedition, and another, lack of present development. Possibly as these and other difficulties are overcome, advantages will develop in favor of the infiltration-capacity idea for such uses as are outlined here.

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[2b] Merrill M. Bernard, Approach to determinate stream-flow, discussion by C. S. Jarvis, L. K. Sherman, W. W. Horner, C. H. Eiffert, Charles S. Bennett, R. L. Gregory, C. E. Arnold, Gordon R. Williams, W. G. Hoyt, L. L. Harrold and F. F. Snyder, and Merrill M. Bernard, Trans. Amer. Soc. Civ. Eng., v. 100, pp. 346-395, 1935.

[2c] Engineering procedure as applied to flood-control by reservoirs with reference to the Muskingum Flood-Control Project, App. 4 on Application of the unit-graph method for the determination of stream-flow from rainfall-records, Eng. School, Fort Belvoir, Virginia, 1936.

[2d] W. G. Hoyt and others, Rainfall and runoff in the United States, Water-Supply Paper 772, 1936.

[3] The term "volume-coefficient" is used intentionally to avoid the confusion with "rate-coefficient."

[4] Merrill M. Bernard, A preliminary manual for the unit-hydrograph method (in manuscript), August, 1934.

Muskingum Watershed Conservancy District,
New Philadelphia, Ohio

PROBABILITY-DISTRIBUTION-CURVE FOR FLOOD-CONTROL STUDIES

Bradford F. Kimball

[Study made by the staff of Division of State Planning of New York State. It was instigated by the suggestion of Edwin S. Cullings, Water Resources Consultant for the Division, that the author review current methods of setting up probability distribution-curves applicable to flood- and precipitation-data.]

I--Introduction

Since 1896 attempts have been made to apply the statistical method to the occurrence of hydrological phenomena with varying degrees of success. In the "Report of the State Engineer and Surveyor of New York" for 1896, in an article on Hudson River Storage by George W. Rafter (p. 803), dated February 1, 1897, an attempt was made to apply the ordinary Gaussian law of error to rainfall-data. It was frankly recognized that the law was imperfect in its application to this type of data because positive and negative errors of equal absolute value were not equally probable, nor in a long series equally frequent (p. 843). At the conclusion of the article, Mr. Rafter stated: "In some future report it is intended to discuss, as the final theoretical question, the method of deducing a formula enabling one to take the rainfall- and temperature-records of a long series of years and deduce therefrom the probable runoffs of a stream..."

In this connection, Robert E. Horton stated [see 1 of "References" at end of paper, p. 664] that: "In 1896 the writer had prepared, at Mr. Rafter's request (by letter dated October 25, 1896), an analysis of the rainfall and runoff of the Upper Hudson Drainage-Basin, in which probable average interval of recurrence of runoff less than the observed minimum was determined by the theory of probability. This and similar phenomena were analyzed by the ordinary method of probability, through the use of the so-called Gaussian law of error."

Mr. Horton further states: "It was Mr. Rafter's desire that the writer should prepare the data for a paper on the application of the theory of probability to flood-flows and other hydrological data, and the matter was discussed frequently and fully during the years 1896 to 1904. It appeared at that time, however, that stream-flow data were so meagre as to make it desirable to defer the completion of the studies.

"It was recognized at that time that the Gaussian Law did not represent accurately the law of frequency of occurrence of many hydrological events, and an effort was made first to find a better general law [$y = (x - b)^2(x - a)^2$] was suggested by Mansfield Merriam, August, 1897) and to deduce individual laws. For the latter, the method of plotting flood-records on logarithmic paper was tried, and proved so promising that, between 1900 and 1908, many logarithmic probability-curves were plotted by the writer and corresponding formulas deduced, involving time as an element; and the methods and results have been applied extensively in professional practice by the writer for a period of several years."

At about this time similar methods were being used to plot flood-records by Weston E. Fuller and Allan Hazen [preface 2]. They designed logarithmic probability-graph paper for this purpose, on which plotted points will lie along a straight line if the logarithms of the observational data obey the normal probability-law. In 1914 a paper by Fuller on Flood-flows received wide attention. This paper and later ones have been reviewed in a study by Thorndike Saville [4, p. 398 and following]. It includes a method developed by Horton in 1924 [4, p. 436 and following].

Although many other papers have been written by engineers on the determination of the maximum storm or flood to be expected in a given area, the above papers (including those reviewed by Saville) appear to have been the chief ones which have been directly concerned with the study of the frequency-period of large storms or floods by probability-methods. It is this question of the expected period of recurrence which lends itself to treatment by the theory of probability.

Of late, it has been pointed out by hydrologists that a more detailed knowledge of the local hydrological characteristics of a watershed is essential for any reliable forecast of floods. Accordingly, at the present time emphasis is placed upon the determination of the unit-hydrograph, and the study of the characteristics of infiltration and runoff of the particular basin under consideration. The studies of the Miami Conservancy District, the charts of the late D. L. Yarnell, and similar studies, are relied upon to give indication of the probable frequency of occurrence of storms of a given magnitude. However, such papers as that of C. S. Jarvis [5, p. 379] and that of A. J. Schafmayer and B. E. Grant [6, p. 225], with their discussions, indicate that a more accurate determination of the frequency of precipitation of a given magnitude is possible by applying the methods of probability-analysis to existing flood-records. (For instance, the discussion of Schafmayer's paper by Eugene L. Grant [6] points out a surprising similarity in the statistical characteristics of the ten selected precipitation-records studied by Schafmayer. This suggests the possibility of analyzing similar records by groups.)

The writer believes that modern statistical methods are adequate for the handling of this problem, but that the question as to what statistical procedure is inherently the most proper one to apply, has not yet been satisfactorily answered. As was pointed out in Mr. Rafter's Report of February 1, 1897, the simple Gaussian Law of Error is not adequate. One of the chief reasons for this is that hydrological data concerning storms and floods are not symmetrical about the mean nor do they have the characteristics of an infinite spread.

Foster got around this difficulty by adapting the Pearsonian curves of Type I and Type III. The chief criticism of his method seems to be:

(1) The unreliability of the third and fourth moments as computed from the relatively short series that have to be dealt with in hydrological studies. This has been brought out by several writers [see, for example, 7; p. 421 and following].

(2) The use of empirical factors in an attempt at the correction of the coefficient of skew [8, p. 202].

(3) In determining β_2 which depends upon the fourth moment in the case of the curve of Type I, again an empirical "average" relationship is adopted between β_2 and β_1 [8, p. 197].

(4) The curve of Type III does not have a finite upper limit.

(5) Because of the uncertainty of the third moment and the two empirical approximations in-

volved in the determination of the best fitting curve of Type I, it does not seem to the writer that the two evolved finite limits are reliable enough to use in a painstaking study of the hydrological meaning of such limits.

Other methods not dependent upon recognized principles of the theory of probability have been developed, such as those of Goodrich, Hazen, and Horton. This seemed to be the status of the problem in 1930. Although these curves in many cases furnish a reasonably good fit, the writer believes that a great advantage is obtained in basing a description of the behavior of the phenomena upon a sound probability-basis. When this is done for a large number of records, considerable variations from one curve to another should be traceable to differences in the hydrological conditions obtaining in the different areas.

In order to do this without employing the third and fourth moments, one is led to some such method as that proposed by Slade in 1934 [see 4 or 9, p. 35 and following]. Slade dispenses with the third and fourth momemts by assuming at the start that there is an upper and lower limit, and transforms the series of observed data with finite "spread" into a normal series with an infinite "spread". This method has been called in general terms the method of the "transformed variate". It dates back to the Danish astronomer and actuary, Thiele, who proposed it in 1889, as Arne Fisher has pointed out [10] in discussion of Slade's paper [9, pp. 73 and 80]. It is a method of recognized standing in the statistical world, being specifically recommended for use in certain cases by the Dutch mathematicians Kapteyn and Van Uven, by the well-known statistician Arne Fisher, and by Professor R. A. Fisher, successor to the late Karl Pearson as one of the directors of the Galton Laboratory, University College, London. Indeed, in the determination of the probable error of a correlation-coefficient (an example of a variable which is confined between the limits -1 and +1), Fisher uses this method rather than a Pearsonian curve of Type I [10, pp. 175-181].

These two methods--that of the Pearsonian curve of Type I and that of the transformed variate--are particularly adapted to handling the frequency-function of a variable that swings between finite upper and lower limits. The logical bases upon which the two methods rest are as follows. The Pearson curve of Type I assumes a change in the normal probability law--as expressed by the basic differential equation [12, pp. 181-183]--such that the frequency-curve derived from this differential equation will fit the observed data. It is the opinion of the writer that the required change in the fundamental probability-law--in order that the resulting frequency-curve shall fit the schedule of observed data--is not one that can be supported on the basis of a logical analysis of the conditions which control the behavior of storm- and flood-frequencies as measured by amount of precipitation or amount of flood-flow in a unit-time.

The method of the transformed variate leaves the normal probability-law alone, and seeks to find whether or not there is some index (or function) of the precipitation or flood-flow that does behave like the normal law. One would not expect the normal law to apply directly to precipitation-data, for this would imply that the probability of an inch of rain in a day would be the same whether that were the first inch of rain, the second inch, or the third inch of rain which had fallen on that day. It is pretty obvious that after two inches of rain, the probability of a third inch is much less than that of having one inch when there has been no rain previously. Since the early nineteen hundreds, engineers have noticed that semi-logarithmic, and logarithmic probability, graph-paper is peculiarly adapted to the plotting of the integral frequency-curves describing flood-flows. Robert E. Horton has criticized some of the present methods of dealing with flood- and precipitation-frequencies because the exponential character of the phenomena as regards probability of occurrence was not taken into account. The two observations just noted, point to the same thing, namely, that the "index" of rainfall or flood-flow that one might expect to be normally distributed would be an exponential index. When this observation is supplemented by the argument that there exists an upper limit, and in most cases a lower limit greater than zero, the bending of the integral frequency-curves near their extremities, when plotted on log probability-paper, is also accounted for, as well as the straightness of the curve between these two points of curvature.

Proceeding from these principles, the setting up of the appropriate integral frequency-curve is a matter of mathematical and statistical technique. The general linear transformation

$$z = (b + x)/(g - x)$$

is one of the simplest ways of transforming the finite interval $-b < x < g$, into the infinite interval $0 < x < \infty$ with the least possible distortion. The transformed variate z is then given an exponential "index" u by setting up the relation

$$z = e^{ku}$$

where e is the basis of the natural logarithmic system and k is a constant to be determined later. As x varies from its lower limit -b to its upper limit +g, u will move on the interval from $-\infty$ to $+\infty$. Thus u is in the nature of an exponential index of x, which has the value $-\infty$ when x is at its lower limit, and the value $+\infty$ when x is at its upper limit. It is this exponential index of precipitation or flood-flow to which one might expect the normal law of probability to apply.

When u is expressed directly in terms of x, the above relations give

$$u = c \log[(b + x)/(g - x)]$$

where $c = 1/k$. This is the second transforming function that Slade introduced in his treatment of the problem [9, p. 57, formula 45a], and it was this function that was used in Saville's Review of Slade's method [4, p. 412].

Regarding now the values of u, determined by the above law, from the rainfall- or precipitation-data x, as "indexes" of the hydrological data tabulated, one expects in general to find that these values of u will obey the normal probability-law. Thus the integral frequency-curve is determined by the use of the mean value and standard deviation of the tabulated values of u. Slade ran into difficulties in determining the integral frequency-curve because he attempted to use the mean value and standard deviation of the x rather than of u. This made it necessary for him to introduce empirical assumptions in the determination of the constant c which it is difficult to justify [9, p.92]. As Arne Fisher has pointed out, Slade's constant p is extraneous [9, p. 80]. When the values of u are used to determine the mean value and standard deviation for purposes of setting up the integral frequency-curve, no difficulties are encountered.

II--Modified Slade method developed by the writer

Since the method developed by the writer is in some respects a modification of that suggested by Slade, this might be termed the "Modified Slade method." The writer has used assumptions of Slade that there is some upper and lower limit imposed because of the intrinsic nature of hydrological conditions. The writer has used the mean value and the standard deviation for purposes of fitting the integral frequency-curve as explained above. Besides getting around the technical difficulties which Slade encountered in setting up the integral frequency-curve, the writer has introduced two tests for the determination of the best fitting upper and lower limits. These tests are statistical criterions as to the upper and lower limits which will give the best fitting curve relative to the schedule of observed data, assuming always that the transforming relation is of the form $u = c \log[(b + x)/(g - x)]$. One of these tests the writer developed himself, and the other was adapted from a paper by the Dutch mathematician Van Uven [12]. The writer believes that the advantages of his method are:

(1) It rests upon a sounder basis both as to probability-theory, and as to the physics of hydrology.

(2) The type of probability-distribution used is that involving a finite upper and lower bound (this property is also possessed by the Foster curve of Type I, Horton's curve, and that of Slade).

(3) Two criterions have been set up for the best determination of these upper and lower limits.

It would be of interest to determine the best upper and lower limits for many records. It should be possible to determine the hydrological meaning of such limits.

Another point of interest is that the hydrological principles upon which the modified Slade method rests, suggest that the constant k in the relation $z = e^{ku}$ might be expected to run about the same in hydrological phenomena of the same type. The limits -b and g would be expected to vary more from one locality to another, but this constant k should indicate a basic relationship between the z-measure of precipitation (which is in a sense "proportional" to the ordinary measure) and the exponential index u (which is the measure subject to the normal probability-law).

The method developed by the writer has been applied in the conventional way to annual flood-peaks of the rivers of New York State along with those of the Tennessee and Kanawha rivers. The record of precipitation at New Bedford, Massachusetts, in connection with annual maximum two-day and six-day storms was also analyzed, although a correction-factor of 12 1/2 per cent had to be introduced in the case of early gage-records (see Table 2).

Although more recent findings of hydrologists indicate that such analysis should be applied particularly to precipitation-records rather than to flood-records, the work done at the Division of State Planning serves to illustrate the method. There is no reason why the same procedure should not be applied to precipitation-records and to groups of selected precipitation-records, provided the precipitation-records grouped are of a somewhat similar character and refer to the same period of time [see 6, Eugene L. Grant's discussion of Schafmayer's paper].

The writer does not claim that the method presented below is by any means the last word in applying by theoretical means a probability-distribution curve to a flood- or precipitation-record. It will be found by some to be too cumbersome. It has been suggested by Arne Fisher that it might be possible to introduce a weighting process, taking into account the larger probable errors of the more unexpected observational data.

The criticism that the method is cumbersome is valid if it is necessary to make a quick estimate with very little labor. However, in getting at the way in which probability-law intrinsically applies to precipitation-phenomena, it seems as if the emphasis should be placed upon sound procedure and accuracy of detail until the underlying principles have been thoroughly tested and established.

It should be clearly understood that the explanation of the application of the method as presented in this paper applies particularly to any series of precipitation- or flood-data, where each term of the observational series relates to a given time-period of constant duration, and these time-periods have equal weight for each term of the series. Thus the explanation applies to such series of data as: Mean annual precipitation; mean monthly or daily precipitation (provided the same month or day each year is used, to eliminate the seasonal factor); maximum two-day precipitation per year; etc. The explanation of the application of the method does not apply to data such as given in Table 4 (a) of Schafmayer's paper [6] where the number of storms over a 33-year period with a rainfall of over 0.35 inch in five minutes is given. In this case, the main principles of the method still apply--an upper and lower limit should be used, and the logarithmic transformation $u = c \log [(b + x)/(g - x)]$. However, the matter of determining the best procedure for fitting the curve will have to receive further consideration. The writer has a feeling that all storms over the entire period which can be properly classed as storms, local or otherwise, should be taken into account in order to obtain reliable results. Furthermore it might be desirable to segregate storms of different types into separate series.

It is my hope, in presenting this paper, that some agency will find it possible to test this method further, and if found to be sound and productive of meaningful hydrological principles, one will then be in a position to abstract and introduce short-cut approximations.

III--Mathematical theory of modified Slade method

Slade followed the lead of Hazen and others in using a logarithmic function to transform the raw data into a series which would approximately satisfy the laws of the ordinary probability-distribution. In fitting this logarithmic function to the data-series in the case where both lower and upper limits were used [see section IV of Slade's article in the Proceedings of October, 1934], Slade attempted to make the mean value and standard deviation of the x's of the original data series agree with the mean value and standard deviation of the x's of the probability-function to be determined. He found that he could not do this without resorting to certain approximations which seem to the present writer to be somewhat forced. The writer has been able to get around this difficulty by setting up equalities between the mean value and standard deviation of the transforms of the x's of the data-series and the probability-distribution. This seems more logical because the mean value and the standard deviation have more statistical significance when associated with a normal or nearly normal distribution.

In order to better grasp this method of approach we return with Slade to the general problem of setting up a function which will give a picture of the situation when the variates are limited by a lower bound -b and an upper bound +g. The problem is essentially then, the problem of transforming the normal probability distribution represented by equation (1).

$$P(u) = (1/\sqrt{2\pi}) \int_{u_o}^{u - u_o} \exp[-(u - u_o)^2/ 2] \cdot du \qquad (1)$$

Thus the transformed variate is allowed to range only over the finite interval $-b < x < g$.

As indicated above, a convenient way of doing this is to use the relation

$$u = c \log [(b + x)/(g - x)] \qquad (2)$$

This relation causes x to vary from $-b$ to $+g$ as u varies from $-\infty$ to $+\infty$, and vice versa.

If we now transform the probability-distribution (1) by means of the relation (2), we have

$$du = c\,dx/(b+x)(g-x)$$

and the integral becomes

$$c/\sqrt{2\pi}\int_{x_0}^{x-x_0} \exp\left\{-(c^2/2)[\log(b+x)/(g-x) - \log(b+x_0)/(g-x_0)]^2\right\}\cdot dx/(b+x)(g-x) \qquad (3)$$

where x_0 is the transformed value of u_0.

It should be noted that the essential question is the determination of the probability corresponding to a given deviation $(x - x_0)$ which is given by the integral (3). The formal equation of the frequency-curve is secondary in importance. It is given by the integrand of this integral, multiplied by N, namely

$$y = [N/(b+x)(g-x)]\exp\left\{-(c^2/2)[L(x) - L_0]^2\right\} \qquad (4)$$

where $L(x) = \log[(b+x)(g-x)]$ and $L_0 = \log[(b+x_0)/(g-x_0)]$.

Having decided to use the function $u = c\log[(b+x)/(g-x)]$ as the means of transforming the normal probability-distribution of infinite spread into one with a finite spread, we now seek the best method of fitting the transformed probability distribution to the schedule of data of the particular problem studied.

Thus far the function $u = c\log[(b+x)/(g-x)]$ has been considered as a means of transforming the normal probability-distribution into the particular distribution applicable to the problem in hand. There is no reason why one should not take the reverse point of view and consider this function as a means of transforming the particular distribution of finite range into a normal distribution of infinite range. Taking this latter point of view the function $u = c\log[(b+x)/(g-x)]$ transforms each value of x, say x_i, of the original data-schedule into a corresponding value of u, say u_i. These transformed values u_i should form an approximately normal distribution of infinite range. Since the mean and standard deviation have more statistical significance when associated with a normal or nearly normal distribution, it seems more reasonable to fit the u_i to a normal distribution by using the mean and standard deviation of the u_i, than to employ the mean and standard deviation of the original x_i for this purpose.

The mechanics of the problem is now to set up formulas for the determination of the unknowns, c and x_0, in the probability-distribution (3). This may be done as follows: The value x_0 is to correspond to the mean value of u which will be denoted by u_0. Thus

$$u_0 = \Sigma u_i/N$$

where N is the total number of observations, or

$$u = c\,\Sigma\log[(b+x_i)/(g-x_i)]/N = c\cdot L_0$$

where

$$L_0 = \Sigma\log[(b+x_i)/(g-x_i)]/N \qquad (5)$$

Since c is unknown, it is possible only to determine L_0 at the outset, rather than u_0. However, from the relation

$$u_0/c = \log[(b+x_0)/(g-x_0)] = L_0 \qquad (6)$$

it is now possible to determine x_0. It will be found that in general x_0 differs somewhat from the mean of the x_i. On the other hand, for reasons which will appear later, x_0 is usually not far different from the median of the x_i.

We now wish to determine c so that the standard deviation of the u_i is the same as the standard deviation of the corresponding normal probability-distribution. Since the standard deviation of the normal distribution (1) is equal to unity we have the relation

$$1/N \sum_{i=1}^{N} \left\{c \log[(b + x)/(g - x)] - c \log[(b + x_0)/(g - x_0)]\right\}^2 = 1$$

which may be written as

$$c^2/N \sum [L(x_i) - L_0]^2 = 1$$

under the notation used in formula (4), or

$$1/c^2 = \sum [L(x_i) - L_0]^2/N \tag{7}$$

The relations (5), (6), and (7) determine x_0 and c so that the mean value of the u_i and the standard deviation of the u_i equal the mean value and standard deviation of the corresponding theoretical normal distribution (1).

For purposes of computation, it is more convenient to use logarithms to the base 10 than natural logarithms. Employing the relation: $\text{Log}_e z = (\log_e 10) \log_{10} z$ we define $\ell(x) = \log_{10} [(b + x)/(g - x)]$ and $\ell_0 = \ell(x_0)$. Thus

$$u = (c \log_e 10)\, \ell(x) \text{ and } u_0 = (c \log_e 10)\, \ell_0 \tag{8}$$

and formulas (5) and (7) become

$$\ell_0 = (1/N) \sum_{i=1}^{N} \ell(x_i) \tag{9}$$

$$1/(c \log_e 10)^2 = (1/N) \sum_{i=1}^{N} [\ell(x_i) - \ell_0]^2 \tag{10}$$

These formulas offer the solution sought. In the computation it will be found sufficient to compute the reciprocal of $(c \log_e 10)$. It will not be necessary to compute c.

Since ℓ_0 is the mean value of $\ell(x_i)$, it follows as in the case of the calculation of the second moment [13, p. 26]

$$(1/N) \sum [\ell(x_i) - \ell_0]^2 = (1/N) \sum \ell^2(x_i) - \ell_0^2$$

and accordingly, formula (10) may be written

$$1/(c \log_e 10) = \sqrt{(1/N) \sum \ell^2(x_i) - \ell_0^2} \tag{10a}$$

IV--Application of the method

For purposes of demonstration we shall apply the method to the 57-year record of the annual flood-peaks of the Tennessee River at Chattanooga, Tennessee, 1875 to 1931 [7, pp. 204 and 412, and Fig. 14, p. 415].

In the first place it may be noted that it is not necessary to measure the values of x from the mean flood-peak. We can take x_i as given directly from the data-schedule, arranged in order of magnitude. We first choose the upper and lower limits to correspond to choices made by Slade in handling this same problem, namely, for Curve I, b = -48.56, g = 658.56. Since the mean is x = 208.56, this corresponds to Slade's b = -160, g = 450. [7, p. 412]. It is then necessary to determine $\log_{10}[(b + x)/(g - x)]$ for each observation. The sum of these logarithms divided by N = 57 gives the mean logarithm ℓ_0. We then need the sum of the squares of these logarithms, and divide this sum by N to get $(1/N) \sum \ell^2(x_i)$. This enables us to obtain $(1/c \log_e 10)$ from formula (10a). The computation may be arranged as shown in Schedule 1.

Column (6) is the result of subtracting item in column (5) from corresponding item in column (4). The algebraic sum of figures in column (6) divided by N gives l_0. The sum of items in column 7 divided by N gives $(1/N) \sum \ell^2(x_i)$ and when l_0^2 is subtracted from this, the result is $[1/(c \log_e 10)^2]$. The square root of this number gives $[1/(c \log_e 10)]$. For the above problem the result is

$$\text{Constant I} = l_0 = -0.47724$$

$$\text{Constant II} = [1/(c \log_e 10)] = 0.23054$$

Schedule I--Determination of constants, Tennessee River at Chattanooga, 57-year period
(Curve I, $g = 658.56$, $b = -48.56$)

(1) x_i	(2) $(b + x)$	(3) $(g - x)$	(4) $\log (b + x)$	(5) $\log (g - x)$	(6) $l(x)$	(7) $l^2(x)$
361	312.44	297.56	2.49477	2.47357	+0.02120	0.00045
349	300.44	309.56	2.47776	2.49074	-0.01298	0.00017
310	261.44	348.56	2.41737	2.54228	-0.12491	0.01560
123	74.44	535.56	1.87181	2.72881	-0.85700	0.73445
108	59.44	550.56	1.77408	2.74080	-0.96672	0.93451
85.9	37.34	572.66	1.57217	2.75789	-1.18572	1.40588

From these constants the values of x corresponding to various probabilities may be found by setting up a second schedule somewhat similar to that of Slade [7, table 10, p. 412]. This schedule is shown below for the more significant probabilities.

Schedule II--Theoretical probability-distribution, modified Slade method, Tennessee River at Chattanooga, 57-year record ($g = 658.6$, $b = -48.6$, constant I = -0.47724, constant II = 0.23054)

(1) Prob. in per cent	(2) $(u - u_0)$	(3) $l(x) - l_0$ = Const. II × col. (2)	(4) $l(x)$ = Const. I + col. (3)	(5) Log in usual form	(6) z = antilog col. (5)	(7) $(z + 1)$	(8) $g - x = [(b + g)/(z + x)]$	(9) x	(10) Ratio x to mean
.01	3.72	0.85761	0.38037	0.38037	2.401	3.401	179.4	479.2	2.298
.1	3.09	0.71237	0.23513	0.23513	1.718	2.718	224.4	434.2	2.082
1.0	2.33	0.53716	0.05992	0.05992	1.148	2.148	284.0	374.6	1.796
3.0	1.88	0.43342	-0.04382	9.95618	0.904	1.904	320.4	338.2	1.622
5.0	1.64	0.37809	-0.09915	9.90085	0.796	1.796	339.6	319.0	1.530
10.0	1.28	0.29509	-0.18215	9.81785	0.657	1.657	368.1	290.5	1.393
15.0	1.04	0.23976	-0.34748	9.76252	0.579	1.579	386.3	272.3	1.306
20.0	0.84	0.19365	-0.28359	9.71641	0.520	1.520	401.3	257.3	1.234
50.0	0.0	0.0	-0.47724	9.52276	0.333	1.333	457.6	201.0	0.964
80.0	-0.84	-0.19365	-0.67089	9.32911	0.213	1.213	502.9	155.7	0.747
85.0	-1.04	-0.23976	-0.71700	9.28300	0.192	1.192	511.7	146.9	0.704
90.0	-1.28	-0.29509	-0.77233	9.22767	0.169	1.169	521.8	136.8	0.656
95.0	-1.64	-0.37809	-0.85533	9.14467	0.140	1.140	535.1	123.5	0.592
97.0	-1.88	-0.43342	-0.91066	9.08934	0.123	1.123	543.2	115.4	0.553
99.0	-2.33	-0.53716	-1.01440	8.98560	0.097	1.097	556.1	102.5	0.491
99.9	-3.09	-0.71237	-1.18961	8.81039	0.065	1.065	572.8	85.8	0.411
99.99	-3.72	-0.85761	-1.33485	8.66515	0.046	1.046	583.2	75.4	0.362

Figures in the first two columns are obtained directly from tables of the standard probability-integral. The figures in the third column are the result of multiplying $(u - u_0)$ by $[1/(c \log_e 10)]$. Referring to the formulas (8), it is seen that $(u - u_0) = (c \log_e 10)[l(x) - l_0]$ or:

$$(u - u_0)/c \log_e 10 = l(x) - l_0$$

In other words the figures in column (2) are multiplied by Constant II to give the figures in column (3). In column (4), $l(x)$ is obtained by adding Constant I ($= l_0$) to figures in column (3). In column (5), the logarithm is written in the usual form with the decimal part positive (the -10 is omitted in writing it). In column (6) are given the antilogs of the logarithms in column (5). For most purposes it will be sufficient to take the antilogs to the nearest third decimal place. These antilogs give the values of $[(b + x)/(g - x)]$. If we set $[(b + x)/(g - x)] = z$, and solve for x in terms of z, we obtain

$$(g - x) = [(b + g)/(z + 1)] \tag{11}$$

Thus we add 1 to z in column (7) and divide $(b + g)$ by $(z + 1)$ to obtain values of $(g - x)$ in column (8). In other words 610 (= b + g) is divided by items in column (7) to give items in column (8). Column (9) needs no explanation. The mean was taken to two decimals (208.56) in obtaining column (10).

Figure 1 shows the curve obtained by using figures shown in the first and last columns of Table 1; the resulting curve is designated as Curve 1 of Figure 1. Curve S is the curve obtained by the Slade method, using the same upper and lower limits. The dots are plotted from the original data-schedule by the usual method. Curves I and S are very similar. However, neither of the curves furnishes a good fit for the lower flood-values. From a theoretical point of view there seems to be no reason why a poorer fit should be obtained at this end of the series than at the other end. It would appear then that the lower limit 48.6 was too high. We accordingly assumed a smaller minimum limit, taking it at zero, without changing the upper limit. The result was Curve II. It is interesting to note that the effect of this change was not only to bring down the lower part of the curve by a considerable amount, but a very discernible effect was produced upon the upper part of the curve. This leads one to the observation that in determining the theoretical probability-distribution by the modified Slade method it is important to use some care in the choice of the lower limit as well as the upper limit.

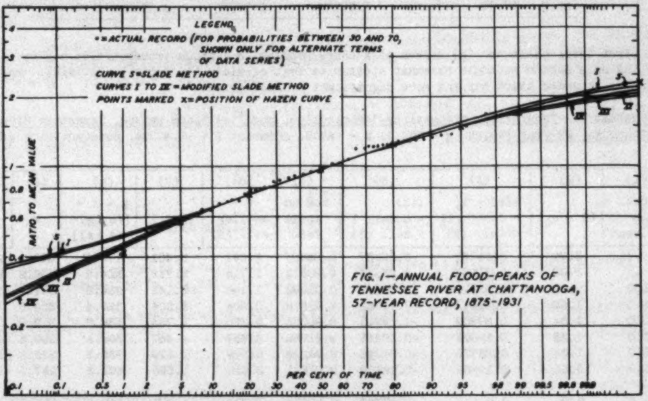

FIG. 1—ANNUAL FLOOD-PEAKS OF TENNESSEE RIVER AT CHATTANOOGA, 57-YEAR RECORD, 1875-1931

Table A --Calculations for Figure 1

Curve	Maximum limit		Minimum limit		Test of fit			Mean	Median	Trans-formed mean
	1000 cfs	Ratio to mean	1000 cfs	Ratio to mean	Lower ratio	Upper ratio	Test ratio			
S	658.6	3.16	48.6	0.23	....	....	....	208.6	201	201.0
I	658.6	3.16	48.6	0.23	40.2	26.5	33.3	208.6	201	201.0
II	658.6	3.16	0.0	0.0	34.0	30.6	33.3	208.6	201	204.1
III	609.0	2.92	0.0	0.0	33.3	31.4	33.3	208.6	201	204.6
IV	549.0	2.63	0.0	0.0	32.1	32.9	33.3	208.6	201	206.2
Hazen curve		C.V. = 0.28	Adjusted C.S. = 0.375				Means	208.6	201	204.4

V--Ratio-test for goodness of fit developed by writer

In order to furnish a test of comparative goodness of fit for different choices of upper and lower limits, other than the appearance of the theoretical curve in relation to the plotted points of the data-schedule on the chart, the writer has developed a method which he believes will be of value in dealing with flood- and precipitation-data. In this work we are particularly interested in the behavior of the extreme parts of the curve. The parts of the distribution for probabilities between 10 per cent and 90 per cent are very little affected by change in the choice of values of the limits b and g. Thus particular attention is given to the part of the distribution corresponding to probabilities greater than 90 per cent and less than 10 per cent.

After choice of b and g, the value of c was determined so that the standard deviation of the theoretical curve would be equal to the standard deviation of the transformed series obtained from the original data-schedule. This statement can be expressed mathematically as follows: The constant c was determined so that following equation would hold

$$(1/N)(c \log_e 10)^2 \sum_{i=1}^{N} [l(x_i) - l_0]^2 = (1/\sqrt{2\pi}) \int_{-\infty}^{+\infty} (u - u_0)^2 \, e^{-(u - u_0)^2/2} \, . \, du \qquad (12)$$

On the left of this equation is the second moment (square of the standard deviation) of the u_i which are the transforms of the x_i of the data-schedule, and on the right is the second moment of the theoretical u-curve. The integral on the right is known to be equal to unity. If the theoretical u-curve is to represent fairly the trend of the u_i for extreme values of u we shall ask that the contribution of the extreme parts of the theoretical curve and data-series to the evaluation of the second moment be as well matched as possible. So far we have only required that the total contributions of the theoretical curve and the transformed data-series to the second moment be equal.

In order to match the sum of squares of variations for extreme values of u and the theoretical curve we choose a probability midway between probabilities corresponding to two consecutive plotted points of our data-schedule in the neighborhood of 10 per cent. For the particular problem treated here, the probability corresponding to x = 275 (ratio to mean = 1.319) is 9.649 per cent and (see Fig. 1) the probability corresponding to x = 271 (ratio to mean = 1.300) is 11.404 per cent [7, table 4, p. 400]. The probability midway between these two is 10.526 per cent. There are six flood-peaks larger than x = 271. It will be found that 6/57 = 10.526 per cent. (In other words we take about 10 per cent of the observations and divide this number by N to get the exact percentage corresponding to this number of observations.) We then find from standard tables of the probability-integral the value of $(u - u_0)$ of the probability-integral which corresponds to such a probability. For this problem we find that $(u - u_0) = 1.252$. This is the value of $(u - u_0)$ such that

$$(1/\sqrt{2\pi}) \int_{(u - u_0) = 1.252}^{\infty} e^{-(u - u_0)^2/2} \, . \, du = 0.10526$$

This does not involve choice of b, g, c, or x_0. It means that the part of the normal probability-distribution from $u - u_0 = 1.252$ to $u - u_0 = +\infty$ corresponds to 10.526 per cent of the total normal probability-distribution. We now ask for the contribution of this part of the normal probability-distribution to the total second moment. This is given by the integral

$$(1/\sqrt{2\pi}) \int_{(u - u_0) = 1.252}^{\infty} (u - u_0)^2 \, e^{-(u - u_0)^2/2} \, . \, du$$

Integrating by parts we have

$$\left[-(u - u_0)(1/\sqrt{2\pi}) \, e^{-(u - u_0)^2}\right]\Big|_{u - u_0 = 1.252}^{\infty} + (1/\sqrt{2\pi}) \int_{u - u_0 = 1.252}^{\infty} e^{-(u - u_0)^2/2} \, . \, du$$

Using the conventional representation

$$\varphi(u - u_0) = (1/\sqrt{2\pi}) \, e^{-(u - u_0)^2/2}$$

This may be written

$$\mu_2(u - u_0) = (u - u_0)\varphi(u - u_0) + \int_{u - u_0 = 1.252}^{\infty} \varphi(u - u_0) \, du \qquad (13)$$

Using standard tables for the normal probability-function φ and the probability-integral φ du, one finds that this expression = 1.232 × 0.1822 + 0.10526 = 0.333. Thus this part of the normal distribution contributes 33.3 per cent of the total second moment. This result is entirely independent of choice of b and g, and determination of constants c and x_0. Since the normal distribution is symmetrical, the same percentage applies to the amount contributed to the second moment by negative values of $(u - u_0)$ between -1.252 and $-\infty$. These negative values correspond to the six lowest flood-peaks in the original data-series, by the same reasoning as that applied to the upper extreme of the data-series. Referring back to equation (12), we have

$$(1/N)(c \log_e 10)^2 \Sigma_6[\ell(x_1) - l_0]^2 = 0.333 \qquad (14)$$

where the summation is taken over the six extreme terms of the series, either the six largest or the six smallest. Since $[N/(c \log_e 10)^2]$ is equal to the total sum of $[l(x_1) - l_0)]^2$ for all N terms, the relation (14) may be written in the form

$$\Sigma_6[l(x_1) - l_0]^2 / \Sigma_N[l(x_1) - l_0]^2 = 0.333 \qquad (15)$$

The best choices of b and g would appear to be those that cause this ratio to take on values nearest to the theoretical ratio 33.3 per cent, and are at the same time consistent with the physical conditions of the problem.

We have named the ratio obtained from the terms at the lower extreme of the series the lower ratio and that obtained from the upper extreme the upper ratio. In most cases, choice of b affects particularly the lower ratio, and choice of g the upper ratio. It should be noted that in calculating

$$\Sigma_6[l(x_1) - l_0]^2$$

the actual differences $[l(x_1) - l_0]$ have to be found and then squared. It is not possible as in the case of the determination of the total second moment to calculate

$$(1/N) \Sigma l^2(x_1)$$

first and then subtract l_0^2.

Resume of method of setting up ratio-test--(1) Take about ten per cent of the terms of the series, say m terms, and divide this number by the total number of terms (N). Carry the division to five decimal places, thus obtaining a percentage-figure p in the neighborhood of ten per cent.

(2) Using tables of the probability-function and integral determine the value of $(u - u_0)$ and $\varphi(u - u_0)$ corresponding to the value of the probability-integral equal to (p/100). Substitution in formula (13) will give the desired theoretical ratio $\mu_2(u - u_0)$. The theoretical ratio may also be found directly from the ratio (m/N) by the use of a table similar to Table 1 explained below.

(3) To find the lower ratio, for each of the m terms at the lower extreme of the data-series, determine; $[l(x_1) - l_0]^2$, and sum these squares. Divide this sum by the total sum for the whole series which is equal to $N/(c \log_e 10)^2$.

(4) To find the upper ratio, calculate $[l(x_1) - l_0]^2$ for the m terms at the upper extreme of the data-series and proceed as in finding lower ratio.

(5) The upper and lower ratios should approximate the theoretical ratio $\mu_2(u - u_0)$. In the particular problem treated above (see Fig. 1) the lower ratio was raised and the upper ratio lowered by lowering both limits b and g. This cannot, however, be stated as a general rule of procedure. Visual inspection of the relation of the theoretical curve to plotted points on the chart is probably the best guide for choosing a second set of trial-values of b and g.

After a second curve is drawn the behavior of the upper and lower ratios will usually give the cue for further trial-choices of b and g. If after the test-ratios have been calculated, there is doubt as to which of several curves is the best fit, the present method may be supplemented by the use of the Van Uven graphical test as explained in Section IV.

Application of ratio-test--Let us apply this test to the flood-data of the Tennessee River at Chattanooga.

(1) Here we have a 57-year record; thus we take m = 6 and (m/N) = 6/57 = 0.10526.

(2) From Table 1 we see that the theoretical ratio lies between 0.33294 and 0.33451. Multiplying the difference 0.00157 by 0.26 gives 0.00041 which added to 0.33294 gives 0.33335. Dropping the last two figures we obtain 0.333 which is as accurate as the conditions of the problem warrant. This is the same result as that obtained by the use of the formula (13) above. Thus it is seen that the determination of the theoretical ratio is not a complicated matter. The theoreti-

cal ratio when once calculated serves for all series of the same number of terms (provided also that the same number of terms m is used to test the extreme parts of the series).

(3) To calculate the upper and lower ratios we first determine the denominator of these ratios. This we shall call Constant III = $\sum (1 - l_0)^2 = [N/(c \log 10)^2] = \sum l^2(x_1) - N l_0^2$. Thus we subtract $N l_0^2$ from the sum of the items in column (7) of Schedule 1. For Curve I we have 16.01165 - 57 x 0.227758 = 3.0294 = Constant III. A check on this is to multiply the Constant $[1/(c \log 10)^2]$ by N. This in general will not give as accurate a result as the first method unless extra decimal places are used. In the original calculation of $[1/(c \log 10)^2]$, the result is 57 x 0.05315 = 3.02955. The first result is to be preferred.

(4) We now need the numerators of the upper and lower ratios. The work for this may be arranged as in Schedule III.

Table 1--Test-ratio determined from ratio (m/N)

(m/N)	Theoretical ratio and difference		(m/N)	Theoretical ratio and difference	
0.11000	0.34063		0.10000	0.32491	
0.10900	0.33913	150	0.09900	0.32326	165
0.10800	0.33761	152	0.09800	0.32159	167
0.10700	0.33607	154	0.09700	0.31991	168
0.10600	0.33451	156	0.09600	0.31822	169
0.10500	0.33294	157	0.09500	0.31651	171
0.10400	0.33136	158	0.09400	0.31479	172
0.10300	0.32977	159	0.09300	0.31305	174
0.10200	0.32816	161	0.09200	0.31129	176
0.10100	0.32654	162	0.09100	0.30952	177
0.10000	0.32491	163	0.09000	0.30774	178

(5) The upper and lower ratios for Curve I are thus found to differ considerably from the test-ratio + 33.3 per cent. Inspection of the Figure would suggest trying a lower minimum limit. Taking the lower limit at zero and keeping the upper limit at 658,000 cubic feet per second (cfs) or 3.16 times the mean annual flood-peak, we obtain Curve II with upper ratio 30.6 per cent and lower ratio 34.0 per cent. The effect of lowering the lower limit has thus been to raise the upper ratio and lower the lower ratio. If we bring the upper limit down to 609,000 cfs, (or 2.92 times the mean annual flood-peak), leaving the lower limit at zero, we obtain Curve III with upper ratio 31.4 per cent and lower ratio 33.3 per cent. If we further lower the upper limit to 549,000 cfs (or 2.63 times the mean annual flood peak), we obtain Curve IV with upper ratio 32.9 per cent and lower ratio 32.1 per cent. Calculations for another curve were made with upper limit at 2.44 times the mean. The upper and lower ratios for this curve were respectively 34.3 per cent and 31.1 per cent. Perhaps the conclusion here is that Curve IV gives the best fit. This would be the case if the choice were made upon the basis of balanced upper and lower ratios and nearness of upper ratio to test ratio. At any rate the positions of Curves III and IV are very close on the Figure. Curve IV gives a 1000-year flood of 1.88 times the mean, and Curve III, 1.93 times the mean annual flood-peak.

Schedule III--Determination of upper and lower ratios
$[l_0 = -0.47724$, Constant III $= 3.0294 = \sum (1 - l_0)^2]$

Order of magnitude	l	$l - l_0$	$(l - l_0)^2$	Resulting ratios
1	0.02120	0.49844	0.24840	Upper ratio =
2	0.01298	0.46426	0.21557	$\frac{0.8017}{3.0294} = 0.2646$
3	0.12491	0.35233	0.12412	
4	0.19864	0.27860	0.07762	
5	0.20465	0.27259	0.07431	= 26.5 per cent
6	0.22888	0.24836	0.06170	
		Total	0.80172	
52	0.78814	0.31090	0.09666	Lower ratio =
53	0.79407	0.31683	0.10036	
54	0.84386	0.36662	0.13440	$\frac{1.21725}{3.0294} = 0.4018$
55	0.85700	0.37976	0.14425	
56	0.96672	0.48948	0.23961	= 40.2 per cent
57	1.18573	0.70849	0.50197	
		Total	1.21725	

VI--Van Uven graphical test of goodness of fit

Arne Fisher in his article discussing Slade's original paper suggests that a graphical method developed by the Dutch mathematician M. J. Van Uven might be used in determining the upper and lower limits of the probability-distribution function employed by Slade. This graphical method cannot be used in determining directly the upper and lower limits of frequency-distribution functions for flood and precipitation for the reason that the position of the point of inflection and the inflectional tangent cannot be determined with any degree of precision because of the fact that the position of the point of inflection is near the upper extreme of the series of data furnished (this statement can be easily checked by taking data for the typical flood-frequency problem and plotting the points in the usual way on arithmetical probability-graph paper). The suggestions which Van Uven makes in regard to testing whether the upper and lower limits chosen are too high or too low may, however, be very profitably added to the tests of goodness of fit of probability-distribution curves now available. Van Uven's test applies particularly to the probability-distribution function of the type that is discussed in this paper. For a complete description of the method and the reasoning upon which it is based, the reader is referred to Van Uven's original article [12, pp. 533-546].

The part of Van Uven's paper that is applicable to testing upper and lower limits of the logarithmic probability-distribution-functions of flood- and precipitation-data has to do with the shape of the curve obtained by plotting the true parameter of the probability-integral against the values of the probability-distribution-function corresponding to the schedule of flood- or precipitation-data in question. If the probability-distribution-function is such that a strictly linear relation exists between the parameter of the probability-integral and this function, we know that we have succeeded in transforming the original data-series into a normally distributed data-series. Thus the points plotted on ordinary graph-paper in this way should approximate a straight line.

For example in the case of Curve I of the annual flood-peaks of the Tennessee River at Chattanooga, corresponding to each of the 57 terms of the data-series, a percentage-of-time figure has already been calculated. These percentage-of-time figures were used in plotting the dots which appear on Chart I indicating actual flood-peaks. To get the parameter-values of the probability-integral we have to go one step further in the calculations and from the table of the probability-integral find the values of the parameter which correspond to the percentage-of-time figures just referred to. Thus for a percentage-of-time figure of 0.877 per cent, the parameter of the probability-integral will be 2.375. The next to the highest annual flood-peak corresponded to a percentage-of-time figure of 2.362 per cent and this in turn would correspond to a value of the parameter of the probability-integral of 1.138. The schedule of parameter-values of the probability-integral corresponding to various terms of the data-series, when once set up, will of course apply to that particular data-series irrespective of what probability-function is used to approximate it. Thus the same schedule of parameter-values, which we shall denote by t, applies to all approximating flood-frequency curves for that particular data-series. To plot the points to be used as a basis of testing frequency-curve I, we use as ordinates the parameter-values t just referred to, and as abscissae the corresponding values of the logarithmic distribution-function $\log [(b + x)/(g - x)]$ denoted by $l(x_1)$ which appear as the items in column (6) of Schedule I. The points plotted in this way give the basis of Test-Curve I shown on Figure 2.

We see that these points lie roughly along a straight line. If a curve is drawn through the succession of points plotted, the following conclusions may be drawn on the basis of Van Uven's test on ordinary graph-paper: (a) If curve turns up at left extremity, the lower limit is too high; (b) if curve turns down at left extremity, the lower limit is too low; (c) if curve turns down at right extremity, the upper limit is too low; (d) if curve turns up at right extremity, the upper limit is too high. Applying this test, one concludes from Test-Curve I that the lower limit is too high. After reducing the lower limit to zero, we have Test-Curve II in which the points lie very nearly along a straight line. Setting up the Van Uven test for Curve III, we again find that the points lie very nearly along a straight line. In the case of the test for Curve IV the curve of the plotted points seems to turn down a trifle at the right-hand extremity. Thus the Van Uven Test indicates that Curve II or Curve III is the best fitting curve, rather than Curve IV. Since the numerical ratio-test described in the last Section, gives a preference to Curves III and IV over Curve II, the conclusion arrived at in using both tests is that of the three curves II, III, and IV, Curve III is to be preferred.

The feature of Van Uven's test is that it can be applied as soon as the figures in column (6) of Schedule I are calculated. Thus it can be applied before the squares in column (7) are calculated, and before Schedule II is set up. It accordingly represents a short cut in determining whether the upper and lower limits chosen are going to give a suitable probability-distribution-function or not. The Van Uven test would have shown at once that Curve I would not be suitable and would have indicated that the lower limit should be taken at a lower value. This would have suggested the trial of the limits used in Curve II, and the upper limit taken in Curve III might very well have been tried as well as that taken in Curve IV. In the case of Curve IV the Van Uven test indicates that the fit is not as good as that in Curves II and III. As the tests for Curves II and III both appear to be fairly good, both of these curves might have been drawn up, involving the calculations of column (7) in Schedule I and those of Schedule II for the two curves.

Persons that are accustomed to use probability-paper in plotting flood- and precipitation-data may find it more convenient to use arithmetical probability-paper in applying the Van Uven test. In this case it will not be necessary to look up the parameter-values of the probability-integral corresponding to the percentage-of-time figures. The spacing of the probability-paper takes care of this. The curve will, however, be turned around. The logarithmic distribution-function will now appear as the ordinate, and what we have called t in Figure 2 will now correspond to horizontal distance. If the percentage-of-time figures are taken such that we have the upper end of the data-series corresponding to the right-hand extremity of the curve, and the lower end of the data-series corresponding to the left-hand extremity of the curve, then Van Uven's analysis would be applied as follows on arithmetical probability-graph paper: (a) If curve turns down at left extremity, the lower limit is too high; (b) if curve turns up at left extremity, the lower limit is too low; (c) if curve turns down at right extremity, the upper limit is too high; (d) if curve turns up at right extremity, the upper limit is too low.

It is of some statistical interest that Constants I and II represent respectively the abscissa of the point where the "theoretical" straight line crosses the zero-ordinate (t = 0) and the reciprocal of the "slope" of this line. The so-called "theoretical" straight line is the line which would be determined by the l(x)'s of the theoretical curve which has been chosen to represent the data-series (this would be Curve I in the case of Test-Curve I and the l(x)'s just referred to would be the figures in column (4) of Schedule II). It is the best fitting straight

VII--Rules of procedure for determining best-fitting probability-distribution-curve

(1) Set up Schedule A as shown in the skeleton outline below.

Schedule A

(1)	(2)	(3)	(4)
Annual maxima in order of magnitude	Ratio of figures in column (1) to their mean value	Percentage-of-time figures [2, p. 13]	Value of parameter of probability-integral

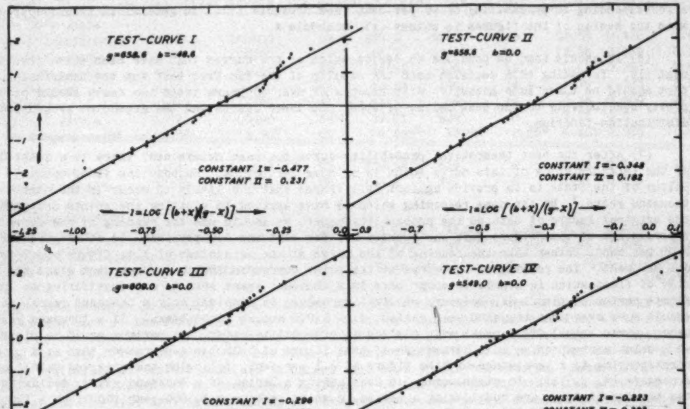

FIG. 2-TEST-CURVES FOR SCHEDULE I

The entries in column (1) are the same as the entries in column (1) of Schedule I, referred to as x_i. If arithmetical probability-paper is available for applying the Van Uven test, column (4) may be omitted. Using logarithmic probability-graph paper, the annual maxima are plotted from the figures in columns (2) and (3) [see 2, chapter 3, or 7, pp. 78 and following].

(2) Decide upon an upper and a lower asymptotic limit. In the case of good-sized rivers in the northeastern section of the United States, one might start with an upper limit of about ten times the mean annual flood-peak and a lower limit of about 0.35 times the mean. Judging by an analysis of the records of the Tennessee and Kanawha rivers, lower values of both of these limits should be used for rivers on the eastern and southern slopes of the Ohio Basin (see Table 2).

(3) Apply the Van Uven test. This involves first of all the filling in of figures in the first six columns of Schedule I. Note that b is the negative of the lower asymptotic limit chosen. If arithmetical probability-graph paper is available, one then plots the percentage-of-time figures of column (3) of Schedule A, against the values of $l(x) = \log [(b + x)/(g - x)]$ obtained from column (6) of Schedule I. If arithmetical probability-graph paper is not available, one plots the figures of column (4) of Schedule A against the figures of column (6) of Schedule I on ordinary graph-paper. In plotting the points it will usually be best to omit every other one, for percentage-of-time figures between say, 20 per cent and 80 per cent or for parameter-values of the probability-integral between 0.80 and -0.80.

A straight line is then fitted graphically to this set of points, and applying the test as described in Section VI one can tell whether limits chosen should be raised or lowered. A second trial will involve the recalculating of items in columns (2) to (6) of Schedule I, but items in Schedule A remain unchanged.

(4) When the repeated application of the Van Uven test has indicated two or three suitable choices of upper and lower limits, the remaining column in Schedule I is calculated and Constants I and II are determined for these curves. It is then necessary to carry through the work of setting up Schedule II. The curves can then be plotted on the logarithmic probability-graph paper on which the flood- or precipitation-record has already been plotted as a series of points.

(5) The writer would advise that some such record as that which appears in Table A of Figure 1 be included with the chart. The numerical ratio-test of goodness of fit should be calculated as indicated in "Application of ratio-test" above. The "transformed mean" denotes the value of x corresponding to probability of 50 per cent (see Schedule II). In general it should approximate the median of the figures in column (1), Schedule A.

(6) It should then be possible to decide which of the curves that have been drawn give the best fit. In making this decision both the results of the Van Uven test and the numerical ratio-test should be taken into account. With records of over 40 years these two tests should point pretty conclusively to the best choice of upper and lower limits and the attendant probability-distribution-function.

(7) After the best theoretical probability-curve has been determined, there is a question of the interpretation of this curve which is of some pertinence. Suppose the flood-control policy of the State is to provide against "all floods that are likely to occur in the next thousand years." By the same reasoning which we have applied to plotting the points designating the original record of data on the probability-paper, we should use the reading of the curve at a percentage-of-time figure half way between 0.1 per cent and 0.0 per cent, in other words at 0.05 per cent, rather than the reading of the curve at the percentage-of-time figure equal to 0.1 per cent. The reading of the curve at the point corresponding to 0.1 per cent gives the size of flood which is liable to occur once in a thousand years when we are considering an infinite period of time. If, however, we limit ourselves to consider only a thousand years, we should have over that thousand year period, just 1,000 annual flood-peaks. If a thousand years hence, these annual flood-peaks were plotted on probability-paper, the maximum would be plotted at a point corresponding to a percentage-of-time figure of 0.05 per cent rather than at a point corresponding to a percentage-of-time figure of 0.1 per cent, following the accepted method of procedure [2, p. 13]. In other words, in considering a period of a thousand years, following the present time, we are considering a period in which perhaps a 10,000-year flood, or a 100,000-year flood, or a 1,000,000-year flood might occur. Thus we take the average of the corresponding percentages of time varying from zero to 0.1 per cent and take the reading of the curve at 0.05 per cent.

The answer to this question is made clearer if one considers the period of one or two years rather than a thousand years. A "one-year" flood, or maximum flood expected in a given year, is not found by reading the chart at 100 per cent, but rather by dividing the time-period of one year in half and reading the chart at 50 per cent. Similarly, the maximum flood expected in a two-year period of time would be found by taking the mid-point of the upper half, which would be the reading at 25 per cent, etc.

Suggestions for reduction in labor of calculation--For most purposes there seems to be no reason why four-place logarithms rather than five-place logarithms may not be used throughout. This would further imply carrying computations in third, fourth, and fifth columns of Schedule II to only four decimal places. A further reduction in the labor of calculation obtains if b and g are expressed as ratios to mean flood, in column (8) of this schedule. The computation for column (9) will then given directly x expressed as ratio to mean, without the intermediate step set down in column (9) of Schedule II.

If the Van Uven graphical test is set up with considerable care, the value of k (= Constant II) can be determined graphically without the use of the squares in column (7) of Schedule I. If ordinary graph-paper is used as in Chart II, k is the reciprocal of the slope of the best fitting straight line (which will pass through the point determined by l_0 and $t = 0$--see discussion of Figure 2). If arithmetical probability-graph paper is used, k is the slope of this best fitting straight line. The horizontal distance must be measured, in this case, in terms of parameter-values of the probability-integral and not in terms of the readings shown on the horizontal scale of the probability-paper.

VIII--Record of flood- and precipitation-data analyzed by modified Slade method

In Table 2 will be found a record of the principal factors associated with the best fitting probability-distribution-curves pertaining to rivers in New York State, including the Susquehanna, the Tennessee and Kanawha rivers of the eastern and southern slopes of the Ohio Basin, and the 92-year precipitation-record from New Bedford, Massachusetts, recently made available to the Division of State Planning.

Table 2--Record of probability-distribution-curves fitted by modified Slade method

Record	Drainage-area in square miles	Base discharge in cfs	Length record in years	Mean flood in 1000 cfs	Limits in 1000 cfs		Ratio to mean		
					Max.	Min.	Max. Limit	Min. Limit	1000-year flood
ivers of New York State									
Susquehanna at Harrisburg	24100	153000	63	295.3	3950	125	13.38	0.423	3.44
Susquehanna at Towanda	7797	53500	42	96.7	1350	34	13.96	0.352	3.19
Hudson at Mechanicville	4500	24600	49	42.89	430	15	10.03	0.350	2.89
Genesee at Rochester	2467	14200	44	23.38	180	12.5	7.70	0.535	3.13
Black at Watertown	1876	14500	41	23.31	55	14	2.36	0.600	2.17
Delaware at Port Jervis	3076	29400	34	69.91	500	0.	7.15	0.0	2.55
Richelieu at Rouse's Point	7870[a]	3.70[b]	64	6.423[b]	10.00[b]	2.25[b]	1.557	0.350	1.42
ther Rivers									
Tennessee at Chattanooga	21400	112000	57	208.56	609.	0.	2.92	0.0	1.93
Kanawha at Kanawha Falls	8367	60000	58	118.8	370.	9.	3.11	0.075	2.34
recipitation									
New Bedford, Massachusetts									
Maximum two-day storms			92	3.531[c]	13.31[c]	1.61[c]	3.77	0.456	2.31
Maximum six-day storms			92	4.479[c]	80.00[c]	2.25[c]	17.86	0.502	2.66

[a] Includes 436 square miles of water-surface. [b] Gage-height in inches. [c] Precipitation in inches

The results shown in Table 2 were obtained almost entirely by the use of the numerical ratio-test. In the case of the Tennessee River the Van Uven Test was applied as described in this article, and in the case of the Delaware River (the shortest record) it was applied once to check the curve of best fit. The Van Uven Test has been a later development and with the limited statistical help available, it was not possible to extend this test to all the records. Accordingly the results of Table 2 should not be considered as final. On the other hand, since the principal feature of the Van Uven test is the saving of time and labor, the writer does not believe that a further application of the Van Uven test after the numerical ratio-test has been applied many times will change the results significantly. However, he believes that when such statistical help is available, it would be advisable to set up the Van Uven test for all the records.

Besides the results shown in Table 2, charts for the record of the Susquehanna River and the record of the maximum six-day storms at New Bedford, Massachusetts, are shown in Figure 3. On both of these charts Curve IV represents what is considered to be the best fit by the modified Slade method. The curves drawn up by the Slade method are the curves marked S, and the positions of the curves determined by the Hazen method are indicated by the crosses. On the chart of the record of the Tennessee River shown on Figure 1, the curve of best fit is Curve III. In the case of the Susquehanna River and the record of precipitation at New Bedford, the position of the Hazen curve is considerably above that of the best fitting curve obtained by the modified Slade method. In the case of the Slade curve (marked S) its position appears to be close to that of the corresponding curve drawn by the modified Slade method (for the same upper and lower limits) with the exception of the precipitation-record of New Bedford, in which case the Slade curve lies considerably above the curve drawn by the present method. (The comparisons which should be made here are Curves S and I in the case of the Tennessee, Curves S and IV in the case of the Susquehanna, and Curves S and IV in the case of the New Bedford record.) The greatest discrepancy is thus seen to exist in the case of the New Bedford record. This is also true in regard to the Hazen curve. In the case of the Hazen curve, it is to be expected that these discrepancies will be larger in the case of the precipitation records, as the Hazen curve was specifically designed to apply only to flood-records [2, chapter 8).

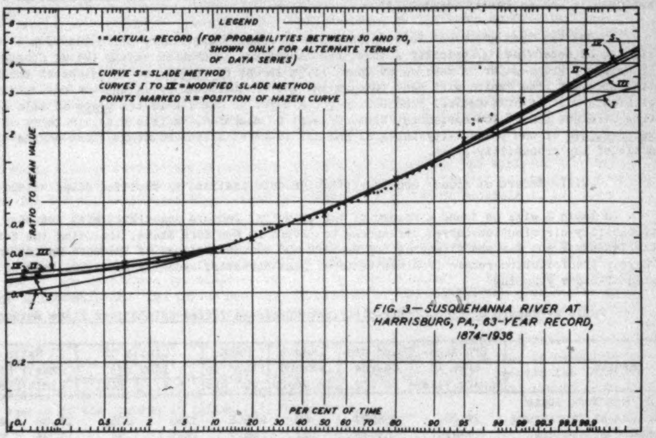

FIG. 3—SUSQUEHANNA RIVER AT HARRISBURG, PA., 63-YEAR RECORD, 1874-1936

Table B --Calculations for Figure 3

Curve	Maximum limit		Minimum limit		Test of fit			Mean	Median	Trans-formed mean
	1000 cfs	Ratio to mean	1000 cfs	Ratio to mean	Lower ratio	Upper ratio	Test ratio			
I	1500	5.08	100	0.339	20.51	44.59	31.69	295.3	264.0	272.4
II	3450	11.68	125	0.423	30.02	35.60	31.69	295.3	264.0	265.1
III	3450	11.68	130	0.440	38.21	41.71	31.69	295.3	264.0	264.1
IV	3950	13.38	125	0.423	30.19	35.27	31.69	295.3	264.0	265.0
S	3950	13.38	125	0.423	.....	.....	.....	295.3	264.0	262.7
Hazen curve	C.V. = 0.410		Adjusted C.S. = 2.148				Means	295.3	264.0	265.0

It is beside the purpose of this article to discuss in detail the results set forth in Table 2. At this stage we are particularly interested in the presentation of this new method of determining probability-distribution-curves. An adequate evaluation of the results obtained will require more varied and extensive application to flood- and precipitation-records.

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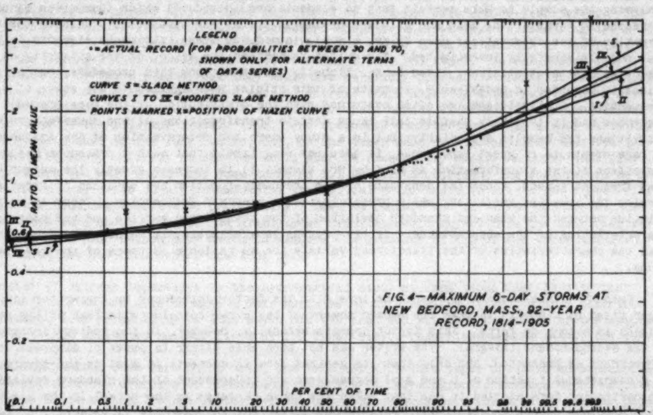

FIG. 4—MAXIMUM 6-DAY STORMS AT NEW BEDFORD, MASS., 92-YEAR RECORD, 1814-1905

Table C --Calculations for Figure 4

Curve	Maximum limit		Minimum limit		Test of fit			Mean	Median	Transformed mean
	Inches	Ratio to mean	Inches	Ratio to mean	Lower ratio	Upper ratio	Test ratio			
S	80.62	18.00	2.24	0.500	....	....	....	4.48	4.11	4.11
I	25.00	5.58	2.00	0.447	26.2	44.8	32.1	4.48	4.11	4.23
II	40.00	8.93	2.25	0.502	31.0	38.1	32.1	4.48	4.11	4.18
III	70.00	15.63	2.25	0.502	30.5	36.5	32.1	4.48	4.11	4.17
IV	80.00	17.86	2.25	0.502	32.0	36.8	32.1	4.48	4.11	4.17
Hazen curve	C.V. = 0.3245		Adjusted C.S. = 5.074				Means	4.48	4.11	4.16

[13] B. H. Camp, The mathematical part of elementary statistics, D. C. Heath and Company, New York, 1931.

[14] See James W. Glover, Tables of probablity and statistical functions, section--"Areas, ordinates, and second to eighth derivatives of the normal curve of error," published by George Wahr, Ann Arbor, Michigan, 1923.

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DISCUSSION

J. J. SLADE, Jr. (Rutgers University, New Brunswick, New Jersey)--The laws of probability are of universal application. That there are different distribution-functions in different fields of application is simply due to the fact that the causes of variation occur in different combinations in the various fields. The statistical analysis of a problem must begin where we ignore causal connections, or else where these are so numerous that it would be futile to take them all into account. At this point we must assume or infer the distribution-function that characterizes the variations of the problem. There are two points of view that may be taken when the distribution-function is introduced: (1) That it is the limit towards which the frequencies of the observed variations tend, or else (2) that it is the transformation of some simpler law towards which the variations of some function of the observations tend. The first is the writer's view, the second is Mr. Kimball's. The distinction is important. In determining the constants of the distribution-function Mr. Kimball transforms the raw data; the writer attempts this determination directly from the observations as recorded.

When it is agreed that the reliable statistical characteristics of a sample are the mean and standard deviation, it becomes of primary importance to examine the statistical processes used in analysing the sample to make certain that no elements are introduced which themselves cannot be expressed in terms of the mean and standard deviation of the raw statistics. Whether the transformation of the observations used by Mr. Kimball introduces these extraneous elements is a point that must be carefully investigated. The writer recognizes the weight of the authority of a number of eminent statisticians, cited by Mr. Kimball, that sanctions this procedure, but he is inclined to consider it undesirable, in spite of this opinion when applied to the study of flood-frequencies. Statisticians are often concerned with the distribution of the more frequent occurrences and it is likely that in this range certain transformations of the observations will not vitiate the results appreciably; but in a study where the determination of the frequencies of rare events is of great importance, it does not seem likely that such a procedure is admissible. The effect of the transformation as used by Mr. Kimball is to increase greatly the weight of the less frequent floods, since the constants of the frequency-function are obtained by first transforming the observed variations while preserving their observed frequencies. There is no simple relation between the mean and standard deviation of the transformed variate and the corresponding characteristics of the observations. If the process is admissible, it needs to be demonstrated that the characteristics of the transformed variate are as reliable as those of the raw observations.

Both Mr. Kimball and Arne Fisher believe that the factor introduced by the writer (in the paper cited by Kimball) to make the second moment of the curve coincide with that of the observations is a tour de force. This factor appears naturally, however, in the tedious integration of the second-moment integral. The writer has not been able either to prove or disprove its correctness as presented, but only that its general form is correct; it must be the square root of a homogeneous function of b and g of degree zero and independent of the standard deviation. The particular form adopted is one that makes the curve converge to the normal in the limit as the end-points recede indefinitely. This is not sufficient to determine it uniquely, of course, and the point needs careful investigation. The writer believes that the difficulties involved should not be side-stepped.

In any problem that is analyzed by statistical methods there may exist certain legitimate transformations of the variate which it may be convenient, or even necessary, to make at the outset. If, for instance, two varying quantities, A and B, bear a definite relation to each other, then, if the variation in A is known, that in B may be inferred. The variation in B may be considered as a transformation of that in A. Such transformations will not preserve frequencies in general, however. Under these conditions the question arises as to what are the quantities best suited to play the role of elements randomly distributed.

To illustrate the point let us take Mr. Kimball's observation that "one would not expect the normal law to apply directly to precipitation-data, for this would imply that the probability of an inch of rain in one day would be the same whether that were the first inch, the second inch, or the third inch of rain which had fallen that day." This is true, of course, if one is concerned with the rainfall-record of one particular station, but the force of the objection disappears when one considers the random elements as belonging to the storm as a whole, which may embrace an area of several thousand square miles; it is storms as a whole that determine the character of a flood. Furthermore, given the storm, the ensuing flood may be approximately constructed, as has been shown by Sherman, Bernard, and others. If the frequency-distribution is compounded from data pertaining to storm-formations, intensity-patterns, paths, velocities, and so on, then from this distribution it would be possible to construct the flood-frequency distribution at a recording station. Obviously in transforming from one distribution to another frequencies would not be preserved, for a given storm may produce quite different floods at different times. If we take the storm itself as having the randomly distributed elements, then flood-peaks without reference to "preparation" of the ground are not the elements best suited for a statistical analysis of the problem.

In this connection the writer would like to suggest that the hydrograph of a stream at a station yields an index of drainage-area preparation through its non-periodic symptomatic trend and its seasonal variations [see A. Fisher, A brief note on seasonal variations, J. Accountancy, September, 1937; also letter by Slade in November issue of 1937], since prolonged high stages necessarily imply well-prepared drainage-areas. A standard of preparation could be adopted and the distribution of variations from normality inferred from the hydrograph. The transformed flood-data would then be the ratios of peaks to the product of the symptomatic trend and the seasonal curve instead of the ratios to the long-term mean. In this way an abnormally high peak would appear as a chance combination of storm and "preparation," neither of which need be abnormally rare.

Finally, a question arises regarding the testing for goodness of fit. To illustrate this vexatious point let us assume that we have the record of a stream over a 2000-year period. The frequency-distribution of floods in this long record would be what we seek in smoothing out a short record. Let us take for definiteness the 100-year flood, which is defined as the flood which, on the average, will be equalled or exceeded once in a 100 years. In our record there would be about 20 of these floods, but they would not be evenly spaced. In fact, it is conceivable that two would occur in some 50-year period, whereas there might be a 150-year stretch without one. If from this record we take a 50-consecutive-year sample at random it may include one or more or none of the 100-year floods. Its flood-frequency distribution would almost certainly not coincide with that of the long record; but, on the other hand, the probability that the two distributions be found entirely dissimilar would also be exceedingly small. A curve that fits the short record too well would thus seem to be as ill-suited to represent the long record as one that fits too poorly. The object in smoothing statistical data, then, is not to get a good fit, but to get the best compatible with probability. When we compute the first two moments of the sample, we have done about all that has statistical significance. Beyond this the small sample would seem to be a poor standard by which to measure the goodness of fit of the curve which we hope reproduces the distribution of the long record.

A matter of extreme importance is the hydrological study of conditions that lead to the maximum flood. If we are able to make a fair estimate of this maximum, then the best distribution compatible with probability is that which has the mean and standard deviation of the sample and which has this estimated maximum flood as its upper limit, regardless of the fit of the curve.

THORNDIKE SAVILLE (New York University, New York, N. Y.)--This paper presents an exposition by Mr. Kimball of a new approach to statistical analysis of hydrological phenomena which he calls the "Modified Slade method," since certain of the basic procedures were advanced in earlier papers by Professor J. J. Slade. The literature on the subject has become quite extensive, and none of the methods of attack proposed by various authors can be regarded yet as either founded upon strictly logical philosophical premises or as giving results substantiated by the conformity of more extensive hydrological records to the prognosticated frequency of occurrence of events based upon the statistical procedures recommended by the various authors.

Each new method, such as that of Mr. Kimball, presents some advance in more logical approaches or sounder mathematical and philosophical attack, and is therefore to be welcomed as stimulating necessary criticism and discussion which may ultimately lead to the formulation of procedures which may meet the real test of any statistical method, namely, the concurrence of predicted frequencies with those actually observed over a long period of record.

The general limitations and defects in methods heretofore proposed have been quite extensively treated in papers by Slade, Horton, and Saville in United States Geological Survey Water-Supply Paper 771, published in 1936. There seems no reason to repeat here the comments presented in those papers, but they should be reviewed in connection with Mr. Kimball's proposals.

Mr. Kimball's method appears to mark an advance in the Slade method, in that he treats of an exponential index in a frequency-formula which seems to follow a normal probability-distribution better than the functions of the observed data which were used by Slade for a similar purpose. Slade's method has the advantage of using functions having a definite and direct relation to the observed data, and thus carrying perhaps greater physical significance to that large majority who use statistical procedures rather blindly, not concerning themselves with the niceties of the philosophical or mathematical premises upon which a given method is predicated. However, Slade has himself recognized the limitations of his method, and both he and Arne Fisher have proposed procedures differing from those of Mr. Kimball which may produce still greater improvements in statistical hydrology.

It seems to the writer that there are a number of fundamental criteria which must be met before a given statistical procedure can be safely advanced as a general working tool for the analsis of hydrological phenomena. Among these are:

(1) The philosophical concepts must be in accord with known hydrological and meteorological behavior. For instance, the theory must be predicated upon finite upper and lower limits; it must be sufficiently flexible to fit both normal and skew-distributions; it must be based on much more complex premises than a merely fortuitous occurrence of events; it must recognize that the theory of sampling used is justified by the character of the hydrological data; it must clearly specify if it is applicable only to a homogeneous time series, etc.
(2) The effect of seasonal and secular occurrences must be recognized.
(3) The effect of changes in land-condition and use must be given consideration.
(4) The presence or absence of concurrent meteorological and soil-conditions must be weighed.
(5) Strictly fortuitous factors must be included.
(6) Areal limitations must be evaluated.

The final test of any statistical method is the reliability of prognostications made by it as related to the actual occurrence of future events not included in the data used in the analysis. Thus, applying the method to portions of a long-term record should give results which are in consonance with the entire record. The writer has yet to see any method which meets this test and which is at the same time applicable to several types of hydrological data. For instance, the floods of 1935 and 1936 in New England and in the Mississippi Basin changed greatly the frequency-estimates based on quite long records prior to those events.

The writer believes it unfortunate that so many statistical procedures have been advanced as satisfactory merely because the curves resulting from the application of the methods fit fairly well the plot of recorded observations. It is possible to fit a curve to almost any kind of data, but the validity of extrapolation from this curve to future events has thus far rarely been justified. The writer believes that in developing new statistical procedures, application to data of temperature and precipitation should be explored first, as such data are far less complex in the factors affecting their frequency of occurrence than is the case of stream-flow or flood-flows. To proceed from the simple to the complex is safe practice, and should be more frequently observed.

The writer believes that Mr. Kimball's proposals constitute an interesting development in statistical procedures in hydrology, but that they should be subject to more thorough discussion and test before they are proposed as a useful adjunct to solving practical problems. It is greatly to be hoped that the Section of Hydrology of the American Geophysical Union or some other agency may undertake a concerted attack on the general problems of the test and appraisal of all present methods of this character. Such a study is needed to bring to a focus on this important problem the coordinated knowledge and experience of hydrologists, meteorologists, and mathematicians. This type of investigation lends itself well to group-effort, and one of the major proposals of the Special Advisory Committee on Hydrological Data of the Water Resources Committee contemplates just such an undertaking. No subject in hydrology is more in need of impartial study, and out of a well-planned investigation utilizing existing knowledge and experience we may expect to materially improve a most important approach to the solution of many hydrological problems.

BRADFORD F. KIMBALL (May 21, 1938)--The interesting discussion of my paper by Dean Saville and Professor Slade introduces several thought-provoking points. Some of these points cannot be adequately dealt with until more investigation has been performed.

Dean Saville states that he has yet to see any method which gives prognostications that are not decidedly out of line with what actually occurs (or words to that effect). This, of course, would at the outset consign any method which pretends to be statistically sound to the limbo of statistical perdition. Accordingly, this objection had better be answered with some care.

The determination of the forecasting-curve rests upon three logical premises:

(1) There exist upper and lower finite limits of flood-flow or precipitation.
(2) The normal probability-distribution must be transformed so that the variable is confined between these limits in order to give a true description of the chance-behavior of the phenomena studied.
(3) The transformation $u = c \log [(b + x)/(g - x)]$ is the logical form of such transformation to use.

At the present stage of knowledge, the upper and lower limits are determined by a pseudo-statistical procedure. It is this point which especially needs further attack from both the statistical and hydrological angle. It is not possible at the present time to assign any statistical formula to the determination of the "probable error" in the determination of these limits. The writer admits accordingly that his attempt to determine these limits is a pseudo-statistical procedure, since it cannot be based on clean-cut logical reasoning. However, as most advances in science are made by comparing theory to actual occurrence, the method of determining the limits in such a way that the theoretical distribution will best fit the samples available, would seem to have considerable value, until some other more accurate method is known for doing this. The fact that when observational data are plotted on log probability paper for a long period of years the points tend to produce a curve which flattens off at the upper end, the writer believes, is sound statistical evidence that an upper finite limit should be assumed. (This is the essential departure from the method suggested by Arne Fisher [7, p. 76] which involves no upper limit.)

The reason for choosing the transforming relation $u = c \log [(b + x)/(g - x)]$ is discussed at some length in the introduction of my paper and need not be repeated here.

Having fixed on upper and lower limits by a sort of trial-and-error process, mixed with direct deduction from the hydrological problem attacked, and having decided that there is some logical basis for the use of the transforming function $u = c \log [(b + x)/(g - x)]$, it will be found in many cases that the "exponential index" u (see Introduction) behaves *very* much like a normally distributed variate. It is then proper to expect that the usual formulas for the probable error of the mean, and standard deviation of this variate will indicate how far off the determined values of these statistical parameters are from their true values (for the upper and lower limits chosen) *due to sampling errors*. Professor Slade, and probably Arne Fisher, will object here that we are dealing with a transformed variate and hence the formulas are not directly applicable. My answer to this is that *given any homogeneous series of measurements of natural phenomena, it can always be considered as a set of transformed values of some other measure of these phenomena*. Furthermore in this case the direct measurements are obviously *not* normally distributed. Hence to presume that the mean value which is found by making the first moment zero--analogous to finding center of gravity of physical object--gives the most probable value is fallacious, since a deviation to the one side of the mean would involve a different probability of occurrence than a like deviation on the other side of this value. The idea of "mean value" is closely associated--almost intuitively--with "most-probable value." This is the reason why in most treatises on flood and precipitation the "mean" flood or precipitation is so often used as a basis for comparison. From *a priori* reasoning, it would seem more sound when a series of data is known to be not normally distributed, to take the *median* as the most probable value rather than the mean. (For example, in vital statistics, median size of family rather than mean size of family gives a truer picture of most probable size of family, since the distribution is very skew, and the mean value gives undue weight to large families.)

Similarly the statistical significance of the standard deviation is that for series that have some approximation to normality, it gives a measure of probable deviation from the most probable value. Thus for such a series if $\sigma = 2$, we can estimate that the probability that a random element of the series will deviate by as much as two from its most probable value is about three out of ten. If a series is known not to be normal this significance is lost, and σ ceases to have any meaning other than that it is the notation for root-mean-square deviation from the arithmetic mean. To assume that the σ of the directly measured series is of more statistical importance than that of the transformed normal series is to the writer's mind an example of an unfounded loathness to drop the use of a mathematical and statistical symbol after it has ceased to possess its original meaning.

The writer thus finds that it is sound procedure to apply the usual formulas for the probable error of the mean and the standard deviation to the sampling errors in the constants l_0 and k of the transformed series

$$u = c \left\{ \log_{10}[(b + x)/(g - x)] - l_0 \right\},$$

where $k = 1/c$ = standard deviation of $\log [(b + x)/(g - x)]$ from its mean value l_0; if the upper and lower limits g and -b are held constant. These probable-error formulas are $PE(l_0) = (2/3)\, k/\sqrt{n - 1}$ and $PE(k) = (2/3)\, k/\sqrt{2(n - 1)}$.

In order to answer Dean Saville's basic criticism, let us apply these formulas to the acknowledgedly imperfect series of annual flood-peaks of the Hudson at Mechanicville--imperfect because of effect of Saratoga Flood-Control Reservoir, built after flood of 1913, on the homogeneity of the series. Lack of time has prevented me from working up a better example. The 49-year series of annual flood-peaks from 1888 to 1936 were analyzed by the writer's method and an upper limit of ten times the mean flood, and lower limit of 0.35 times the mean, were assigned. (The writer ought here to apologize for using "mean flood" as the unit of comparison in the light of remarks made above; however, the above analysis was carried out before the preference for the use of the median became apparent. He does wish that some one would start using the median rather than the mean as the basis of comparison.) A similar analysis was carried out with the flood of 1913 (2.65 × mean flood) omitted. This was the only flood of more than twice the mean flood in the series. Substantially the same upper and lower limits were assigned--but not with malice of forethought--simply on the evidence of the behavior of the depleted series (The lower limit was reduced from 15,000 cfs to 14,300 cfs).

Now let us for the sake of the argument assume that these upper and lower limits are correct. Then the principal sources of error are the sampling errors of the remaining two parameters l_0 and k. Consider the estimates of the 1000-year flood obtained from the two series. The 49-year series gives 2.89 times the mean flood, and the depleted 48-year series gives 2.58 times its mean, or 2.49 times the mean of 49-year series. Is this difference between 2.89 and 2.49 compatible with probable sampling errors of k and l_0? Considering the complete series, the values of k and l_0 to two decimal places are $l_0 = -1.20$, $k = 0.24$, and $n = 49$, with $PE(l_0) = 0.023$ and $PE(k) = 0.016$. Calculation of 1000-year flood may be considered in the three stages, namely, (1) $3.09 \times k + l_0 = \log z$, (2) $g - x = (b + g)/(z + 1)$, and (3) $x = g - (g - x)$. (The denominator of fraction in (2) is z plus one, not z plus ell; numerator is the difference between the magnitude of the limits since b is negative or zero.) From these formulas we find that if l_0 varies from $(-1.2 - 0.023)$ to $(-1.2 + 0.023)$, and k from $(0.24 - 0.016)$ to $(0.24 + 0.016)$, the extreme values of l_0 and k produce extreme values of x at 3.16 and 2.54. Thus there is a chance of 50 to 50 that sampling errors in determining k and l_0 will cause x to vary between 3.16 and 2.54 for prognostication of 1000-year flood. Thus the value 2.49 determined *without* the aid of the unusual flood of 1913 is not much more of a variation from the estimate than would be expected in half the samples of this size drawn from an infinite series of data (using same upper and lower limits).

Since the flood of 1913 was quite an unusual one, the best estimate of the 1000-year flood is probably obtained from a curve somewhat below the picked curve of the 49-year record. The 1000-year flood referred to here is of course the flood of expectancy of 0.1 per cent. The judgment as to what flood one ought to allow for as most probable to be reached and not exceeded in the *next* thousand years should of course be estimated from the point on the curve corresponding to an expectancy of 0.05 of one per cent (as discussed in Section VII), and its probable range of error of estimate due to sampling errors in k and l_0 should be calculated accordingly.

It is an interesting fact that in this particular series, if the estimate of the ultimate maximum limit is raised from 10 to 15, the prognostication of the 0.1 per cent flood is found to be very little changed. For the other long record of a river in New York State--the 63-year record of the Susquehanna--this same statement is true. In other words, for the two records extending over half a century for rivers in New York State, an upper limit of ten gives a fairly good fit, and a variation of four or five units above this value does not materially affect the estimation of floods with as low an expectancy as 0.1 of one per cent. The lower limits were taken in the neighborhood of 0.35 for the Hudson and 0.4 for the Susquehanna. A variation in these limits causes more disturbance (see Fig. 3). In the case of the Tennessee and Kanawha rivers, lower limits near zero, and upper limits in the neighborhood of three times the mean flood prove more satisfactory. In such cases, with the span between the limits relatively small, the sampling errors in k and l_0 have much less effect upon the estimation of unusual floods. Indeed for Curve III of the record for the Tennessee River, probable errors in k and l_0 cause the estimation of the 0.1 per cent flood to vary only between 2.01 and 1.85, with best estimation at 1.93. Here, by far the greater source of error lies in the choice of upper and lower limits (see Fig. 1). For records of much less than 50 years in length, the estimation of upper and lower limits by the methods of this paper appears to be much less satisfactory.

Thus when all is said and done, if a more satisfactory determination of upper and lower limits can be made by comparing records of watersheds and precipitation-areas of similar characteristics, very great improvements upon estimates of expectancies of large floods and storms should be possible. Sampling errors in k and l_0 can at present be measured, and will be reduced as time goes on.

It is the writer's hunch--perhaps too optimistic--that the constant k, being in the nature of a "natural constant" (see Introduction), may be found to have a relationship to the span between the maximum and minimum limits, which it will be possible to interpret in terms of the hydrological characteristics of the particular region studied.

In dealing with Dean Saville's basic criticism, I believe I have commented upon several of the points raised by Professor Slade. Although in the transformed series, it is true that upper extremes are "more probable" than they are in the case of a direct analysis of the raw data-series, on the other hand, lower extremes are "less probable" in the transformed series than in the analysis of the raw data. This is as it should be since the distribution is skew, with the median and mode to the left of the mean. If the probability of the flood of 1913 is computed directly from the raw data-series of the 49-year record of the Hudson River, one finds a deviation of 1.65 and a standard deviation of 0.38. Thus the deviation in terms of σ is 4.35σ, which incurs a probability of occurrence of about 0.001 of one per cent, or once in 100,000 years. Surely the flood of 1913 deserves a higher expectancy than this.

In this same connection, I do think that a few extreme observations out of line with the rest should not receive too much weight in the determination of upper and lower limits. This should be guarded against in placing too much confidence upon either the Van Uven test or my own test, where the position of one or two points out of line with the others serves to indicate an upper or lower limit which does not seem sensible. This is a weakness which it will be hard to avoid until there is more consistent information as to what values these limits should have.

Professor Slade has introduced an important point in stating that a stream-flood when brought into a statistical series should when possible have the seasonal factor separated from what he calls the non-periodic symptomatic trend. Any work which can be done to separate these factors would undoubtedly be valuable.

In conclusion I should like to make the following suggestion as to a possible method for the better determination of upper and lower limits from records now available: For what appears to be an exceptionally reliable record of precipitation or flood-flow, of over 50 years in length, let the mean daily precipitation or flood-flow be scheduled, using the same day each year in sequence, thereby obtaining many separate series from one record. Subject such series to a determination of maximum and minimum limits by the Modified Slade Method.

In this way the following advantages will be obtained:

(1) The reliability of the conclusions will be increased, because several series are used where only one series was used previously.
(2) More light will be thrown upon the hydrological meaning of the upper and lower limits.
(3) Such series would be expected to behave less erratically than series made up only of "peaks."
(4) Information will be gained concerning the seasonal characteristics of the phenomena analyzed.

CONTRIBUTIONS OF THE WATER RESOURCES COMMITTEE TO HYDROLOGICAL INVESTIGATIONS

Thorndike Saville

Some 400 years ago a philosopher named Thomas Tusser wrote "A description of the properties of the wind," and said:

> "Except wind stands as it never stood
> It is an ill wind turns none to good."

Out of the metaphorical winds of economic adversity and the actual winds bringing the floods and droughts of the past five years there has been blown a great store of new facts and knowledge into the armory of the hydrologist, there to be sorted and classified and made available for his unceasing struggle to perfect his knowledge of water behavior; the science of hydrology.

Hydrologic studies sponsored by the Mississippi Valley Committee--The Public Works Administration was one of the most important emergency agencies of the Federal Government created to help deal with the depression. Within this agency the Mississippi Valley Committee was set up to render a special report upon the water-problems of that vast area within the United States. This Committee soon came to the conclusion that wise planning of the enormously expensive water-control projects proposed for the region could not be successfully or economically prosecuted without more fundamental hydrologic studies than were then available. One of its last acts, in 1934, was to make available a considerable sum to the United States Geological Survey for special compilations and studies. Two reports of fundamental character were issued in 1936 as a result of these investigations. One was "Floods in the United States", issued as Water-Supply Paper 771. In the preparation of this report valuable assistance was received from a special committee of the American Society of Civil Engineers, as well as from numerous individual experts and public agencies. The other was entitled "Rainfall and runoff in the United States", issued as Water-Supply Paper 772. In the preparation of this report guidance was received from a special committee of the Section of Hydrology of the American Geophysical Union.

Inventory of water-resources of the United States--Prior to the issuance of these two reports the Mississippi Valley Committee had become the Water Planning Committee of the National Resources Board. This Water Planning Committee not only continued to assist in the execution of the two reports mentioned, but undertook a special investigation long needed by hydrologists in this country, namely, the preparation of an inventory of the water-resources of the United States. This was compiled by regions, each major drainage-basin of the country being assigned to an expert water-consultant drawn in general from the ranks of private consulting engineers. Inevitably much of the basic data involved was contributed by various Federal and State agencies. The regional reports were separately published, and they were abstracted to form the general inventory of the status of hydrological knowledge of the water-resources of the United States which appeared as the report of the Water Planning Committee in 1935. It is greatly to be hoped that this inventory may be revised in the light of new data at least once each ten years.

Report on deficiencies in hydrologic data--Meanwhile, the National Resources Board had become the National Resources Committee and the Water Planning Committee became the Water Resources Committee. The present Water Resources Committee of the National Resources Committee is therefore the direct successor first of the Mississippi Valley Committee and subsequently of the Water Planning Committee. (The Water Resources Committee is composed as follows: Abel Wolman, Chairman, State Sanitary Engineer of Maryland and Professor of Sanitary Engineering, Johns Hopkins University; H. H. Barrows, Professor of Geography, University of Chicago; Bushrod W. Allin, Land Use Coordination Office, Department of Agriculture; M. S. Eisenhower, Coordinator of Land Use Planning; N. C. Grover, Chief Hydraulic Engineer, U. S. Geological Survey; Edward Hyatt, State Engineer of California; R. B. McWhorter, Chief Engineer, Federal Power Commission; J. C. Page, Commissioner of Reclamation; Thorndike Saville, Dean, College of Engineering, New York University; Julian L. Schley, Chief of Engineers, United States Army; R. E. Tarbett, Chief Engineer, United States Public Health Service; Sherman Woodward, Chief Water Engineer, Tennessee Valley Authority.) The inventory of the water-resources of the United States disclosed, as might be expected, many serious deficiencies in basic hydrologic data. Such deficiencies had been long apparent in specific areas, but the inventory brought to focus the needs for the country as a whole. To canvass these needs so that an orderly program for the collection and publication of such vitally essential data might be prepared for the Congress, the Water Resources Committee created a Special Advisory Committee on Hydrologic Data.

The Advisory Committee includes representatives of the Weather Bureau, Geological Survey, Corps of Engineers, Soil Conservation Service, Bureau of Agricultural Engineering, together with consulting engineers from various parts of the country.

This Advisory Committee considered first the improvement in standards and specifications for the collection and publication of hydrologic data, including the designation of Federal agencies which should be primarily responsible in different fields of collection and publication of data, the minimum requirements of training and experience for personnel, and standardized methods and procedures for both collection and publication of basic data.

Next, the Advisory Committee undertook a nation-wide canvass, with the assistance of Federal agencies, to determine the extent of deficiencies in hydrologic data and the means and cost of remedying them. The principle of complete establishment and maintenance of base-stations by the Federal Government at strategic locations was established, as well as that of Federal participation in secondary stations contingent upon matching of funds by State, local, and private agencies.

The final report, comprising both of the sections referred to above, was issued in May, 1936. It attempted to portray as dramatically as possible the past damages and possible future losses due to inadequacies of basic hydrologic data, and set forth a fundamental continuing program for their alleviation. The cost to the Federal Government was estimated at about $1,500,000 per year and to non-Federal cooperating agencies at about $400,000 per year, or a total of substantially $1,900,000 per year. This is insignificant insurance for a great nation to pay for adequate information for the controlling and safeguarding of its water-resources. The facts of this report and the program formulated therein should not be permitted to lie dormant. The members of this group are more cognizant than any others of the vital need for increased basic hydrologic data, and a scientific program which has already been endorsed by you should not be left to lie on the shelf safely blanketed in an approving resolution.

Report on deficiencies in hydrologic research--One scientific inquiry begets another. So in the train of hydrologic interests and activities of the Water Resources Committee thus far described, there was added a further study as the result of those just mentioned. A survey of deficiencies in basic hydrologic data brought to light numerous suggestions for research, to apply the results of collection of data to the solution of some of the more controversial or little known phenomena of behavior of water, and the relations thereto of land-conditions and land-management practices. Therefore the Water Resources Committee authorized the Special Advisory Committee on Hydrologic Data to undertake a survey of deficiencies in hydrologic research. This inquiry has been under way for about a year, and a report is expected within the next few months. The Advisory Committee, in collating needs for hydrologic research, have followed their earlier practices of preparing preliminary topical discussions and submitting these for review and comment to numerous hydrologists throughout the country. The result has been the preparation of a report outlining specific types of research-projects in the different fields of hydrology, suggesting the purpose of the research, outlining general procedures for its conduct, giving a few fundamental bibliographical references, suggesting the types of agencies and institutions which can properly conduct the indicated project, and mentioning the principal public agencies which should be in a position to assist in the conduct of the project, by furnishing basic data, professional advice, or otherwise. The aim of the report is to stimulate the orderly prosecution of hydrologic research directed toward definite objectives regarded as in most immediate need of investigation.

Coordination of hydrologic research--Manifestly a report such as this cannot and should not attempt either to encompass all needed hydrologic research, nor to propose projects already under investigation by one or more competent agencies or individuals. Nonetheless, most of us realize that there are few research-projects in hydrology which do not of necessity overlap in one or more particulars. We are aware of large-scale and expensive research-projects of one or another agency, committee, or group which would be the better for a little coordination with somewhat different but allied research-projects being carried on by others. The Special Advisory Committee on Hydrologic Data felt strongly that coordination of hydrologic research, both that in progress and that to be undertaken, was highly desirable. It therefore recommended, and the Water Resources Committee endorsed, the institution of a Hydrologic Research Clearing House, a more or less informal activity to be undertaken by the Water Resources Committee. It is proposed to issue semiannual lists of active and proposed research-projects, to serve as a medium of information, and to furnish a desirable amount of coordination in hydrologic research-undertakings.

Bibliography of unpublished hydrologic data--In connection with the activities previously described, one of the outstanding discoveries was that a large amount of basic hydrologic data had been collected by various public and private agencies but was not generally available in published form, and to a great extent was quite useless because its existence was not known. The Water Resources Committee authorized a project for the investigation of the sources of unpublished hydrologic data, for its evaluation and classification, and for the compilation of bibliographical references to it. The results of this study will shortly be published as a Water-Supply Paper of the United States Geological Survey.

Hydrologic and hydraulic nomenclature--In connection with the hydrologic activities previously described, the Special Advisory Committee was confronted with numerous problems of definition and nomenclature relating to terms used in hydrology. In undertaking to prepare a more or less standardized list of such terms the Committee found that the American Society of Civil Engineers, the American Water-Works Association, and the American Public Health Association had formed special committees to deal with nomenclature as affecting water-works and sewerage practice. It seemed wise to consolidate the work of all of these committees, and a Joint Committee of the three Societies was formed with the same Chairman as that of the Special Committee on Hydrologic Data. A comprehensive effort is under way, supported by the Water Resources Committee, to prepare a general list of terms and definitions dealing with hydrology and with hydraulic and sanitary engineering.

Miscellaneous hydrological activities--The activities of the Water Resources Committee and its predecessors in the field of hydrology thus far described have been to a considerable extent assigned as a major responsibility of the writer, and have therefore been outlined here at some length. The Water Resources Committee has also promoted other hydrological activities of a less scientific but more practical character. These are briefly reviewed below.

International Bibliography of Hydrology--At the request of, and in cooperation with the Section of Hydrology of the American Geophysical Union the Water Resources Committee secured funds through the National Resources Committee to participate in the cost of publishing the United States Section of the International Bibliography of Hydrology for 1936.

Report on "low dams"--This Manual, to be issued shortly, contains chapters dealing with new procedures for estimating floods from small watersheds.

Report on influence of vegetal cover--Another sub-committee has recently undertaken to compile a resumé of the relationships between vegetal cover and water-behavior. An abstract of the findings probably will appear in a forthcoming report of the Water Resources Committee.

Report on Red River of the North--An extensive report on comprehensive planning for use and control of water in this River basin included some special hydrologic studies related to evaporation and its effect upon water storage projects.

Report on Upper Rio Grande--This monumental report, shortly to be released, is based largely upon intensive hydrological studies sponsored by the Water Resources Committee in cooperation with Federal and State agencies.

Summary--The members of the Water Resources Committee and their predecessors, organized to coordinate Federal activities in the field of control and use of water, have earnestly promoted the development of all measures to ensure adequate collection and publication of basic data, to stimulate research, and to enhance the application of the science of hydrology to water-undertakings. In all but two of the activities mentioned herein, the policy has been to constitute a sub-committee or group composed of both Federal and non-Federal experts to formulate reports for the Water Resources Committee. Specialists from private practice have been employed by these committees for particular studies. This policy has resulted in bringing to a focus divergent points of view, and the result has in all such cases thoroughly justified the premise that bureaucrats and private practitioners are all alike scientists; that the quest of truth and search for sound methods of advancing the science of hydrology present a meeting ground upon which diverse groups may work harmoniously and effectively toward a common goal. It is the belief of the writer that only by such deliberate and planned cooperation between public and private interests can hydrology consolidate those gains it has made during the past five years and move forward securely into the blaze of a great new science, the dawn of which now appears clearly visible.

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THE DISTRIBUTION OF THE AVERAGE DEPTH OF SNOW-ON-GROUND IN NEW YORK AND NEW ENGLAND: METHOD OF STUDY

Robert G. Stone

I--On the study of snow-cover depth as a climatic phenomenon

These brief remarks are not intended to reveal the crystallized results of a completed investigation but rather to call attention to the subject and methods of a study underway. As the final report will not be published for some time and the interest in the subject is growing the hope is cherished that some readers may be interested to plan or start similar studies in other sections of the country. The International Commission of Snow meets here in September 1939 and it would be an appropriate occasion for the presentation of a comprehensive series of snow-maps for the United States.

The study of snow-cover depth as a climatic phenomenon has been practically neglected in this country [see 34, 35, and 38 under "References" at end of paper], and indeed nowhere pursued in any great detail outside of Germany, Scandinavia, and the Alps [1-48 constitute a fairly complete bibliography on the distribution of snow-cover depth and duration in middle latitudes in meteorological literature]. Snow-surveying is but a very special phase of snow-cover. The presumption that the usual records of snowfall-precipitation indicate the snow-cover in any clear-cut way is without foundation where the adequate solution of specific problems is involved. Many factors affect snow-cover without any relation to snowfall at all, and even the ratio of snowfall to snow-cover varies radically from storm to storm. The annual winter snow-covers of middle latitudes are of interest to ecologists, agriculturists, engineers, sportsmen, public-works, geographers, and meteorologists--and for many different reasons, which it is not necessary to discuss here. But it is important to note that these various needs cannot be met by any single simple index of the snow-cover [32]. In one case it is the density or water-content [3, 24, 28], in another the depth [3, 29, 35, 37, 38], in still another the duration or mere presence of snow lying on the ground [10, 15, 21, 31, 33], or it may be the character of the snow-surface [35, 39] and the interior structure of the snowbed [3, 4, 24, 28], which is most significant. The hydrologists have already taken care of their peculiar needs to a great extent by special observations of the depth and water-content, but unfortunately such data are not in a form nor from the kind of station-network which serve other purposes very well. For widest general utility we need fairly detailed maps of the average depth of snow-cover over the whole country (or those parts of

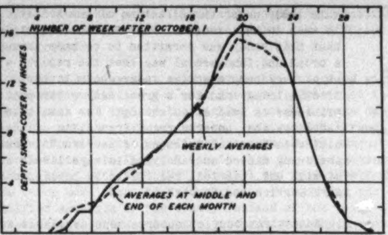

FIG. 1—AVERAGE DEPTH OF SNOW ON GROUND, BETHLEHEM, NEW HAMPSHIRE (BASED ON 40 YEARS' RECORD)

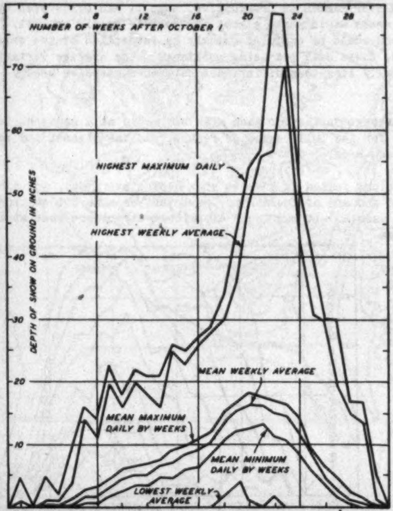

FIG. 2—SNOW-COVER DETERMINED FROM 40 YEARS' RECORD, BETHLEHEM, NEW HAMPSHIRE (LOWEST MAXIMUM DAILY DEPTH WEEK BY WEEK WAS IN ALL 40 YEARS NEVER ABOVE ZERO INCHES)

it receiving snow of any consequence) for frequent standard dates through the season [3, 38]. For further interpretation of such data, the scatter of the depths in different years about the average depth is necessary, and also any secular or cyclic trends. From such a body of material an almost infinite amount of further information can be obtained by mere manipulation of the data according to the purpose at hand. Except for Finland, Germany, East Baltic, the Alps, and Japan, such material has not been readily available to any extent [3, 20, 26, 38, 4, 10, 12, 13, 37, 38, 41-48].

II--The National Park Service study of snow-depth in New York and New England: Progress-report and methodology

The Branch of Recreational Planning and State Cooperation of the National Park Service, in cooperation with other Federal and State agencies, has since 1933 been undertaking surveys of the natural and other conditions favorable for recreational development in certain sections of the country, with view to guiding future public development for recreation and also for directing such projects already underway with the aid of CCC funds as Emergency Conservation Work [40]. This was authorized in a more systematic and correlated way by Congress in June 1936 by "An act to authorize a study of the park, parkway, and recreational-area programs in the United States...." [40].

Owing to the tremendous growth of skiing as a popular sport in New York and New England, among other regions, it was only natural that this survey should for those States include the collection of climatic data on snow [40]. Accordingly, two student technicians, Miss Sylvia Govern of Brooklyn, and J. H. Conover of the Blue Hill Observatory, were employed for a time to collect and work up such

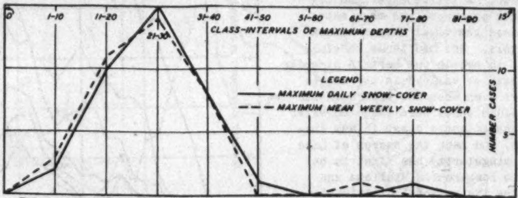

FIG. 3—FREQUENCY-DISTRIBUTION OF THE SEASONAL MAXIMA BY 10-INCH CLASSES OF DAILY AND WEEKLY AVERAGE DEPTHS OF SNOW ON GROUND, 40 YEARS' RECORD, BETHLEHEM, NEW HAMPSHIRE

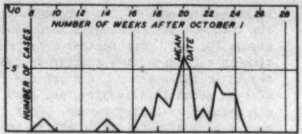

FIG. 4—FREQUENCY-DISTRIBUTION OF DATES OF HIGHEST WEEKLY AVERAGE DEPTHS OF SNOW ON GROUND FOR 40 YEARS, BETHLEHEM, NEW HAMPSHIRE

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data [38], under the direction of the writer. It is much to the credit of Park Service officials that this study was permitted to be made in such a broad and fundamental way that the results will be of very comprehensive value, both to the science of climatology and for a great many other practical problems, as well as to skiing. The data have been tabulated and, under a grant from gifts of the Associates in Physical Sciences of Harvard University, are being mapped and analyzed in ways likely to be of wide use, the full results to be published by the Park Service and Harvard.

The procedure has been as follows: The only consistent body of observations available was found to be the records of the daily depth of snow on ground made at nearly all cooperating stations of the Weather Bureau since sometime during the 1890's. The amount of labor in computing means for each calendar day through the winter seemed so prohibitive, that it was discovered by sampling that the averages by successive weeks would serve almost any purpose nearly as well. Usually the average for each week of record could be computed quickly by inspection of the original forms on which the observations are filed, these data not being published. The average variation from day to day during the week was generally less than differences between successive weekly averages (see Fig. 2).

From the tabulations of the weekly average depths for each week of record at a station, it is easy to compute the mean for each week for the full period of record, and the highest and lowest values can also be readily read off for each week.

Unfortunately quite a few of the stations failed to observe snow-depths every day, and some of them kept only the depths at the middle and end of the month. Nevertheless some 300 stations in New York and New England together were usable. It was found after some experience that at any one station the curve of snow-depth through the year drawn from weekly averages and the curve drawn from twice-monthly averages were surprisingly close, as Figure 1 shows. In view of this, to save time, only twice-monthly averages were computed for most stations, and it is felt that most of the latter can be taken as comparable and practically homogeneous with the weekly-average stations.

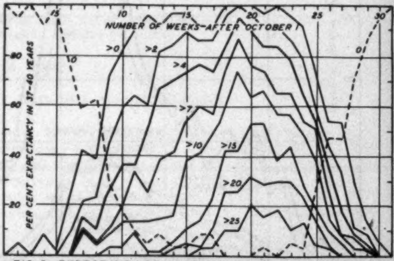

FIG. 5—EXPECTANCY OF SNOW ON GROUND, WEEKLY AVERAGE DEPTHS AT 6 P.M., BETHLEHEM, NEW HAMPSHIRE

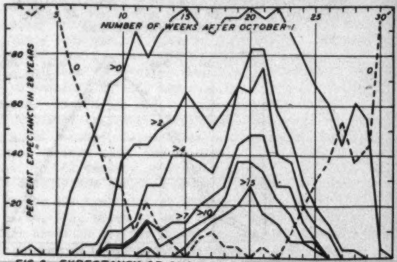

FIG. 6—EXPECTANCY OF SNOW ON GROUND, WEEKLY AVERAGE DEPTHS AT 8 P.M., FITCHBURG, NEW HAMPSHIRE

It was found that it takes about ten years to give a very stable mean, though obviously somewhat fewer years in case of weekly averages than with twice-monthly averages. But a record shorter than ten years and longer than five years can also be used with the longer records if the curve of mean depth through the season be smoothed by inspection and new twice-monthly values be read off from the smoothed curve. A little experimenting justified this procedure and thus materially increased the total number of stations available. One had to be careful, however, not to smooth out certain singular secondary humps or valleys in the curves which persist even when the record is for many years--such cases were found to exist only along the Atlantic coast in New England (Fig. 6) and near the shores of Lake Ontario. A singularity has first to be proved at the long-record stations and then from them its regional distribution delimited, before applying smoothing to any short-record stations that might also appear to have the same singularity. These singularities are a feature of the climate when they persist in a 20-, 30-, or 40-year record, and in Europe they have been demonstrated in very much longer records.

Figure 2 shows that even when we are concerned with the extreme values, the weekly average is probably sufficient for most purposes. Note that the curve of highest daily depth on record week by week and that of the highest weekly average depth on record week by week are strikingly parallel, and they average but two inches apart through the season, though somewhat more at peak-season. Thus the tables of weekly averages are adequate for studying the frequencies and extremes of snow-depth. In Figure 2 can be seen the mean maximum daily depths and mean minimum daily depths, week by week; note that they parallel the mean weekly-average curve rather well and do not depart from it by more than about 35 per cent even during the deepest half of the snow-season. This justifies analysis of the weekly instead of the daily averages, since it is obvious that a mean daily-average curve would rarely depart from the mean weekly curve by more than a third of the

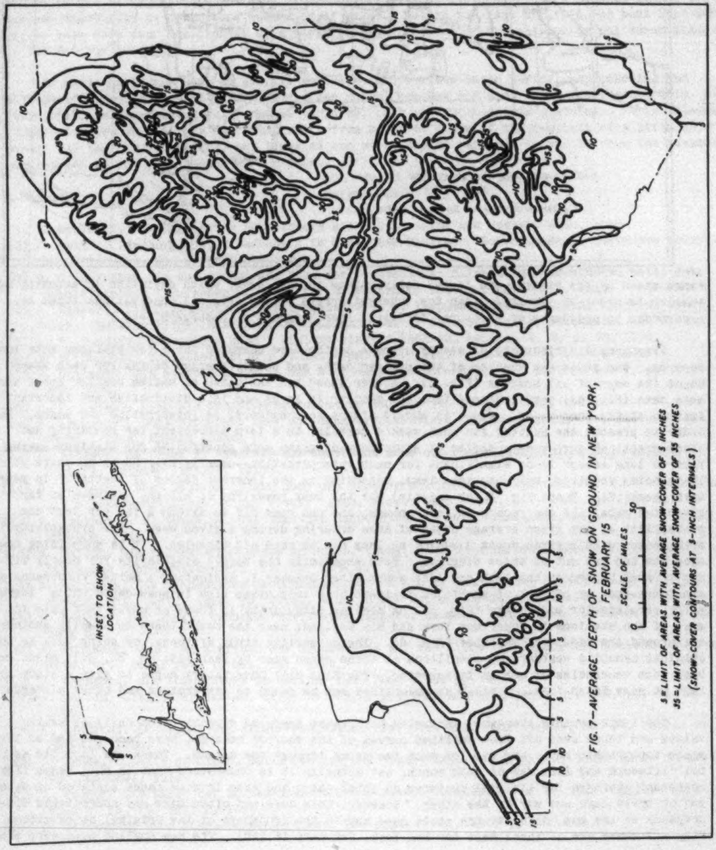

FIG. 7—AVERAGE DEPTH OF SNOW ON GROUND IN NEW YORK, FEBRUARY 15

5=LIMIT OF AREAS WITH AVERAGE SNOW-COVER OF 5 INCHES
35=LIMIT OF AREAS WITH AVERAGE SNOW-COVER OF 35 INCHES
(SNOW-COVER CONTOURS AT 5-INCH INTERVALS)

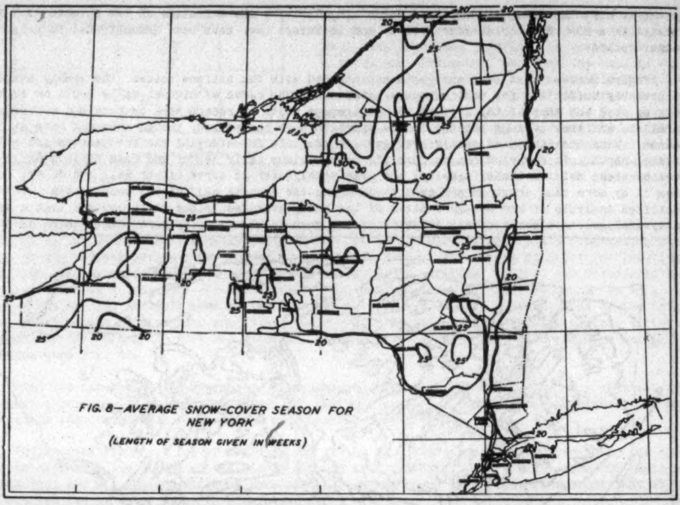

FIG. 8—AVERAGE SNOW-COVER SEASON FOR NEW YORK

(LENGTH OF SEASON GIVEN IN WEEKS)

range shown by the highest and lowest daily values in each week, which deviation is actually much smaller in order of magnitude than the inherent errors of the original observations (that is, errors due to accidents of exposure, drifting, poor judgment of observers, etc.)

Frequency distributions of weekly-average depths were computed for a few stations with long records. One shows the scatter of the maximum daily and weekly-average depths for each season about the mean of all seasons (Fig. 3); another shows how the dates of maxima scatter about the mean date (Fig. 4); both indicate that the scatter is large and is a distinctive and important feature of the snow-cover. Hence it should always be considered in interpreting the means. In order to present the scatter for each week's normals, in a form convenient for comparing and other practical purposes, a series of expectancy-diagrams were constructed for stations having records long enough to be significant for such interpretation--usually over 25 or 30 years at least being required, but sometimes less, depending on the inherent degree of scatter. In many ways these (Fig. 5 and Fig. 6, for example) are the most revealing of all the analyses so far given the data. If the record is long enough, one can read off to within a few per cent the probability of any given average depth of snow occuring during a given week. The probability of a season with a given depth lasting so long can be read off likewise. It is surprising how much one can get out of these diagrams. Note especially the marked singularity for nearly all depths that occurs on the 17th and 18th weeks after October 1, indicating a marked recurrence of midwinter-thaw or of lack of snowfall, followed by a very steep rise in snow-depth which thereafter persists for many weeks (Fig. 5). A similar singularity is found at about that date in most of the stations in upper New York and New England; near the coast there is usually another one around the middle of December (Fig. 6). The scientific study of these, by going back to the original synoptic weather-map conditions at those dates year by year [14, 27, 33, 36], might reveal some tendencies of use in forecasting. The Blue Hill Observatory hopes to give a study to them at some later time. Similar singularities can be found in temperature and other elements.

The twice-monthly averages, including both those computed directly from daily or weekly values and those read off from smoothed curves of the shorter records, have been plotted on large scale topographic maps, one map for each two weeks through the season. These are labelled as of the fifteenth and last day of each month, but actually it is understood that in some cases they represent averages for the week centered on those dates and even in some cases centered on dates two or three days one way or the other. However, this does not often make any appreciable discrepancy on the map, owing to the scale used and to the crudeness of the original observations. Figure 7 shows one of these maps for New York, February 15 [38]. The New England maps were drawn on a still larger scale and with even closer relation to the contours. The resulting pattern is regarded as about right if not looked at too minutely, but of course the isopleths should be taken more as representing lines of equal snow-depth than of actual depths in inches, since the undoubted influence of varying exposure of stations, drifting, etc., makes it hard to take the figures too literally. Like index-numbers that business statisticians use, the figures of snow-depth can be accepted for indicating the relative depths at different elevations and localities, that is, for comparisons in time and space. After working with so many snow-data it is difficult for the writer to conceive any closer approximation to the true general distribution of snow-cover than in this way, except for very limited areas where special needs warrant many stations and more careful observations. Even hydrological snow-surveys would have to run many more courses in a given region than customary at present, before they would provide material for a uniformly detailed snow-map. Also the stations in the courses would have to remain the same for at least ten years if the results are to be conveniently summarized. The snow-surveys, however, do not lend themselves to work with thin snow-covers nor to the conditions peculiar to the beginning and end of the snow-season in the East. Perhaps the many new observers of snow for winter-sport resorts can be organized eventually to contribute something of value. But in the last analysis the best hope for better snow-data over the country as a whole is by refinement of the climatological observations of the Weather Bureau.

In addition to the maps of average depth, which form the basic series, maps showing the average length of season with more than given depths of snow are also being constructed (Fig. 8). These are more directly useful for some purposes, such as winter-sports planning. It is intended finally to show on a single chart the shift from month to month of the isopleth of a given snow-depth, say the five-inch line, and likewise for other depths perhaps; this will show the advance and retreat of the snow-season at a glance.

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GLACIOLOGICAL RESEARCH CONTEMPLATED FOR AN EXPEDITION TO NORTH GREENLAND

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Introduction

Plans for an expedition have been drawn up by the author and approved by the American Geographical Society and the Danish Commission on Scientific Exploration in Greenland. The two most important programs of study contemplated are meteorological and glaciological. The latter is here presented with the hope that authoritative criticisms and suggestions may be offered. For the same reason the meteorological program is being presented to the Meteorological Section of the Union.

The proposed Expedition will establish its headquarters on the west coast of Greenland in approximate latitude 74° 30' (Fig. 1). This is just to the south of Melville Bay in an area that is familiar to the author, for he lived there two years with expeditions sponsored by the University of Michigan and Pan-American Airways.

This particular area offers remarkable opportunities to those who would study glaciation in its many aspects. Névé fields of recent origin, valley-glaciers both isolated from, and outlet from the inland-ice, and the great inland-ice itself are all present and easily accessible. This accessibility is probably greater here than in almost any other glacial region of the world.

The Expedition will have a staff of ten or twelve men. The headquarters will be established on one of the coastal islands lying at a distance of 20 to 30 miles from the ice-border. Work at this station, will be primarily meteorological. A second station, also to be maintained throughout the duration of the Expedition, will be erected at or on the border of the inland-ice. In addition to meteorological observations made synoptically with those of the headquarters camp, glaciological studies of the border region of the ice-cap will be carried on at this station.

In the spring and summer of the second season (1941), probably beginning in late April, two additional stations will be set up farther inland on the ice-cap. One of these will be at, or just above, the snow line, and the second in the central part of the ice-cap at about 76° north. The program of study at these stations will include both meteorological and glacio-meteorological work.

Further glaciological research will be carried on in the interior regions by a party of two men which will travel in motor-sleds of a new design. It is planned to run numerous traverses which will provide continuous topographic profiles over perhaps a third of the ice-cap. This will be sufficient for the construction of an accurate topographic map. In addition, geophysical determinations of ice-thickness will be made at as many different localities as possible.

The glaciological programs here proposed may be divided into three separate parts as follows: (1) Studies of glacial movement and ice-deformation in the border zone of the inland ice; (2) glacio-meteorological studies of growth and wastage of glaciers; (3) topographic and depth-measurements of the interior regions of the inland-ice.

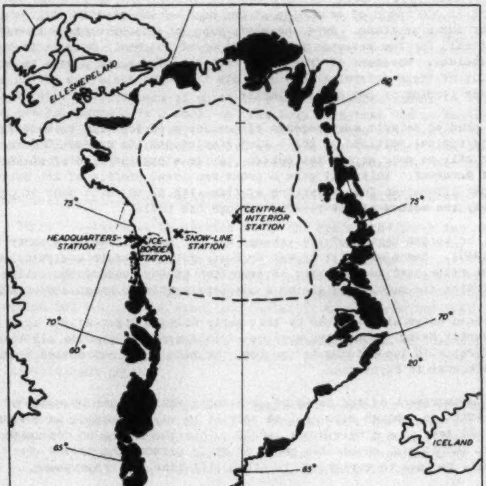

FIG. 1—THE GREENLAND ICE-CAP

■ EXPOSED LAND ALONG THE COAST

(DASHED LINE DELIMITS AREA TO BE SURVEYED BY MOTOR-SLED TRAVERSES)

Studies of glacier-movement and ice-deformation in the border-zone of the inland-ice

These studies will be the chief glaciological work of the men stationed at the ice-border camp, and will, so far as possible, be continued throughout a complete year.

The primary observations of glacial motion will be confined to one of the larger outlet-glaciers in the region, and will include not only the more rapidly moving lower part of the ice-tongue, but also the slowly moving feeder-area or basin of exudation at its head. Precise measurements will be made by triangulation with a Wild theodolite.

A row of metal stakes will be drilled into the ice of the rapidly moving tongue and triangulated on at frequent intervals through the year. Probably a weekly or ten-day schedule will prove most satisfactory. However, daily observations will be carried out for a week or more at a time whenever critical seasonal or cyclonic changes in weather may take place. For instance, such daily observations will be inaugurated whenever there is a sudden rise in temperature, and will be continued until the weather has again returned to something like normal. Likewise, daily observations will be carried through the first springs melts when cyclonic conditions bring the first periods of warm weather, and through the first periods of autumn freezing when water in the crevasses of the ice may promote movement by alternate freezing and thawing.

It is also planned to make even more intensive observations for periods of 24 to 48 hours at the end of each month or five-week period. These will be made in the manner used by R. P. Goldthwait in his Alaskan glacier-studies of 1934. He made precise determinations on a single point at intervals of 15 minutes for periods of a day or so, thus revealing diurnal variations in the glacial movement.

While certain of the stakes first set out will be left in the ice and followed as far as possible, it is believed that observations of more value will result if the main study of movement is confined to a single section across the glacier. To accomplish this the stakes will be frequently reset on the same section as the ice moves downstream carrying them away from it. This will eliminate variations in movement that would result from changes in the cross-section of flow.

The value of such a study as this is not in the determination of the rate of movement of any single glacier, but rather in the fact that it will show the complete cycles of seasonal and subseasonal changes that take place, and such changes will probably apply to other glaciers.

In order to make these observations through the dark months of the winter, small wind-proof lanterns will have to be mounted on the tops of the stakes for each course of observations. The stakes will be made of metal cut in four- or five-foot sections and threaded at the ends. Burial by snow will thus be avoided by screwing additional sections to the tops of the stakes as the drifts rise around them.

Determinations of movement in the basin of exudation at the head of the glacier will be made in the same manner along two or three sections. Here, however, such an intensive study as outlined above would not be practical, for the movement is known to be so extremely slow as to make frequently-determined data valueless. Movement of from a few feet to a few yards a year is all that may be expected. The value of these determinations will lie in their revelation of the distribution of ice-movement in the feeding of the outlet-glacier.

In connection with these studies seismic measurements of the depth of ice will be made in various cross sections and longitudinal sections of the outlet-glacier and its basin. One such section of particular interest will be made across the outlet-glacier along the line of stakes set for the intensive study of movement. This will give a cross-sectional profile of the bottom of the glacier, and, with proper allowances for variations of flow with depth, will make it possible to calculate approximately the volume of ice flowing through the outlet.

Other glacial studies of the border-region of the ice-cap will be carried out by a party of two men in the first summer (1940). These men will travel on foot, carrying their equipment on hand-sleds. Their work will be restricted to the basin of exudation at the head of the outlet-glacier on which the motion-studies are made, thus giving a completely unified program of research.

The depth-soundings mentioned above will be made by this party using a light-weight apparatus such as the one designed by Maurice Ewing. Since the surface of this border-region is all lower than the snow-line, and therefore solid ice extends to the top, the more rapid reflection method of sounding can be used in preference to refraction.

As far as possible direct measurement of the depth of crevassing will be made by means of a piano-wire sounding-line. In addition, enough rope will be carried to allow a man to be lowered into a crevasse to a depth of 500 feet on a single-block tackle. This can safely be done with a tackle weighing only 26 pounds, and it will enable the party to study alterations of ice with depth. Particular attention will be paid to variations in crystallization and temperature.

It is planned that during the first season a privately owned airplane will be flown into the region for the purpose of aiding the expedition with a program of aerial mapping which will cover this basin and adjacent areas of the coast. Both oblique and vertical photographs will be used, and a topographic map of the basin will be constructed from them. Such a map and the photographs will be of particular interest in showing the main lines of movement and disposition of crevasses. It may also be of value in leading to a solution of the nature and origin of the step-like terraces that lie at the interior borders of the basins. These steps, ascending toward the higher and more level interior plateau, have been described by Peary, Nansen, de Quervain, and others. Their origin has been thought by some to be due to a subsurface topographic control. Others consider them an unsolved problem, perhaps related to various characteristics of accumulation or motion of ice. At the head of the Cornell Glacier in the region where the present studies are contemplated, the writer crossed the border-zone four times in the summer of 1933. The terraces were particularly impressive, and seemed to the writer to be the result of "land-slides." Not only is their form, with back-tilted treads and frequent lakes, similar to typical land-slide terraces, but slides of this sort should be expected at the inner borders of such basins. The origin of the basins is by rapid ice-drainage through the outlet-valleys, the drainage taking place more rapidly than replenishment of ice from the interior. A basin is thus analagous to the depressed water-surface that occurs at the head of a sluice-way in a dam. The lowering of the surface of the basin and the fact that its ice is continually being drawn away from the main ice-mass behind, actually amounts to an undersapping of that main ice-mass. As a result typical landslides take place. It should be emphasized that this idea is rather a hunch than a hypothesis, but it is nevertheless deserving of consideration.

Study of deformation and movement of ice on a small scale is also contemplated. This is to be done by applying a modification of the Saunder-Schmidt method of petrofabric analysis. The modification of the method used by petrographers is necessary because the crystal-sizes in glacial-ice are too large for high-power magnification, and the birefringence of ice is of so low a value that interference-figures are nearly impossible. Orientation of the long axes of the crystals will have to be determined on slip-bands which correspond to the basal pinacoid, or possibly on "negative crystals" which can be developed in the plane of the basal pinacoid by penetration of ice with heat-rays. The instrument used will consist of a large inexpensively constructed universal stage, polarizers of polaroid, and a low-power microscope-tube. The cost of the whole thing is but a few dollars.

Studies of this sort will be made at the ice-border station during the winter. Oriented slabs of ice taken from positions within the glacier where stress-conditions are known will allow a determination of crystal-orientation in response to stress. Also, experimental slabs of glacier-ice will be deformed under pressure and the resulting crystal-changes studied. This should lead to a clear idea of the manner in which inter- and intra-crystalline displacements take place in the movement and deformation of ice.

Further significance of great importance attaches itself to this study because of the fact that ice is essentially a rock, and the only rock that can be easily handled and studied at a temperature close to its melting point where flowage and plastic deformation take place. It is more than probable that such studies as these may therefore lead toward the formulation of basic principles of deep-seated rock-deformation and numerous tectonic problems.

Glacio-meteorological studies of glacier-growth and wastage

These studies will be carried on by the men at the three ice-cap stations (the ice-border station, the snow-line station, and the central interior station), and will be supplementary to the strictly meteorological programs. During the winter similar studies will also be carried on at the headquarters-station. Particular attention will be given to all those phenomena which have an influence on ablation and alimentation, especially precipitation, relative humidity, temperature, direction and velocity of wind, and radiation to and from the ice.

The object of this work is to gain quantitative as well as qualitative data on the manner in which meteorological phenomena affect the "economy," that is, the growth and wastage of the glacier in its different parts.

Evaporation of snow will be studied and measured by the methods devised by J. E. Church. Ablation from glaciers where melting is excessive will be determined by means of such recording "ablatographs" as have been designed and used by H. W. Ahlmann. Thermal conditions and changes within the ice and snow will be studied by means of thermocouple sets buried to different depths, as was successfully done by Ahlmann and H. U. Sverdrup in Svalbard.

Microscopic studies of snow and its alteration to firn will also be carried out and the record supplemented with photomicrographs.

At the snow-line station and the central interior station borings will be made through the firn for the purpose of determining annual layers (which may not be present in the central part of the ice-cap) and the amounts of snow annually accumulated.

At the central station an attempt will also be made to discover the depth at which compacted snow finally gives to solid ice. Since this locality is well within the snow-line where even the temperature in summer is low enough to prohibit extensive melting of the surface-snow and consequent penetration of thaw-water, it is probable that alteration of snow to ice is by compaction alone.

Because it is expected that borings can not be successfully carried to depths sufficient for reaching solid ice, it is planned to take a special heating element along which will melt its way through the snow. The instrument is designed in the shape of a cone about 18 to 24 inches long and two inches in diameter at the large end. It will be electrically heated and allowed to descend point down through the snow. Electric current for the heating will be supplied from a gasoline-driven generator through wires attached at the large end of the cone. Sufficient wire will be allowed for reaching a considerable depth--a thousand feet or so. Melt-water produced during the descent of the cone will be absorbed by the surrounding unmelted snow, thus leaving a clear hole.

Penetration of solid ice below the snow should be indicated by a considerable reduction in the rate of descent of the cone--perhaps by a complete halt in the descent, since melt-water would no longer be absorbed and, unless the temperature of the ice were close to 0°C, the water would re-freeze above the cone holding the wires fast.

To assure a vertical descent of the cone, the pointed lower end will be made either hotter or heavier or both. In addition, it may be necessary to attach several feet of light metal tubing to the upper end. This would act as a vane prohibiting any deflection that might arise from inequalities of snow-density at any given level.

If practicable, a thermocouple-thermometer will be attached to the cone and temperatures observed at different depths by cutting the current and allowing the whole instrument to cool for sufficient time to bring the surrounding snow back to normal.

It is probable, though not certain, that observations with this cone may offer an indirect means of determining snow-density at depth. This is true because the rate of penetration of the cone will depend on three factors, namely, the heat-output of the cone, the temperature of the snow, and the density of the snow. Since the first factor will be known, and the second factor will be determined by each reading of temperature at depth; the last factor, density, can be calculated for the snow immediately above and below the point of known temperature. If it should be found that the temperature becomes constant with depth, then the rate of descent will depend directly on the snow-density. It must be kept in mind, however, that the snow will be laminated in softer and harder layers with the result that these density-determinations will give only average values for a section of at least a few feet.

The uncertainty of this method arises from the fact that penetration of the cone will be immediately preceded by the penetration of melt-water from above. This may materially raise the temperature of the snow, particularly by the release of latent heat on refreezing. Likewise, it will increase the density of the underlying snow by refreezing in the capillary pores. For these reasons it is probable that the method can give satisfactory density-determinations only where the initial temperature of the snow is very close to 0°C--the only condition under which the penetrating melt-water from above would not refreeze and upset the temperature-density conconditions.

One other problem of uncertain solution is the measurement of snow-erosion and transportation by wind in the interior regions of the ice-cap. This is unquestionably an important factor of ablation, yet one that is difficult of evaluation. The amount of erosion can probably be determined by measuring the level of the surface against thin stakes set deeply in the snow. Frequent cores taken to a given level would show the weight of snow lost by such erosion.

The determination of wind-transportation and deposition will be much more difficult; and so far as quantitative measurements of transportation in the air are concerned, it may well be impossible to get accurate data, for there is no means of distinguishing drifting snow from falling snow.

Topographic and depth-measurements of the interior regions of the inland-ice

These studies will be made during the second summer (1941) by two men who will traverse the interior regions of the ice-cap in motor sleds of a new design. The expected cruising range of the sleds is sufficient that the party should be able to make at least six, and probably more, crossings of the northern third of the ice-cap during the season. The different crossings will be made on different lines, and all of them carried out so as to give continuous topographic profiles. This will be done by means of simultaneous aneroid-readings made at intervals of from one to three miles. The two sleds will travel in line, separated by the desired interval. With both sleds stopped simultaneous aneroid-readings will give the difference of elevation between them. Then the lead sled will advance for a distance of from one to three miles while the second sled will come forward to occupy the last station of the lead sled. Again with both sleds stopped the difference of elevation will be determined. With this procedure carried on throughout all traverses, a continuous series of accurate elevations will be had, and the sum of the data will be sufficient for the construction of an accurate topographic map.

Geophysical determinations of depth will be made at as many well-selected places as possible. The number of determinations, however, will depend on the method used and the weight of necessary equipment. The seismic-reflection work planned for the ice-border, where solid ice extends to the surface, will not be of use in the interior, for there the overlying uncompacted snow introduces an unknown variation in density which affects the rate of transmission of the seismic waves. Refraction-methods of seismic sounding would give excellent results. They also offer the advantage of showing density-variations within the ice as well as within the underlying bed-rock. There is, however, the great disadvantage in the amount of explosive necessary for setting up seismic waves through the unconsolidated neve. This would require loading the sleds with explosive at the expense of gasoline, thus greatly reducing the cruising range of the sleds.

There is some hope that an electrical-resistance method can be used. A modification of the standard method is now being worked out by F. W. Lee, and it is believed that it will prove suitable for measuring the depth of ice in glaciers. This would offer the advantage of comparatively light equipment and consequently longer cruising ranges for the sleds.

It is also hoped that some sort of gravimeter can be carried and made to give useful results. However, the requirement that all instruments be simple, rugged, and light makes this hope seem further from realization.

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EVAPORATION-STUDIES AT BATON ROUGE, LOUISIANA

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Introduction--A record of hydrological data for the City Park drainage-area, Baton Rouge, Louisiana (0.808 square mile) has been kept since April 1934. This area is composed of city area with roofs, lawns, sidewalks, and paved streets, as well as a considerable area of golf-course and an artificial lake which has an area of 64 acres. The precipitation is measured by seven standard United States Weather Bureau rain-cans and one weighing-type recording gage. The discharge over the lake spillway is determined by gage-heights from a Stevens type water-stage recorder and a rating curve which was determined by current-meter measurements. Evaporation-data were essential for a clear understanding of the losses from the lake and accordingly a United States Weather Bureau Class-A type land-pan was installed. Later a United States Geological Survey floating-type pan was placed in the lake. This pan was originally made of galvanized iron but, due to severe pitting, was replaced by a copper pan of the same type.

It is the purpose of this paper to show the effect of different meteorological data on the evaporation from the floating-type pan and to present a formula for the evaporation from a copper pan of that type.

Factors considered--The temperature is very important due to the increase of vapor-pressure which accompanies a rise in temperature. It is here considered that the evaporation is a function of the difference between the vapor-pressure as determined by the relative humidity and that which would exist for saturated vapor. This difference was found by taking the product of the vapor-pressure for the daily mean air-temperature and one minus the mean relative humidity for the day.

The daily wind-movement was considered due to the fact that a strong wind sweeps the saturated vapor away and replaces it with relatively drier air. Logically then, the evaporation should increase with greater wind-movement. Figure 1 illustrates the effect of wind-movement on the rate of evaporation.

During the early stages of the investigation, it was found that a sharp drop in temperature was always accompanied by a great increase in the rate of evaporation. For this reason a recording thermometer was installed for obtaining a continuous record of the water-temperatures. The City Park Lake is shallow and the mean water-temperatures are generally higher than the mean air-temperatures. The effect of the temperature-difference upon the rate of evaporation is shown in Figure 2. This factor is very important, and has seldom received its proper consideration. The author does not know of any case where it has been directly incorporated in a published equation.

The evaporation-formula for the floating-type copper pan--From curves similar to those of Figures 1 and 2, the following empirical formula was developed

$$E = (V\ P\ D + 0.0016\ T\ D)(0.564 + 0.051\ T\ D + W/300)$$

in which E is the evaporation in inches per day, V P D is the vapor-pressure at mean air-temperature multiplied by one minus the mean relative humidity, T D is the mean water-temperature minus the mean air-temperature, and W is the wind-movement about three feet above the water-surface in miles per day.

Table 1 gives a daily summary of the data and computations for the month of October 1936. It is interesting to note the variation in the rate of evaporation with changes in temperature, relative humidity, and wind-movement. Note the way in which the computed values agree with the observed ones.

Such close agreement as appears in Table 1 does not always occur. During windy weather, the wave-action on the lake is rather bad. The pan was shielded by a fixed barricade which in turn was surrounded by a floating raft. In spite of this protection, water splashed into the pan during periods of high wind, and it was necessary to estimate the evaporation for those days.

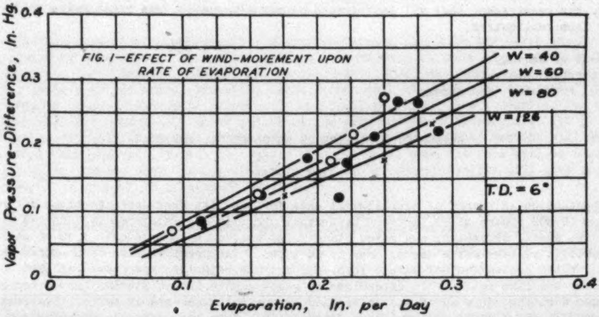
FIG. 1—EFFECT OF WIND-MOVEMENT UPON RATE OF EVAPORATION

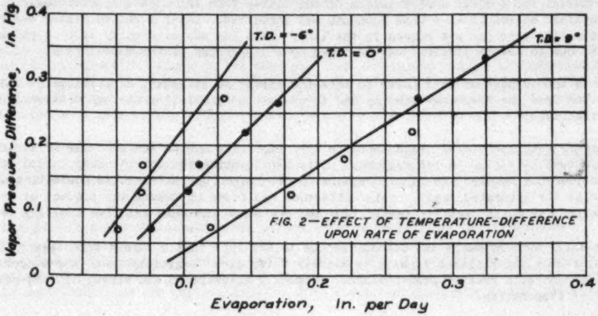
FIG. 2—EFFECT OF TEMPERATURE-DIFFERENCE UPON RATE OF EVAPORATION

Table 1--Sample daily summary of evaporation-data

Date	Precipitation	Wind	Relative humidity	Mean temperature			Vapor-pressure		Evaporation	
				Water	Air	Diff.	Actual	Diff.	Meas.	Calc.
1936	inches	mi/day	per cent	°F	°F	°F	Inch mercury		inch	inch
Oct. 1		116	71	81	72	9	0.783	0.227	0.34	0.34
2		86	78	79	70	9	0.732	0.161	0.29	0.23
3		71	78	78	70	8	0.732	0.161	0.25	0.21
4		31	81	78	70	8	0.732	0.139	0.17	0.16
5		62	90	78	74	4	0.838	0.084	0.08	0.09
6	1.24	61	91	78	74	4	0.838	0.075	0.06[a]	0.08
7	0.08	45	94	78	73	5	0.810	0.049	0.08	0.06
8		61	90	76	70	6	0.732	0.073	0.09	0.08
9		53	81	77	64	13	0.595	0.113	0.18	0.19
10		77	79	76	66	10	0.638	0.134	0.20	0.20
11		130	70	75	64	11	0.595	0.179	0.30	0.31
12		59	78	73	62	11	0.555	0.122	0.21	0.18
13		42	78	72	64	8	0.595	0.132	0.17	0.16
14		57	83	72	66	6	0.638	0.108	0.15	0.12
15		33	86	73	72	1	0.783	0.110	0.05	0.08
16		83	82	75	74	1	0.838	0.151	0.10	0.14
17		60	79	77	74	3	0.838	0.176	0.16	0.17
18		73	76	77	68	9	0.684	0.164	0.23	0.22
19		29	77	75	64	11	0.595	0.137	0.19	0.19
20		63	82	75	67	8	0.661	0.119	0.18	0.16
21		36	81	76	75	1	0.866	0.165	0.11	0.12
22		25	84	78	74	4	0.838	0.134	0.11	0.12
23		90	81	77	69	8	0.707	0.134	0.27	0.19
24		101	81	75	70	5	0.732	0.139	0.18	0.17
25		85	84	76	76	0	0.896	0.143	0.14	0.12
26	.17	97	85	76	71	5	0.757	0.114	0.16	0.14
27		187	78	69	57	12	0.465	0.102	0.32	0.22
28		125	72	65	60	5	0.517	0.145	0.20	0.19
29		84	78	63	58	5	0.482	0.106	0.15	0.12
30		68	74	64	56	8	0.448	0.116	0.16	0.16
31		66	77	65	63	2	0.575	0.132	0.12	0.12
Totals	1.49	--	--	--	--	--	--	--	5.40	5.04

[a] Estimated value

While the estimate was based largely upon the reading of the land-pan, in the majority of cases the computed value exceeded the estimated one.

The computed evaporation for the entire period for which complete records were available appears as Table 2. During the period from April 1935 to August 1935, no temperature-readings were obtained. The thermometer-bulb became corroded and leaked, and it was necessary to return the thermometer to the factory for repairs. During the period until April 30, 1936, the galvanized pan was used. After allowance had been made for the differences which existed for the estimated days, the calculated evaporation for this period exceeded the observed values by 20.6 per cent. From May 1, 1936, until March 31, 1938, the copper pan was used. During this latter period the computed evaporation exceeded the observed total by 0.9 per cent. Were the March 1938 value omitted, at which time the barricade was water-soaked and sunk below the water-surface, the difference between the computed and observed values would have been minus 0.7 per cent.

Conclusions--It is possible to compute the evaporation for a floating-type pan with a high degree of accuracy.

The difference between the air- and water-temperatures has much effect upon the amount of evaporation. High water-temperatures are inducive to high rates of evaporation.

A low relative humidity is accompanied by a high rate of evaporation.

High rates of evaporation accompany high wind-movements.

Acknowledgments--These investigations were conducted under the direction of the author on a cooperative basis between Louisiana State University and the United States Geological Survey.

Table 2--Summary of floating-pan evaporation-data

Date	Evaporation		Diff. on est. days	Net diff.	Remarks
	Meas.	Calc.			
1934	inch	inch	inch	inch	
		Galvanized Pan			
Sep.	5.92	7.22	0.04	1.26	
Oct.	4.94	6.23	0.13	1.16	
Nov.	3.91	5.01	0.42	0.68	
Dec.	2.36	2.92	0.02	0.54	Evaporation-pan leaking.
1935					
Jan.	2.53	3.69	0.28	0.88	
Feb.	2.68	3.93	0.24	1.01	
Mar.	4.33	5.84	0.22	1.29	
Sep.	6.03	6.81	0.03	0.75	
Oct.	4.99	6.13	0.00	1.14	
Nov.	3.60	4.04	-0.03	0.47	
Dec.	2.69	3.13	-0.15	0.59	
1936					
Jan.	2.74	2.50	-0.15	-0.09	
Feb.	2.46	3.53	0.09	0.98	
Mar.	3.90	6.64	0.86	1.88	
Apr.	5.78	5.82	0.47	-0.43	Galvanized pan removed, badly rusted.
Totals	58.86	73.44	2.47	12.11	
1936					
		Copper Pan			
May	6.29	6.52	0.32	-0.09	Copper pan installed.
June	8.54	9.86	1.13	0.19	Pan leaking, rivets rusted out.
July	6.88	7.27	0.17	0.22	
Aug.	5.89	6.87	0.13	0.85	
Sep.	5.49	5.48	0.13	-0.14	
Oct.	5.40	5.04	0.02	-0.38	
Nov.	3.11	3.38	0.19	0.08	
Dec.	1.93	1.93	0.04	-0.04	
1937					
Jan.	2.01	2.49	0.38	0.10	
Feb.	3.08	3.46	0.51	-0.13	
Mar.	3.84	3.84	0.07	-0.07	
Apr.	5.63	4.89	0.04	-0.78	
May	7.64	7.68	-0.02	0.06	
June	7.48	7.09	0.00	-0.39	
July	7.60	7.03	-0.02	-0.55	
Aug.	6.25	5.29	0.04	-1.00	
Sep.	6.50	6.71	0.06	0.15	
Oct.	4.85	4.75	0.04	-0.14	
Nov.	3.46	3.83	0.08	0.29	
Dec.	2.47	2.35	-0.24	0.12	
1938					
Jan.	2.40	3.67	0.62	0.65	
Feb.	3.15	3.42	0.07	0.20	
Mar.	4.35	6.26	0.06	1.85	Outside float water-soaked, splashing possible.
Totals	114.24	119.11	3.82	1.05	

Credit is due the various graduate students and student assistants who have been engaged on various phases of the project and who have taken many of the observations.

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ENERGY-BALANCE IN STREAM-FLOWS CARRYING SUSPENDED LOAD

Robert T. Knapp

Problem--The mechanism by which a flowing fluid transports solid material in suspension has received a great deal of thought. Many avenues of approach have been explored in attempts to gain more insight into the phenomenon. One of these which has been employed with the object of establishing the basic criteria of transportation in suspension has been the study of the energy-balance of the flow. The Transactions of the American Geophysical Union contain important discussions that fall in this category. In 1933, W. W. Rubey [Trans, 1933, p. 497] presented a paper on "Equilibrium-conditions in debris-laden streams" to the Section of Hydrology. This article proposes a general expression of stream-equilibrium in the form of an energy-equation. In this equation the loss in potential-energy of the flowing mixture plus the decrease in kinetic energy of flow is equated to the energy consumed in friction plus the energy consumed in supporting the debris. One of the significant conclusions reached by Rubey on the basis of this analysis, together with pertinent field-data, is that the energy consumed in supporting the debris is normally a very small fraction of the total energy in the flow.

In 1935 another paper in this category was presented to the Section of Hydrology by Howard L. Cook [Trans, 1935, p. 456], entitled "Outline of the energetics of stream-transportation of solids." In this presentation Cook outlines six energy-demands on the flow and concludes that "until the various energy-demands have been fairly well investigated there is little possibility of using the energy-equations to predict the behavior of artificial channels, or to apply this line of reasoning to investigations in the morphology of streams."

Purpose of present article--Both of these papers emphasize the necessity of examining in detail the mechanism of debris-transportation before any useful conclusions in this field can be drawn from the application of the general energy-equation. The purpose of this article, therefore, is to present some further considerations involved in the transportation of that portion of the debris which is carried in suspension. The conclusions reached are general in character and are put forward in the form of working hypotheses, with the hope that they may serve as the basis of additional experimental study of the phenomenon.

Viewpoint--The phrase "energy-balance" is a very loose one and can be interpreted in several ways. The general usage in hydraulics is to apply it to the mechanical energy only. Transformations from mechanical energy to heat are considered "losses" instead of merely manifestations of another form of energy. All such equations simply define special cases of the general principle of conservation of energy, and as such are very useful. Indeed, since energy is conserved it is perfectly proper to set up equations to account for the disposition of any particular subdivision of it that may be of interest. As has been indicated, one of the normal approaches to the study of the energy-considerations in the transportation of suspended load is to attempt to set up the energy-balance of the flow as a whole. However, it is also helpful to investigate simply the energy-exchange between the fluid and the suspended particles. This latter viewpoint leads to the following hypothesis concerning the capacity of a given stream to transport material of a specified kind.

Statement of hypothesis--Briefly stated, this hypothesis begins with the postulate that a particle of sediment in suspension in a flowing stream of fluid simultaneously adds and subtracts energy from the stream through two independent processes. It then reasons that if the amount of energy added is less than the amount subtracted, the net result is that the particle is a burden on the energy of the stream and that, therefore, there must be a definite limit to the number of such particles that can be kept in suspension in a unit-volume of the fluid. On the other hand, if the amount of energy added by the particle is greater than the amount subtracted, then the energy of the stream is increased by its presence. Under the latter conditions it would appear that there would be no limit to the number of such particles that could be kept in suspension and transported by the unit-volume of the fluid. In other words, if the suspended sediment is a drain on the energy of the flow then there will be a definite load-carrying capacity of the stream for that particular size of material, whereas, if the sediment adds to the net energy of the flow then the only limit to the concentration of such sediment that can be transported will be that imposed by the transition from fluid to plastic flow.

Mechanism of energy-contribution from suspended load--Now consider the mechanisms through which the suspended particles and the fluid exchange energy. The first concerns the energy added to the fluid by the particle. If, over a given length of channel, the concentration of sediment of a given particle-size remains constant, then statistically it can be considered that each particle moves parallel to the channel bed. Examine one such particle suspended in a stream

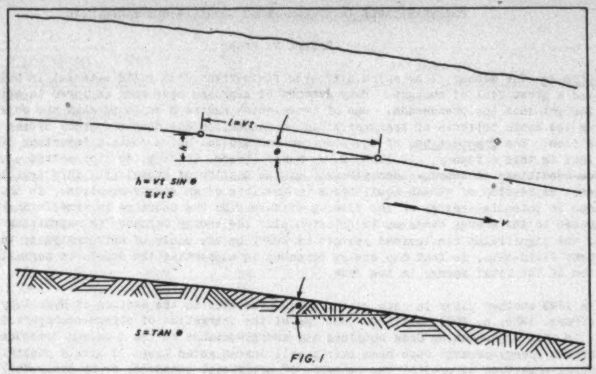

FIG. 1

flowing at a velocity v in a channel of slope s as shown in Figure 1. In time t the fluid, and therefore the particle, moves downstream a distance $l = vt$. The vertical component of this movement is $l \sin\theta = vt \sin\theta \cong vts$ (since $\sin\theta \cong \tan\theta$). If ρ_f is the density of the fluid, ρ_p is the density of the particle, and V its volume, then the apparent weight of the immersed particle is $(\rho_p - \rho_f)\,gV$. Thus the energy given up by the particle to the stream in time t in excess of the energy that would have been given by the displaced fluid is

$$\Delta e = vts\,(\rho_p - \rho_f)\,gV \tag{1}$$

If there are n such particles per unit-volume of the mixture then their total weight will be $n\rho_p\,gV$. This may be called the concentration c. The apparent weight, however, will be less, being equal to $c(\rho_p - \rho_f)/\rho_p$ The energy-contribution of such a suspension, per unit-time, per unit-volume of mixture will be

$$E = vsc\,(\rho_p - \rho_f)/\rho_p \tag{2}$$

Mechanism of energy-utilization by suspended load--The second mechanism is that by which the particle subtracts energy from the fluid. Consider a particle falling freely in a body of fluid which is at rest. It will quickly attain an equilibrium velocity v_p which represents the force-balance between the skin-friction and the apparent weight of the particle. The particle is obviously dissipating to the fluid in time t an amount of energy

$$\Delta e_1 = v_p t\,(\rho_p - \rho_f)\,gV \tag{3}$$

If the fluid contains a concentration c of such particles, then per unit-time and per unit-volume of the mixture, these particles dissipate energy at the rate

$$E_1 = v_p c\,(\rho_p - \rho_f)/\rho_p \tag{4}$$

However, if the particles are suspended in a moving stream of fluid, and therefore remain statistically in the same location relative to the fluid, then, in order to produce the forces on the particles needed to keep them in suspension, the fluid must supply the mechanical energy dissipated in heat in the skin-friction and the rate of supply must be E_1.

Energy-balance due to presence of suspended load--The net unit-energy contributed to the stream by the suspension is therefore

$$E - E_1 = (vs - v_p)\,c\,(\rho_p - \rho_f)/\rho_p \tag{5}$$

According to our hypothesis, if this value is positive then there is no limit to the concentration of such material that can be transported in suspension, but if it is negative then apparently there must be a definite limit to the concentration that can be supported.

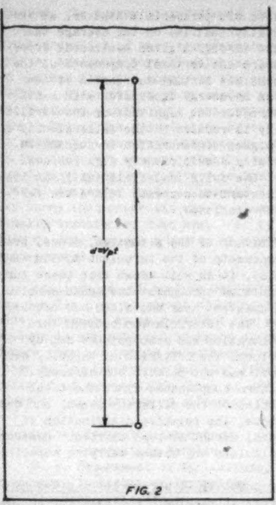

FIG. 2

Alternate formulation of hypothesis--An examination of equation (5) shows that the energy will always be positive as long as vs is greater than v_p. This leads to the following alternate method of stating the original hypothesis: The concentration of sediment of a given size which can be transported by a given stream is unlimited, except by the fluidity, if the vertical component of the stream-velocity is greater than the velocity of free fall of the same sediment at the same concentration in the same fluid when the fluid is at rest.

This alternate wording of the hypothesis makes allowance for the experimental fact that the fall velocity of particles in a fluid at rest actually varies somewhat with the particle-concentration, decreasing as the concentration increases. This effect is commonly referred to as "hindered settling." A physical explanation of this variation in fall velocity can be obtained if the fall of the particle in the stationary fluid be examined more closely. It is generally assumed that in relative motion between a fluid and a solid surface, if the fluid wets the surface, a layer adheres to the solid and moves with it without slip. The difference in velocities of the surface and the fluid causes shear in the fluid layers. This shear is high near the surface, and decreases in intensity with the distance from the surface. Actually, therefore, no energy is dissipated in heat on the surface itself, but such dissipation takes place all through the fluid. Therefore, in the case of the falling particle the energy is not actually dissipated on its surface. Instead, the particle drags a layer of fluid down with it. This in turn drags more fluid but now not without slippage. Thus the particle pulls down with it a large amount of fluid compared to its own volume, but at an average velocity lower than that of its own fall. This movement of fluid downward in the immediate vicinity of the particle necessitates the flow of an equal amount upward, and thus a circulation is set up in the fluid. This velocity-energy is soon dissipated as heat through the viscous friction in the fluid. However, in case other similar particles are falling in the fluid at the same time, their fall will be slowed up by this upward current, or, in effect, part of the fall-energy of the first particle is used to help support the second one. Obviously, the greater the concentration of particles the more of this velocity-energy can be used for support before it is transformed into heat, and thus the lower is the observed fall-velocity. It is evident therefore, that in equations (3), (4), and (5) v_f should represent the fall-velocity at the same concentration as that existing in the flowing stream.

Factors determining the normal load-capacity of a flow--If the vertical component of the stream-velocity is less than the fall-velocity of the particle, the question naturally arises as to what factors determine the concentration of this sediment that can be carried by the stream, that is, the maximum load-carrying capacity for material of this particular particle-size. In an attempt to indicate the possible direction for further study on this problem, another simple hypothesis is proposed as follows: A given stream is fully loaded with particles of a given size when the vertical-velocity components of the turbulent motion have been reduced in magnitude until they are equal to the free fall-velocity of particles of this size and concentration in fluid at rest. The reasoning back of this statement is very simple. The velocity of fall of the particle in the container in Figure 2 will be the same with respect to the container irrespective of whether this container is at rest or is moving at a constant velocity in a straight line in any given direction. This is a direct consequence of Newton's laws of motion if the fluid in the container is at rest with respect to the container. Now, if under a special set of conditions the particle was observed to have no velocity of fall with respect to the container, it would be concluded that there must be an upward fluid velocity in the container with respect to the particle of a magnitude equal to the normal velocity of fall of the particle, since the conditions for force-equilibrium require a relative velocity between the fluid and the particle equal to this free fall-velocity. Lower upward velocities would reduce the settling rate of the particle, but eventually it would reach the bottom of the container and once there would remain there. Thus, if particles in a stream flowing in a steady uniform state remain statistically in suspension, it must mean that vertical fluid velocities exist in the stream with magnitudes equal to or greater than the fall-velocity of the particles. If only lower vertical velocities existed the particles would soon settle out and stay out of suspension. Higher velocities are permissible however, as vertical components of velocities can exist only in a balanced state, that is, on the average there must be the same quantity of fluid flowing downward as upward through a given horizontal cross-section. In the normal stream-flow these vertical currents are the vertical components of the turbulent motion. Thus the turbulence of the fluid is responsible for the suspension of the particles in the flow. However, each little particle acts as an energy-dissipator with a net effect as indicated by equation (5). This dissipation must reduce the magnitude of the vertical velocity of the turbulent fluctuations, and when the velocity is reduced to the fall-velocity of particles there would be no apparent way of supporting any higher concentration in suspension. Indeed, it is very probable that this criterion does not specify a sufficiently high residual vertical-velocity component in the turbulent fluctuations of the fully loaded flow as it is likely that the concentration of the given sized particles would start to decrease before the vertical-velocity components were reduced to the particle fall-velocities.

The above picture naturally represents an over-simplification of the situation, since, even at one given location in the flow, the vertical-velocity components of the turbulent motion vary greatly in magnitude about their statistical mean value. Also, it is well known that these turbulent velocity-fluctuations are not constant in average magnitude throughout the cross-section of the flow, but vary roughly with the rate of shear, being greatest near the sides and bottom, and decreasing with the distance away from these boundaries. The interrelation between this velocity-distribution and the distribution of sediment-concentration has been pointed out by von Karman, O'Brien [Trans. 1933, p. 487], and others. (In fact, the criterion of capacity here proposed is contained implicitly in their results, although it was not pointed out directly.) This inter-relationship of course makes the present problem more complicated since the total carrying capacity of a stream would be the sum of the capacities of the different zones, and due to the damping effect of the energy-dissipation discussed above, the relative distribution of sediment-concentration would be expected to vary with the total amount of load carried. However, it is much beyond the scope of this article to attempt to calculate any actual carrying capacities.

Effect of other material in suspension on load-carrying capacity of stream for a given particle-size--One or two secondary conclusions may be of interest in passing. For example, on the basis of the above hypothesis the load-carrying capacity of a given stream for a material of given size would not be independent of the loads of material of other size being carried. According to the energy-balance, all material whose fall-velocities were smaller than vs would increase the capacity of the stream to carry the larger particles, while all material with fall-velocities larger than vs would reduce the capacity of the stream to carry any of the particle-sizes coming within this latter class.

Transportation of large fragments--The behavior of large fragments in flows carrying high concentrations of sediments is also of interest. It is well known that the fall-velocity of a particle increases with increasing density-difference but decreases as the effective viscosity of the fluid increases. The effective density-difference between a large fragment of solid material and the fluid would be reduced materially by the presence of fine sediment in suspension, since such sediment acts to increase the apparent density of the fluid with respect to the fragment. At the same time, the presence of the sediment also acts to increase somewhat the effective viscosity of the fluid. Therefore, the fall-velocity of the fragment may be greatly decreased and thus it may be carried in suspension in this case, whereas in clear fluid flowing at the same velocity it might settle out immediately. Thus the fact that large rocks are transported in flood-times by flows carrying high silt-contents may not indicate the high velocities of flow that preliminary considerations might first suggest.

Effect of energy utilized in suspension upon energy-balance of stream as whole--In conclusion, it may be worthwhile to examine briefly the role that the energy utilized in suspension of the debris-load plays on the energy-balance as a whole. O'Brien, in the paper previously referred to, pointed out some of the important aspects of the role, but it is felt that they are not sufficiently recognized by all. Neglecting velocity-changes, the entire amount of energy supplied to the flow must come from the decrease of potential-energy of the flowing mixtures. In accounting for the utilization of this energy-supply it is very important to differentiate between transitional and end processes in order to avoid having the same quantity of energy appear more than once in the general energy-equations. For example, the concept of the frictional resistance to flow at the channel-boundaries implies a dissipation of mechanical energy into heat at these surfaces. Actually, little if any such dissipation takes place. Some transformation of mechanical energy to heat occurs in the laminar boundary-layer, but most of the energy represented by the product of the forces at the boundaries and the mean velocity of flow is transmitted through it to the main body of the fluid, where it appears as "turbulence."

However, the energy of turbulence is still mechanical energy and thus turbulence represents only a state through which the original potential-energy passes in its transformation into heat. The final mechanism of this transformation must be viscous shear in all cases, as this is the only process available through which an incompressible fluid can convert mechanical energy into heat. In a clear flow the energy in the turbulence must be dissipated through the viscous shear in the eddies. The addition of suspended load supplies a secondary method producing viscous shear within the turbulence units. It does not necessarily increase the amount of energy dissipated, but may simply change the distribution or the mechanism of the dissipation. Thus it is evident that in an energy-balance which attempts to account for the dissipation of the potential-energy, the energy in the turbulent motions actually does not appear at all, being accounted for by the energy dissipated in viscous shear either in the eddies within the turbulence units or adjacent to the particles of suspended load. Likewise a term for bed-friction would be redundant, as would all similar terms that represent simply stages through which the mechanical energy passes before it is finally transferred into heat. If it is desired to trace the entire path of the energy, then it must be done through a series of equations, each of which represents a stage in the transformation-process.

Application--It must be emphasized that the problems of transportation of suspended load are too complex ever to permit of being fitted in their entirety into the simple hypotheses presented here. However, it is felt that the general viewpoint on which they are based must be taken into account in any rational analysis of the problem of suspended load. It is hoped that in the near future more and better experimental data will be made available with which to test these and other allied hypotheses and that finally it will become possible to predict quantitatively the behavior of debris-laden flows of any fluid, either liquid or gas, carrying in suspension in its movement any material of different density than itself.

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NOTES ON THE FORMATION OF SAND

E. W. Lane

Many rivers flow on beds of clean sand although they carry in their waters countless tons of silt and clay. For example, the Mississippi, whose muddy water is visible many miles out in the Gulf of Mexico, for most of its length has a bed of material containing neither silt nor clay. The greater part of the material is a comparatively uniform sand. The result of the sorting action of flowing streams which causes these sand-formations is familiar to us all, but just what is its cause? Why is it that so often the silt and clay that is carried in such large quantities by the flowing water (in many cases comprising the greater part of the suspended load) is present in such small quantities in the bed-material, and why are the sands of river-beds so often composed of grains of comparatively uniform size?

Most people are familiar with the fact that small particles settle in water more slowly than large ones and that therefore they are more easily kept in suspension, but with the solids carried by the stream so largely composed of the small material, this slower settling rate does not seem an adequate explanation of the almost entire absence of the fine material in the bed.

When one starts to study the particle-size composition of river bed-material in a quantitative way, he is struck with the frequency with which he finds samples, which contain significant amounts of particles above a size of about 0.1 to 0.3 mm in diameter and only insignificant amounts below this size. Often there are large quantities of particles only slightly larger than 0.1 mm but very little of sizes smaller than this. In the mixtures of gravels and sands the size-distribution often seems to end at about this size. Figure 1 shows the mechanical analyses of a number of samples from the beds of rivers of widely varying natures which illustrates the facts stated above.

The author has seen no adequate explanation of these phenomena, and therefore proposes the following one. It is believed that the phenomena are both the effect of the same cause and that this cause may be understood from a study of the laws governing the settling rate of solid particles in water.

The laws governing the rate of fall of spheres in a fluid have been thoroughly studied. The rate of fall depends on the size and specific gravity of the sphere and on the density and viscosity of the fluid. Since the viscosity of a fluid often varies with temperature, the latter also has an influence on settling rates. If sand-grains were perfect spheres with a specific gravity of 2.65 (a typical value for sand), their settling rates in water would be as shown in Figure 2.

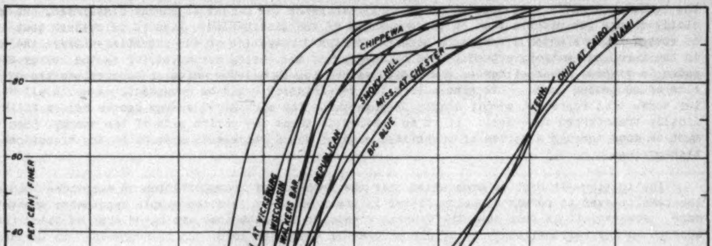

FIG. 1—BED-SEDIMENT COMPOSITION OF VARIOUS RIVERS

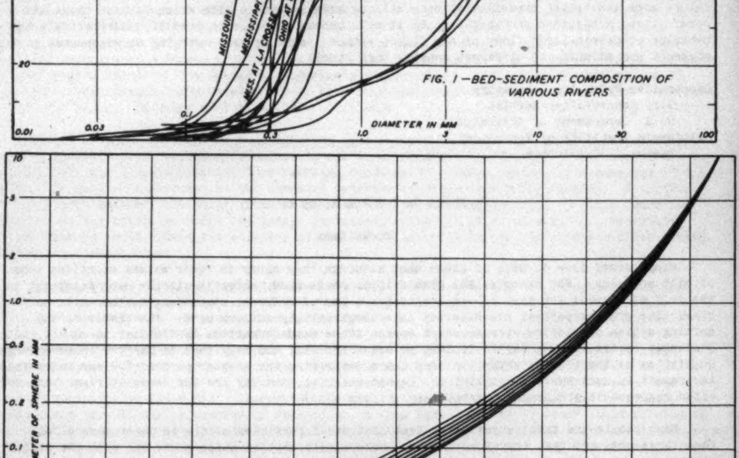

FIG. 2—SETTLING RATE OF SPHERES OF 2.65 SPECIFIC GRAVITY IN WATER

This diagram shows the rates of settlement for spheres ranging from 10 to 0.004 mm in diameter plotted on a logarithmic scale. Each line represents the rate for a different temperature, the range covered by the diagram being from 0 to 30° C. It shows that for small sizes the particles at a given temperature fall at a rate proportional to the square of the diameter, while particles of the larger sizes covered by the diagram fall at a rate nearly proportional to the square root of the diameter.

Data collected by the United States Bureau of Reclamation, based on the work of Hazen, Bardwell, Warren, Nagel, Barnoby, and Hayden, shown in Figure 3, indicates that sand grains follow approximately the relations indicated for perfect spheres, but that the range of sizes between which the change from the settling rate varying as square of the diameter to the rate varying as the square root of the diameter takes place, is smaller for sand than for the perfect spheres. For sand the greater part of this change takes place between diameters 0.1 and 0.3 mm. It is this change of the laws of settlement rate at this range of particle-sizes which is responsible for the phenomenon previously mentioned.

Consider for example particles settling in water at 25° C. Figure 3 shows that a particle of 1/10-mm diameter would settle 1 cm per sec and a particle one-half as large a diameter, or 1/20 mm, would settle only 1/4 cm per sec or one-quarter as fast. In other words, an upward velocity of 1 cm per sec would be required to keep a particle of 1/10 mm in suspension, but a velocity of only 1/4 cm per sec would be required to keep up a particle 1/20 mm in diameter. Since energy-losses in turbulent phenomena tend to vary as the square of the velocity, the energy-losses occurring in a stream which is capable of keeping particles 1/20 mm in diameter in suspension would be only $(1/4)^2$ or 1/16 of that required to keep particles of 1/10 mm in suspension. In other words, below a particle-diameter of 1/10 mm the turbulence required in the water to keep particles in suspension decreases very rapidly with decrease in size of the particles, and therefore very little energy is required to keep large quantities of such material in suspension.

On the other hand, above a diameter of 0.3 mm, the rate of settlement is nearly as the square root of the diameter, and since the energy-loss in the turbulent water should vary approximately as the square of the velocity of the upward components, the energy required to keep particles

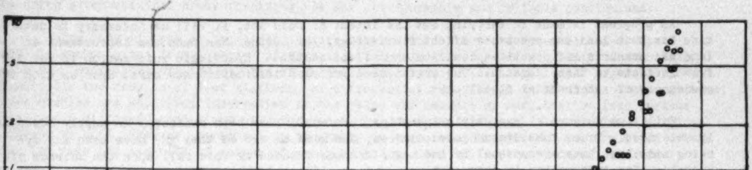

FIG. 3 —SETTLING RATE OF SAND-PARTICLES IN WATER

larger than 0.3 mm in suspension should approximately increase directly as the diameter. The grain-size found in the bed would therefore tend to change gradually above 0.3-mm size. Much of the bed-material in some streams is too coarse to be carried in suspension, but is rolled along the bed. This is mixed with the sand, but in any case the material finer than 0.1 mm is likely to be absent.

In general it can therefore be said that the formation of sand in rivers is principally due to the abrupt change in the laws governing the rate of settling of the particles in water. Below a size of approximately 0.1 mm they settle at a rate which varies as the square of the diameter and above 0.3 mm at a rate which varies roughly as the square root of the diameter. The energy required to keep particles below 0.1 mm in suspension therefore decreases so rapidly with decrease in particle-size that most streams are able to carry in suspension all of the particles which are brought in to them having a diameter below about 0.1 mm without depositing and therefore very little of this material is present in the bed.

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LAND-USE AND FLOOD-FLOWS

W. C. Lowdermilk

Problems of flood-control increase in urgency of solution with more intensive use of land. The Omnibus Flood Control Act of 1936 (Public H. R. 738, 74th Congress) represents an important forward step on the part of the Federal Government to work out with States and Districts ways of alleviating flood-damage and of retardation of storm-runoff from its beginning in unabsorbed raindrops to raging rivers. The gathering grounds of flood-waters become as much a part of the problem as the flood-ways of the trunk-streams. In short, the science of hydrology is called upon to furnish the information necessary to a vast and vital undertaking in the use and protection of land- and water-resources.

As progress is made in carrying out the intent of this Act, it will be necessary to determine how much land-use practices affect flood-flows, and further how much the improvement of land-use measures and practices contributes to flood-control. Hydrologic information is yet far from adequate to these demands. The urgent need for such information has burst upon us with the suddenness of a torrential flood.

Fully two decades of systematic investigations would have been necessary to supply adequately this need. True, some disconnected studies, and good as far as they go, have been and are being made; but they are unequal to the heavy demands created by this call upon the science of hydrology for information to carry out on a nation-wide scale the intent of Congress as expressed in the Omnibus Flood Control Act.

Methods of establishing the required information call for careful consideration. One suggestion is to plan a network of centers for land-use--hydrology investigations to represent recognized hydrologic regions. At these centers, there should be carried out systematic studies of the interrelations of land-use on water-conservation and control in the head-waters and on drainage areas giving rise to storm-runoff and flood-flows. The information gained at each center is highly valuable in itself, but its value for extrapolation would be multiplied severalfold when used in connection with the information from surrounding centers of the network.

It is scarcely necessary to bring to mind the rapidly increasing use and importance of water in our civilization for domestic, industrial, power, irrigation, and municipal uses. Water has become in parts of the country and is becoming in expanding areas, the limiting factor to further development and use of land-resources and support of population. The science of hydrology in land-use is faced with a challenging task.

The question of what and how much land-use measures and practices affect storm-runoff, flood-flows, and water-conservation and utility requires answers promptly, problem area by problem area. After nearly a century of attention and controversial discussion, we are not yet supplied with adequate quantitative information. In the past two decades, however, unprecedented efforts in hydrologic studies have been made. A number of important and promising investigations have been begun. The outlook is especially good for the building up of a body of useful information in hydrologic science.

The requirement is for information which will furnish reliable guides in the modifications and adjustments of land-use in the interests of sustained use of the land-resource, including soils and waters. Special attention must be given to the effects of land-use on flood-flows. To say that floods have occurred in the past does not help. We may expect the recurrence of conditions which produced past floods. The task is to develop adequate information on the hydrology of land-use with specific reference to the conservation of soil and water and to the control of storm-runoff.

The complexity of the problem is known to all students of hydrology. Progress in such investigations is conditioned on analysis of hydrological problems into their several interacting factors, to isolate such factors insofar as is possible, to subject them to critical study and to integrate their functions, region by region and drainage by drainage. It is the purpose of this paper to set forth some general requirements to the answer of this perennial question of the influence of land-use on flood-flows. Special attention is given to storm-runoff as distinguished from ground-water runoff from springs. As the writer has noted in a former paper, wet-weather seepage may contribute an important part of storm runoff during prolonged rains; but ground-water flow from springs except in Karst topography may be neglected in small drainages. Ground-water runoff may become important, moreover, in flood-stages of larger river-systems.

The factors involved in determining the influence of land-use on flood-flows may be grouped into three major categories; they are: first, the absorption and retention characteristics of the land; second, the concentrations of unabsorbed waters of drainage-channels into trunk-streams under variable rain-patterns and third, the accumulation of erosional debris in stream-channels.

These three categories of factors are so different and complex in their effects that unless they are examined and evaluated separately investigations will lead to confusion and unreliable results. In early studies, beginning with the Basin of Ru de la Grenetiere by the famous French engineer, Belgrand, little attempt was made to isolate and study such factors separately. As a consequence, the literature still refers to conclusions based upon studies which can not be fully relied upon. The results are divergent, and conclusions conflicting as well. More recently, however, as evidenced by a series of papers scheduled for this meeting, the factors of hydrology are being given critical study preparing the way for synthesis and reliable conclusions.

A large body of data on the first factor, namely, absorption and detention-capacity of the land, is accumulating from a series of investigations conducted by Federal, State, and private agencies. Noteworthy are the reports of the soil-erosion experiment stations. Figure 1 shows graphically the results at four stations, of differential runoff from rains of two inches or more. These studies are supplying information on the rates and amounts of infiltration into various soils under different conditions of natural and crop-cover on different slopes. The significant progress to date, however, marks only a beginning in evaluating the several factors which determine the rate and capacity of intake of water by soils under optional methods of land-use.

Our knowledge of the absorption and retention by the land was first based upon computing runoff as a residual of measured precipitation upon the contributing drainage-area. The stream-flow and evaporation-study by Ruvaroc and Penck is in this respect a classic. Such computations, however, are as dependable as the base data. The total catch of water from precipitation within a drainage-basin generally has been computed on meagre and insufficient rainfall-records. Until the rain-catch is measured within known variations it is possible to do no more than speculate on the effects of different conditions of land-use on storm-flows.

Within the past few years much progress has been made in making overall approximations of the absorption and retention characteristics of the land within large drainages. In this connection there may be cited the studies by R. E. Horton, by L. K. Sherman, and by the United States Geological Survey under the immediate direction of W. G. Hoyt. For example, land absorption and retention during the floods in New England were found to have values up to 7.6 inches in the Hudson River and Susquehanna regions up to 10.6 inches, and in the Potomac, James, and Upper Ohio regions up to 4.8 inches.

Further requirements in such an approach are to be found in detailed studies of the life histories of storms based upon a closely spaced network of recording rain-gages such as is being carried on in the Muskingum Drainage of Ohio by C. W. Thornthwaite. Isohyetals drawn for half-hour intervals and corresponding stream-flow measurements will yield values in land-intake when channel-storage and flood-wave propagation are accounted for.

Such generalized studies, however, while essential to problems in hydrology, do not answer the specific questions of the comparative effect of land-use practices on storm-runoff. Investigations of greater refinement and more complete control are required to make comparative determinations.

Such studies under closer control are generally carried out on small areas and plots so that experimental errors may be reduced to a minimum. A series of results of such investigations by Auten, Forsling, Musgrave, and Ramser, as well as by the writer and others have been published. They range from infiltration-rates determined by tubes to small plots and small unit-drainages. These results show striking differences in the immediate intake of water by soils under different conditions of cover or treatment. It will suffice our purpose to summarize the order of these differences. An example of the surficial runoff from bared surfaces is shown to be several times

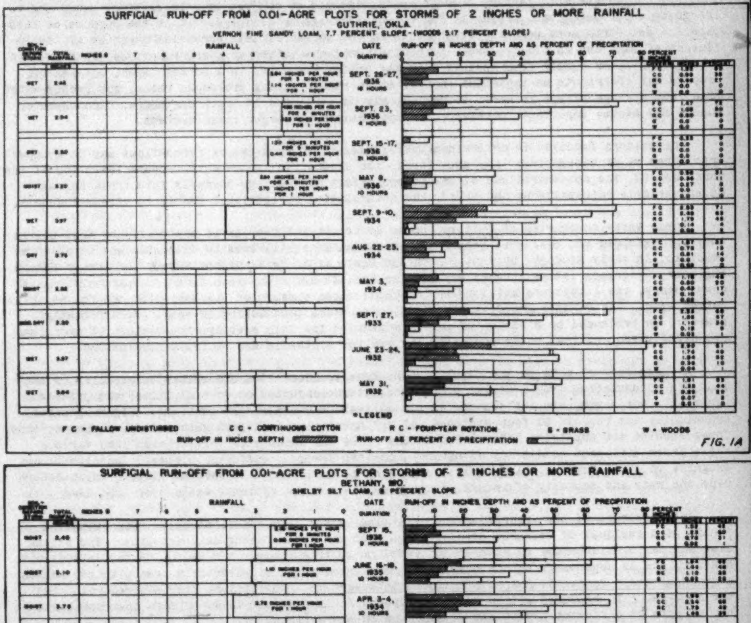


FIG. 1A

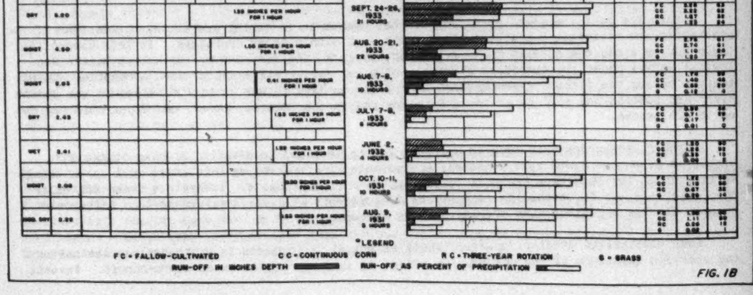
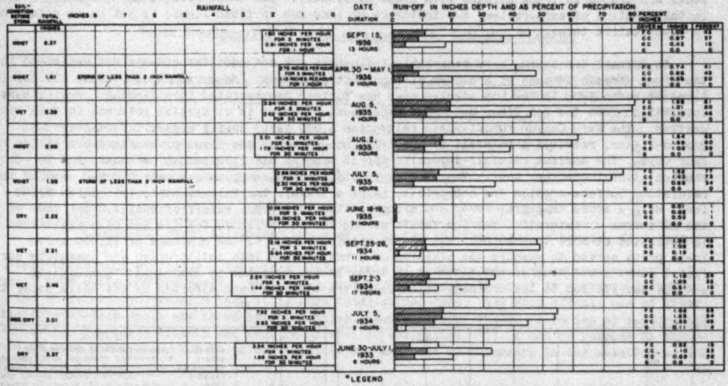


FIG. 1B

that from plots under improved crop rotations and 5 to 30 times greater than from grass- or forest-covered plots. As has been reported in numerous papers, it must be concluded that the surface-condition of the land has a decisive and important influence on the rate of intake of precipitation-waters. But such results are not cumulative for large drainages. Others factors become operative to obscurê the measured differences of runoff from plots and small areas in the flow of large streams. The reasons for this will be examined briefly further on in this paper.

Furthermore, results are cited from small unit-watersheds to show decisive differences in the runoff during storms from land under different cropping practices. Results from two of the soil-erosion field-stations of the Soil Conservation Service at Guthrie, Oklahoma and Bethany,

SURFICIAL RUN-OFF FROM 0.01-ACRE PLOTS FOR STORMS OF 2 INCHES OR MORE RAINFALL
LA CROSSE, WIS.
CLINTON SILT LOAM, 16 PERCENT SLOPE



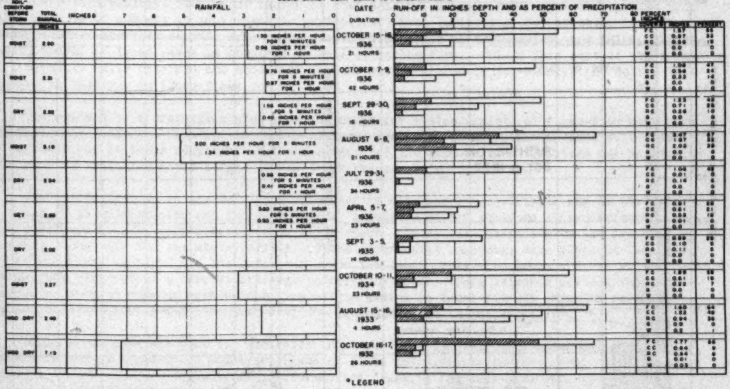
*LEGEND

F C = FALLOW-CULTIVATED C C = CONTINUOUS CORN R C = THREE YEAR ROTATION G = GRASS

RUN-OFF IN INCHES DEPTH ▨ RUN-OFF AS PERCENT OF PRECIPITATION ▭

FIG. 1C

SURFICIAL RUN-OFF FROM 0.01-ACRE PLOTS FOR STORMS OF 2 INCHES OR MORE RAINFALL
STATESVILLE, N. C.
CECIL SANDY CLAY LOAM, 10 PERCENT SLOPE



*LEGEND

F. C = FALLOW-UNDISTURBED C C = CONTINUOUS COTTON R C = FOUR-YEAR ROTATION G = GRASS W = WOODS

RUN-OFF IN INCHES DEPTH ▨ RUN-OFF AS PERCENT OF PRECIPITATION ▭

FIG. 1D

Missouri, are set forth in Table 1 (see Figure 2)

The runoff from the several fields is referred to that from the cotton field where runoff for each storm from the cotton field is expressed as 100; the runoff from pasture was 11 per cent, or one-ninth; from woods, 26 per cent or one-fourth. These results do not show the same order of difference as do the small plots of one-hundredth acre; they serve to integrate results from a variety of conditions as very important differences on the influence of land-use on storm-runoff.

Such values of infiltration- or absorption-differences are determined quantitatively, and have been repeated for many different conditions and for hundreds of storms. The differences are of first importance in erosion control and prevention; they must have significance for flows in the cumulation of runoff and erosional debris within a given drainage-system, although such effects are traced to trunk streams with great difficulty. How these results may properly be used in the solution of our problem, the influence of land-use on flood-flows, is yet to be determined in quantitative terms.

As Bernard has shown, such remarkable influences are not additive and are not reflected in the trunk-streams because of a number of important phenomena. Measured runoff in the California flood of March 1938 in the Los Angeles area has further demonstrated this finding. Preliminary information from the San Dimas Watershed Experimental Forest is of special interest in this connection. The Fern Canyon runoff-plots (elevation 5,000 feet) having a good forest-cover and ground-litter, received a rainfall of about 23 inches during the flood-producing storm within four days. The maximum rainfall-intensity for 15 minutes was 1.70 inches per hour, and for 60 minutes, 1.56 inches per hour. The surface-runoff from these plots was only 0.9 per cent of the rainfall. Surface-erosion was negligible, being a trace. The Tanbark plots (elevation 2,600 feet) with a dense chaparral-cover and ground-litter, yielded a runoff of only 2.0 per cent of the 20 inches of rainfall. Yet the runoff during the storm-period for the San Dimas Watershed is estimated at more than 50 per cent of the rainfall-catch of the drainage of 15,000 acres. These plots served to measure the division of precipitation into infiltration and immediate surficial runoff, but they do not serve as an index of the response by drainages. Wet-weather seepage had yielded to the drainage-channels waters that had been absorbed by the soils under the

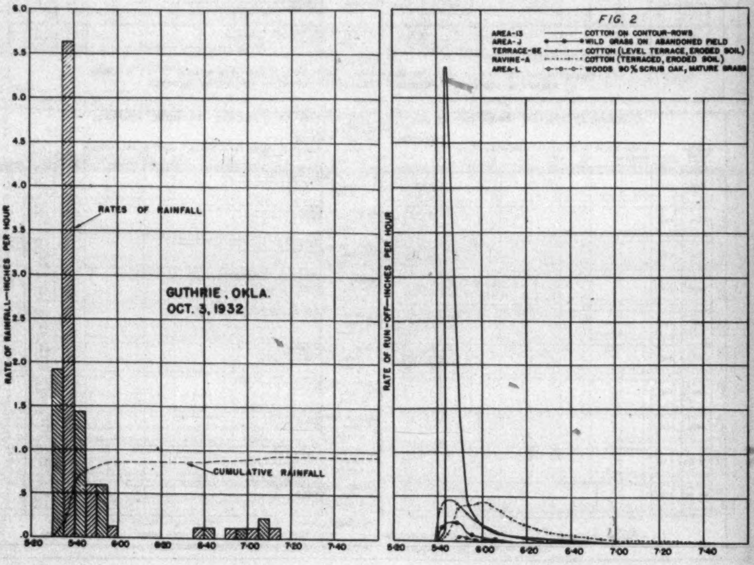


FIG. 2

Table 1-a--Results of soil-erosion tests, Guthrie, Oklahoma

Area	Type surface	Area	Slope	Peak-runoff of storms			
				Oct. 3, 1932		Sep. 10, 1934	
		acre	per cent	in/hr	per cent	in/hr	per cent
Area 13	Cotton on contour rows	3.21	5.13	5.70	100	4.70	100
Area J	Wild grass on abandoned field	5.28	4.44	0.41	7	3.13	67
Area 15A	Cotton, gullied before terracing	3.13	3.42	--	--	2.39	51
Terr. 6E	Cotton, level terrace, eroded soil[a]	1.2	4.56	0.90	16	1.62	34
Area L	Woods, 90 o/o scrub oak-mature grass	5.62	4.8	0.09	2	1.20	26
Pasture	Grasses, good topsoil, not grazed	2.5	5.65	--	--	0.51	11

[a] Length 650 feet

Table 1-b--Results of soil-erosion tests, Bethany, Missouri

Terrace areas, length 1,050 feet		Area	Slope	Peak-runoff	
				Storm of Aug. 9, 1933	
		acre	per cent	in/hr	per cent
5H	Corn in contour rows	1.24	9.7	1.97	100
2G[a]	Alfalfa (first year poor stand)	0.75	12.0	1.79	91
3H	Wheat stubble, clover, and timothy	1.41	8.5	1.79	91
2H	Corn, contour rows, some wild grass	1.46	8.3	1.43	73
6H	Soybeans, drilled contour rows	1.23	9.8	0.62	32
4H	Sod, clover, timothy, and lespedeza	1.25	9.6	0.47	24

[a] Length 650 feet; all grades are four inches in 100 feet; all intervals, five feet.

surface-protection of litter. This is a good example of a discrepancy between measurements of runoff from plots and drainages. The rate of intake of water by the soil is far more decisive in its effect on soil-erosion than it is upon total stream-flow. Yet such effects are of great significance when soils are bared by cultivation, causing flashy flows. A series of factors are responsible for these effects and require careful investigation. It is not my purpose to elaborate on this point here, but to point it out in our analysis as necessary in the solution of our problem.

We now wish to consider the second factor referred to, namely, the concentration of flood-flows; it is the most elusive aspect of flood-stages. I mean to say that in considering the influence of land-use, the factor of variations in the accumulation of water in tributary channels has been usually overlooked in past investigations, but recently is being given due recognition by workers in hydrology. The major factor of the behavior of unabsorbed water in its flow from land-surfaces is subject to many possible variations. Such variations, for example, include first, channel-storage, second, synchronization in discharges of tributaries, and third, differential travel of flood-waves as compared to rate of stream-flow. Unless this part of the mechanism of a flood is fully examined and accounted for, we may be misled in our conclusions on the influence of land-use on flood-flows.

The pattern of a drainage-system sets the stage for a wide variety of flood-flow behaviors responsive to different rainfall-intensity patterns occurring at different seasons of the year. So variable may be these responses that the effects of the land-surface condition may be lost in this maze of resulting flood-stages in the trunk-streams (Fig. 3).

The rainfall-study on the Muskingum Watershed is disclosing how variable may be the pattern of rainfall in distribution and in intensities. A network of 500 stations including recording rain-gages are located approximately four miles apart. Isohyetal rainfall-maps are prepared at half-hour intervals for the storm-periods. These progressive isohyetal maps disclose the developing of rainstorm patterns and their movement over the land. They make plain why variations in stream-flow behavior in a complex drainage-pattern may occur. This evidence gives ground for further caution in drawing conclusions from stream gaging on trunk-streams when the rainfall-pattern in distribution and intensity over the drainage-basin is not known (Fig. 4).

Thus, it is apparent that the contrasts in runoff from land-surfaces under different conditions of use as measured on small experimental plots may diminish progressively down stream as a result of the variable responses to different and changing rainfall-patterns, of channel-storage, of stage-heights and differential movement of flood-waves. The results from the smaller experimental areas may not have areal significance in computing discharge-stages for large drainages. And not until the behavior of storms and the patterns of intensity and amount of rainfall are known and the characteristics of channel-storage, of flood-flow accumulations can be charted, can the influence of the comparative condition of land-surfaces on flood-flows be determined by stream-gaging in large streams.

The third of the major factors, namely, the problem of accelerated and induced soil-erosion and the effect of sedimentation in stream-channels, has high importance in flood-control. In time

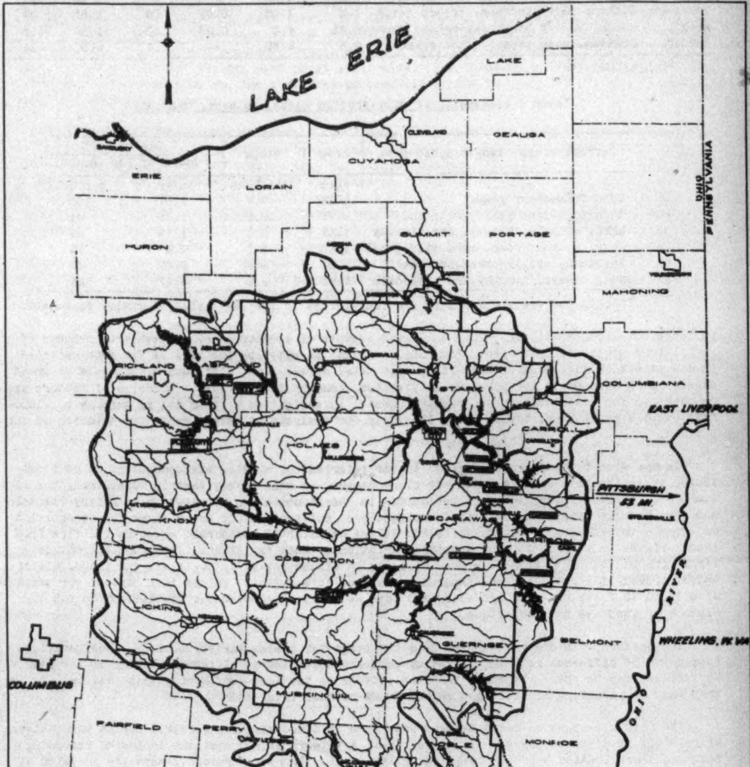

LAKE ERIE
LAKE
CLEVELAND
GEAUGA
CUYAHOGA
ERIE
LORAIN
OHIO
PENNSYLVANIA
SUMMIT
PORTAGE
HURON
MAHONING
WAYNE
RICHLAND
ASHLAND
STARK
COLUMBIANA
EAST LIVERPOOL
CARROLL
HOLMES
PITTSBURGH 53 MI.
KNOX
TUSCARAWAS
HARRISON
COSHOCTON
LICKING
GUERNSEY
BELMONT
WHEELING, W. VA.
COLUMBUS
MUSKINGUM
FAIRFIELD
PERRY
NOBLE
MONROE
OHIO RIVER
MORGAN
WASHINGTON
PARKERSBURG, W. VA.
SCALE OF MILES

FIG. 3
MUSKINGUM WATERSHED CONSERVANCY DISTRICT

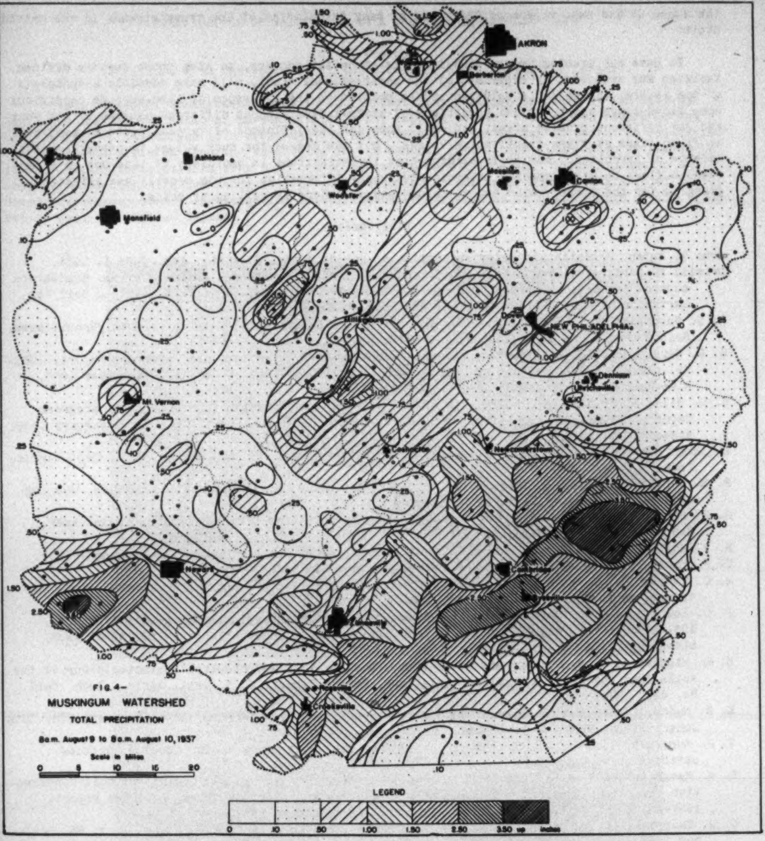

AKRON
Barberton
Shelby
Ashland
Wooster
Massillon
Canton
Mansfield
Millersburg
Dover
NEW PHILADELPHIA
Dennison
Uhrichsville
Mt. Vernon
Coshocton
Newcomerstown
Newark
Cambridge
Zanesville
Roseville
Crooksville
LEGEND
0
.10
.50
1.00
1.50
2.50
3.50 up
inches

FIG. 4—
MUSKINGUM WATERSHED
TOTAL PRECIPITATION
8a.m. August 9 to 8a.m. August 10, 1937
Scale in Miles
0 5 10 15 20

the accumulations of erosional debris in silt-laden waters will nullify any engineering structures unless progressive adjustments are made. The accumulation of erosional debris is one of the most serious menaces to the lasting effectiveness of flood-control works. Unlike the effects of storm-runoff, which makes a non-stop trip from the catchment-area through tributaries to the ocean, heavier erosional debris progresses in innumerable stages downstream. Flood-flows swell and pass on, but sediments accumulate gradually changing channel- and storage-capacities.

Eakin (Silting of Reservoirs, U. S. Dept. Agric., Tech. Bull. No. 524, July 1936) and later Dobson and Brown have called attention to the significant adjustments of features of stream and valley to the increased stream-load of erosional debris derived from accelerated erosion of drainage basins. The effect on floods is brought about in the reduced channel cross-section and configuration. And, furthermore, the aggregate volume of debris-load in transport from place to place in the channel affects fundamentally the volume of the current as well as its velocity. In time the cumulative effect may so alter the channel-characteristics and capacity as to jeopardize the effectiveness of downstream flood-detention and control-structures, as has been the case in the Huang Ho and Hwai rivers of China and of many tributaries of the trunk-streams in the United States.

To have our problem broken down into its essential factors, to have those factors defined, isolated and evaluated by experimental and observational study, will make possible a synthesis of the several values for a solution to the problem of the influence of land-surface conditions upon flood-flows under various conditions of storm-intensities at different seasons of the year and for different problem areas; not until then can the influence of various types of land-use on flood-flows be given quantitative values. But the demand for such values is heavy upon the science of hydrology. And it is needful that investigations in hydrology of land-use in relation to flows be given special attention to supply information for the orderly and effective planning for the conservation and sustained use of waters as well as of soils.

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INVESTIGATION OF VELOCITIES IN A 12-FOOT RECTANGULAR FLUME

C. H. Pierce

(Published with the approval of the Director, U. S. Geological Survey)

From a series of measurements made in the 12-foot wide rectangular flume at the National Hydraulic Laboratory, National Bureau of Standards, information was obtained in regard to the distribution of velocities in a cross-section with respect to the manner in which the velocities near the sides differed from the velocities in the central part of the flume, also in regard to the variations in velocities from point to point in the vertical depth. This information was used in connection with the laboratory-project conducted by the Water-Resources Branch of the Geological Survey in the investigation of current-meter performance in measurements of the velocity of water in shallow depths.

FIG. 1—GENERAL VIEW OF FLUME WITH GRAVEL-BED IN PLACE

FIG. 2—CLOSE-UP VIEW OF COURSE GRAVEL WITH CURRENT-METER AT MEASURING SECTION

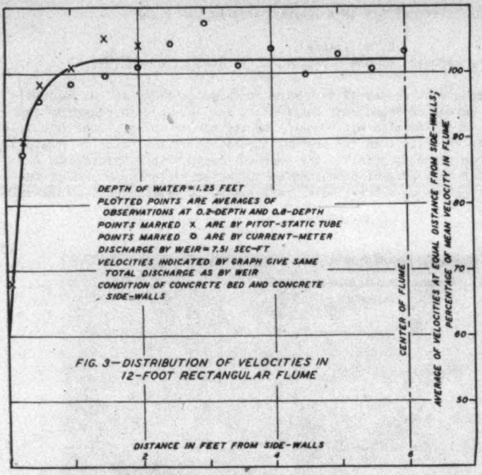

FIG. 3—DISTRIBUTION OF VELOCITIES IN 12-FOOT RECTANGULAR FLUME

Measurements of the flow in the flume were made with the different conditions of smooth concrete, 3/4-inch-gravel, and coarse-gravel beds in order to obtain information applicable to various conditions of natural stream-beds. The coarse-gravel bed consisted of run-of-bank gravel, all retained on a 1-inch screen and passing a 5-inch screen. The gravel-bed extended about 50 feet above and 10 feet below the section where velocity-measurements were made. A general view of the flume with the gravel-bed in place is shown in Figure 1. A close-up view of the coarse gravel, with a current-meter at the measuring section, is shown in Figure 2.

The water flowing in the flume was measured by a sharp-crested weir calibrated in place. For flows less than 3.5 cfs the water was also measured by an 8-inch or a 4-inch Venturi meter calibrated in place, and for those flows within the capacities of the Venturi meters the Venturi measurements were accepted as being more accurate than the weir-measurements. The depth of water in the flume was regulated by needle-gates a short distance below the place of measurement, the needle-gates being adjusted so as to obtain the desired velocity and depth for a given discharge.

About 700 measurements of discharge were made with current-meters of various types. Measurements were made with a Pitot-static tube for purposes of comparison and for determining the distribution of velocities near the vertical walls. The current-meter measurements were made at various depths from 0.2 foot to 1.5 feet corresponding to settings of the needle-gates that gave average velocities in the flume ranging from 0.1 to 1.5 foot per second. The current-meter measurements were compared with the weir or Venturi-meter measurements of discharge, and from these comparisons a series of diagrams was prepared showing the results of the comparisons for the various conditions and methods of measurement in which the current-meters were used.

Space permits only brief description of the methods of measurement and the results obtained showing how the movement of the water varied from place to place in the flume. Figure 3 shows the way in which the velocity of the water decreased in the vicinity of the vertical walls. This diagram is plotted as an average of the conditions that existed on the two sides, and is plotted in this manner in order to give a general picture of the conditions. The velocity was distributed fairly uniformly across the flume between distances of about a foot from each wall, although it was slightly greater on the left side than it was on the right. This was caused by conditions upstream from the measuring section, and was smoothed out to some extent by taking averages of the velocities at equal distances from the two vertical walls, although the plotted points still show some minor variations. The diagram in Figure 3 corresponds to a depth of water of 1.25 feet and a discharge of 7.51 cfs flowing on the concrete bed of the flume, the average velocity being 0.50 foot per second, and is typical of the conditions that existed for other depths and velocities.

Figure 4 shows the velocity-distribution in vertical sections taken near the walls and also at Station 6 (center of flume). The conditions under which these velocity-measurements were made were the same as those described above, that is, a depth of 1.25 feet and a discharge of 7.51 cfs, except that in this case the flume bed was covered with 3/4-inch gravel. The Pitot-static tube was used in the measurements of velocities shown in Figure 4. The individual diagrams for sections on the two sides of the flume, shown in Figure 4, indicate slightly higher velocities for the sections on the left than for those on the right side, as was mentioned earlier in this paper.

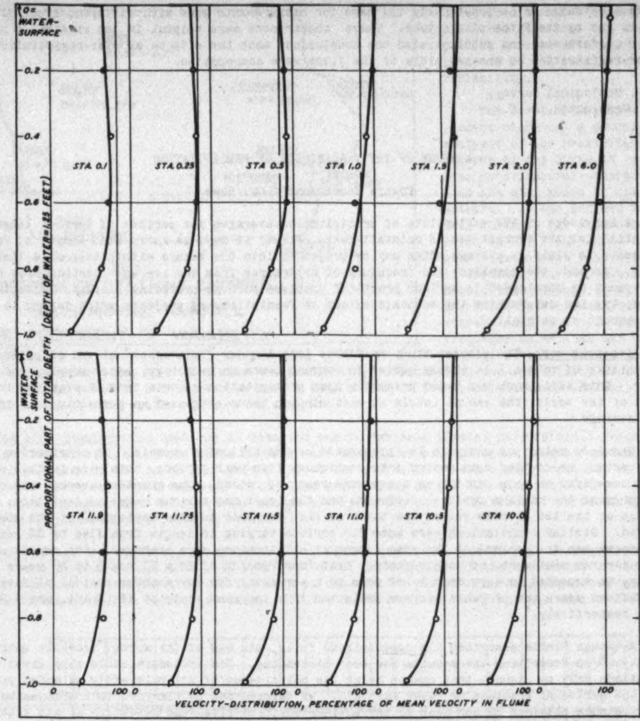

FIG. 4—DISTRIBUTION OF VELOCITIES IN 12-FOOT RECTANGULAR FLUME; DEPTH OF WATER= 1.25 FEET, MEAN VELOCITY =0.5 FEET PER SECOND; STATIONS ARE DISTANCES IN FEET FROM LEFT-SIDE WALL

Velocity-observations made near the sides of a flume by means of a vertical-axis cup-type current-meter generally indicate velocities in excess of the true velocities on that side of the flume where the resultant of the velocity-gradient across the meter-cups falls on the side of the axis toward which the cups are turning. Similarly, the current-meter indicates velocities less than the true velocities on that side of the flume where the resultant velocity falls on the side of the axis from which the cups are turning. For the conditions under which the measurements described in this paper were made, it was found that current-meter over-registration on one side of the flume was equal to, and counterbalanced by, the under-registration on the other side of the flume, so that a diagram of velocities plotted as in Figure 3, using distance from side walls and average of velocities at equal distances from them as the coordinates for the points, gave a correct representation of the average conditions and could be used in the computation of discharge. It was found that the effect of flume-walls on current-meter performance, and also the effect of the walls on velocity-distribution, did not extend farther than two feet from either wall; in fact, there was only a small effect at a distance of one foot. The discharge in the two 2-foot widths adjacent to the walls, and the discharge in the intermediate 8-foot width--component parts of the same measurement--were compared with the total discharge indicated by the measurement, and in that manner the proportional parts of the total discharge that were carried by those flume-areas were determined. The two 2-foot widths were found to carry slightly less than one-third of the total discharge, and for the same depths and velocities the proportional parts were found to be practically the same for measurements made with different types of current-meters and by the Pitot-static tube. These comparisons were helpful in the studies of current-meter performance, and substantiated the conclusions that the effects of over-registration and under-registration on the two sides of the flume were compensated.

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A STUDY OF THE VARIABILITY OF PRECIPITATION

Edward Soucek and J. W. Howe

A knowledge of the variability of precipitation-averages for periods of various lengths is essential for the correct use of rainfall-data. First, it must be known what length of record is needed to yield an average which may be projected into the future within tolerable limits of error. Second, the magnitude and frequency of departures from the average--particularly negative ones--must be considered in various practical problems such as providing storage, selecting sources of supply, and determining the economic extent or feasibility of projects which depend in any way upon amount of rainfall.

Previous work--The pioneer study by Binnie [see 1 under "References" at end of paper] on the variability of rainfall is widely quoted in current texts on hydrology, water-supply, and water-power. This early work was based primarily upon precipitation-records from 26 stations in various parts of the world, the annual totals at each station being expressed as percentages of the long-term average.

Binnie's method of analysis is illustrated by the following example. In considering a five-year period, he divided each record into contiguous five-year periods, obtaining, in this way, about one-fifth as many periods as there were years of record. The greatest average was considered to represent the maximum positive deviation and the least the maximum negative deviation. The average of the two extreme values was taken as the "probable extreme deviation" at the station considered. Similar computations were made for periods varying in length from five to 35 years and the departures for periods of the same length at all stations were averaged. The result was the basis for the statement, so widely quoted, that "for records of from 20 years to 35 years the error may be expected to vary from 3-1/4 down to 2 per cent, and for shorter periods of five, ten, and fifteen years the probable extreme deviation from the mean would be 15, 8-1/4, and 4-3/4 per cent, respectively."

Although Binnie described his computations fully, his use of the words "probable extreme deviation" in describing the results has been misleading. His procedure could have given maximum deviations only by chance--that chance being the coincidence of an arbitrarily selected period with the period of greatest or least rainfall. As pointed out by Blanford in a discussion of the paper, Binnie obtained 32 per cent as the maximum positive five-year deviation at one station, whereas the actual maximum was 39 per cent.

The word "probable" was not used by Binnie to mean that value which was as likely to be exceeded as not, but referred instead to the mean of the maximum deviations which resulted from his computations. It is thus seen that Binnie's "probable maximum deviations" were not extreme nor were they probable in the statistical sense of the word. Furthermore, averaging of the results at the 26 stations obscured differences between individual station-records which were as great as the deviations themselves. Binnie's studies of one-, two-, and three-year periods were based on actual maximum and minimum values and were, in this respect, more reliable than those for longer periods.

Although the writers have criticised some points of Binnie's paper, it should not be inferred that the work is without value. No doubt he was aware of the deficiencies of his procedure, but regarded them as simplifications necessary to expedite the analysis of a large mass of data. Binnie should receive credit for the idea of relating the variability of precipitation-averages to the length of period and for providing a valuable guide to the solution of hydrologic problems.

In 1916 Hazen [2] presented a map showing geographic differences in the "coefficient of variation" of annual rainfall and warned that Binnie's results would not apply to large areas in the United States. The coefficient of variation is a dimensionless measure of dispersion. Generically similar frequency-distributions may be expected to show deviations from the average for a given frequency of occurrence, in direct proportion to this coefficient. Binnie's work, being

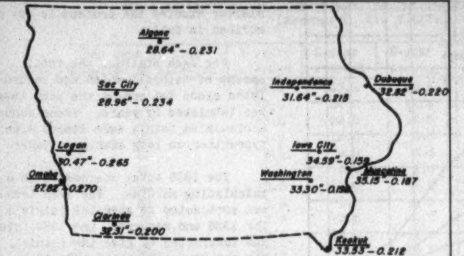

FIG. 1—
LOCATION OF STATIONS SHOWING MEAN ANNUAL RAINFALL AND COEFFICIENT OF VARIABILITY

based on a number of widely scattered stations, did not provide a basis of comparison in terms of the coefficient of variability.

The first objective of the present study was a detailed analysis of the precipitation-records of the State of Iowa, an area whose rainfall-characteristics are well known to the authors. It was decided that this analysis (1) should be based on all occurrences to guard against the possibility of omitting extreme values, (2) should take account of all occurrences rather than just the extreme ones, so as to give the frequency as well as the magnitude of deviations from the mean. The second objective was to investigate the possibility of estimating the variability of precipitation in other parts of the country by direct application of the coefficients of variability provided by Hazen to the results of the Iowa analysis.

The Iowa studies--Ten stations in Iowa and one in Nebraska (Omaha) have rainfall-records dating from 1875 or earlier. Only these records were considered of sufficient length for the studies. The work was limited to the 62-year period 1875-1936, available at all eleven stations, so that any general trend or cyclic tendency which might exist would affect all data similarly. The eleven stations used, and their average annual precipitation and coefficient of variability for the period considered, are shown in Figure 1.

For each of the stations, progressive two-, five-, ten-, 20-, and 39-year averages were computed. (The 39-year rather than a 40-year period was used because of the incidental use of this part of the work in another study.) The work of computation was considerably expedited by successive subtraction from mass-curve data. For the convenience of those who may wish to make

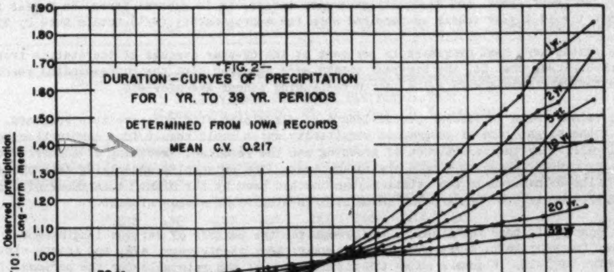
FIG. 2—
DURATION-CURVES OF PRECIPITATION FOR 1 YR. TO 39 YR. PERIODS
DETERMINED FROM IOWA RECORDS
MEAN C.V. 0.217

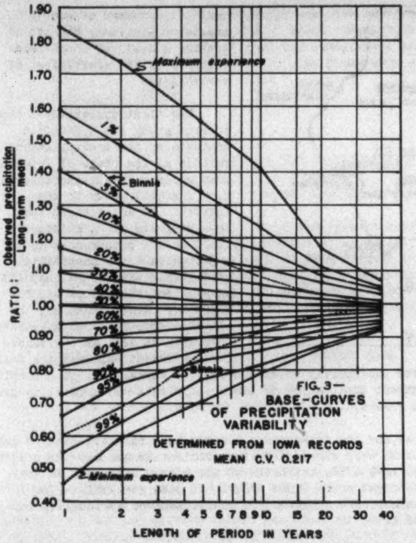

FIG. 3— BASE-CURVES OF PRECIPITATION VARIABILITY DETERMINED FROM IOWA RECORDS MEAN C.V. 0.217

similar studies the process is described in full.

For each station, the total amount of rainfall which had accumulated since the end of the year 1874 was tabulated by years. These annual accumulated totals were listed with a typewriter on long sheets of paper.

The 1936 total was posted on a calculating machine. The 1935 total was subtracted to give the rainfall for 1936 and returned, the 1934 total was subtracted to give the rainfall for the two-year period 1935-36 and returned, the 1931 accumulation was subtracted to give the total for the five-year period 1932-36 and returned, and so on for the 10-, 20-, and 39-year periods ending with 1936. The process was repeated to give one-, two-, five-, 10-, 20-, and 39-year totals ending with 1935, 1934, and so back through the record. The one-year totals were of course obtainable from the records but were computed and read against the original data as an indication of the accuracy of the machine work. A cardboard mask with slots cut out at the proper places was held against the list of of accumulated totals to indicate those to be subtracted from the one posted on the machine. It is estimated that this method reduced the work of calculation by 50 per cent of that necessary to compute the progressive averages directly.

Treated in this way, each 62-year record yielded 62 one-year, 61 two-year, 58 five-year, 53 ten-year, 43 twenty-year, and 24 thirty-nine-year totals, or in general terms, an N-year record gives (N - t + 1) t-year totals as compared with the approximately (N/t)-totals used by Binnie.

The totals were next expressed in per cent of the 62-year average at the station from which they were derived, that is, the one-year totals were compared with the corresponding yearly average, the two-year totals with twice the corresponding annual average, etc.

The values for a particular period-length at all eleven stations were then obtained. It was felt that the slight loss of geographic sensitivity which would result from combination would be more than offset by the convenience of grouping and the resultant smoothing of any erratic tendencies which might occur in single station-records. The principles underlying this choice are similar to those involved in the "station-year" method used by the Miami Conservancy District, A. F. Meyer, the late D. L. Yarnell, and others in studies of storm-rainfall.

The number of items contained in the groups for the periods of various lengths were: One-year, 682; two-year, 671; five-year, 638; ten-year, 583; twenty-year, 473; and thirty-nine-year, 264; a total of 3311. Figure 2 shows the distribution of the rainfalls for the periods of various lengths as duration-curves on probability-paper. The end-points of each curve are actual extreme values, and other points are mid-values of groups used in constructing the curves, both as regards precipitation and percentage-frequency.

A characteristic of the method of analysis, to which attention should be called, is the fact that the averages of all values for the different periods considered are not necessarily identical. This is due to the fact that the initial and final year each enter into but one of the periods of a given length; the years next to the ends enter but twice; etc. In other words, the process applies a trapezoidal weight to the records--only the annual totals t or more years from either end of the record enter the computations for a t-year an equal number of times. This effect could have been avoided by considering each record to be a closing cycle, but this would have in-

Table 1--Variability, Iowa group, in per cent of average rainfall
(Average C. V., 0.217)

Per cent of time equalled or exceeded	1-year average	2-year average	5-year average	10-year average	20-year average	39-year average
1	59.0	47.5	33.5	27.0	11.0	6.0
5	40.5	30.0	21.0	16.0	9.0	4.0
10	28.5	22.5	14.0	11.0	6.5	2.5
20	17.0	12.0	8.0	5.5	3.0	1.0
30	9.5	6.5	4.5	2.5	1.0	0.0
40	4.0	2.5	1.0	0.5	-0.5	-1.0
50	-0.5	-1.0	-1.5	-1.5	-2.0	-2.5
60	-6.0	-5.0	-4.5	-4.0	-3.5	-2.5
70	-12.5	-9.5	-7.0	-6.5	-5.0	-4.5
80	-19.5	-13.5	-10.5	-8.5	-6.0	-5.0
90	-27.5	-20.0	-14.5	-12.0	-8.5	-6.0
95	-33.5	-24.5	-17.0	-14.5	-10.5	-7.5
99	-45.0	-35.5	-22.5	-18.5	-13.0	-8.5

Table 2--Comparison of actual and predicted variabilities in per cent of average rainfall, Boston, Massachusetts
(Long-term average, 41.02 inches; C. V. 0.156)

Per cent of time equalled or excelled	1-year average		2-year average		5-year average		10-year average		20-year average		39-year average	
	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.
1	157	142	142	134	127	124	122	119	112	108	104	104
5	127	129	125	122	122	115	120	112	110	106	103	103
10	122	120	118	116	121	110	115	108	108	105	102	102
20	111	112	110	109	106	106	104	104	102	102	101	101
30	105	107	103	105	101	103	100	102	100	101	99	100
40	100	103	99	102	100	101	98	100	97	100	96	99
50	98	100	96	99	97	99	97	99	94	99	96	98
60	95	96	95	96	96	97	97	97	93	97	96	98
70	91	91	93	93	94	95	95	95	93	96	95	97
80	86	86	89	90	92	92	92	94	92	96	94	96
90	84	80	87	86	88	90	88	91	92	94	94	96
95	80	76	85	82	85	88	87	90	91	92	94	95
99	70	68	79	75	83	84	85	87	91	91	94	94

Table 3--Comparison of actual and predicted variabilities in per cent of average rainfall, Cleveland, Ohio
(Long-term average, 34.02 inches; C. V. 0.162)

Per cent of time equalled or excelled	1-year average		2-year average		5-year average		10-year average		20-year average		39-year average	
	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.
1	155	144	138	135	122	125	116	120	111	108	103	104
5	123	130	120	122	121	116	116	112	110	107	103	103
10	121	121	116	117	114	110	114	108	109	105	103	102
20	115	113	111	109	108	106	107	104	103	102	102	101
30	106	107	109	105	105	103	101	102	98	101	101	100
40	101	103	103	102	103	101	99	100	97	100	99	99
50	99	100	99	99	99	99	98	99	97	99	98	98
60	96	96	96	96	97	97	96	97	96	97	96	98
70	93	91	92	93	94	95	95	95	95	96	96	97
80	82	85	89	90	91	92	93	94	95	96	96	96
90	78	79	84	85	86	89	91	91	94	94	95	96
95	75	75	79	82	85	87	89	89	93	92	95	94
99	72	66	74	74	84	83	86	86	93	90	95	94

Table 4--Comparison of actual and predicted variabilities in per cent of average rainfall, Ogden, Utah
(Long-term average, 16.22 inches; C. V. 0.254)

Per cent of time equalled or exceeded	1-year average		2-year average		5-year average		10-year average		20-year average		39-year average	
	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.
1	152	169	139	156	129	139	122	132	118	113	107	107
5	142	147	131	135	127	125	120	119	117	111	107	105
10	134	133	126	126	123	116	119	113	116	108	107	103
20	125	120	123	114	118	109	115	106	112	104	106	101
30	117	111	116	108	112	105	111	103	105	101	103	100
40	107	105	106	103	106	101	106	101	102	99	102	99
50	100	99	98	99	98	98	97	98	99	98	99	97
60	90	93	93	94	90	95	93	95	92	96	97	97
70	84	85	85	89	87	92	89	92	91	94	96	95
80	76	77	81	84	86	88	85	90	90	93	95	94
90	67	68	68	77	80	83	82	86	88	90	94	93
95	62	61	66	71	77	80	81	83	88	88	92	91
99	43	47	55	58	68	74	79	78	84	85	92	90

Table 5--Comparison of actual and predicted variabilities in per cent of average rainfall, Portland, Oregon
(Long-term average 42.25 inches; C. V., 0.191)

Per cent of time equalled or excelled	1-year average		2-year average		5-year average		10-year average		20-year average		39-year average	
	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.
1	159	152	146	142	137	129	132	124	116	110	106	105
5	140	136	136	126	133	118	130	114	115	108	105	104
10	129	125	129	120	128	112	126	110	113	106	105	102
20	114	115	112	111	110	107	110	105	105	103	103	101
30	108	108	103	106	99	104	97	102	97	101	101	100
40	102	104	102	102	96	101	96	100	95	100	99	99
50	98	100	95	99	95	99	95	99	95	98	97	98
60	93	95	93	96	94	96	94	96	95	97	95	98
70	91	89	91	92	92	94	93	94	94	96	94	96
80	81	83	86	88	90	91	92	93	94	95	93	96
90	75	76	84	82	87	87	91	89	92	93	92	95
95	73	71	75	78	84	85	89	87	92	91	92	93
99	62	60	64	69	83	78	83	84	90	89	91	93

Table 6--Comparison of actual and predicted variabilities in per cent of average rainfall, San Diego, California
(Long-term average, 9.90 inches; C. V., 0.441)

Per cent of time equalled or excelled	1-year average		2-year average		5-year average		10-year average		20-year average		39-year average	
	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.	Obs.	Pred.
1	278	220	179	197	143	168	124	155	111	122	104	112
5	166	182	154	161	132	143	122	132	110	118	104	108
10	153	158	138	146	123	128	120	122	110	113	104	105
20	135	135	123	124	110	116	110	111	106	106	103	102
30	117	119	115	113	108	109	106	105	104	102	102	100
40	105	108	106	105	105	102	104	101	101	99	101	98
50	93	99	95	98	103	97	102	97	101	96	101	95
60	82	88	89	90	99	91	98	92	101	93	100	95
70	73	75	80	81	94	86	96	87	98	90	99	91
80	61	60	77	73	88	79	93	83	95	88	98	90
90	56	44	66	59	80	71	82	76	93	83	96	88
95	49	32	57	50	76	65	80	71	92	79	96	85
99	42	9	49	28	69	54	78	62	92	74	96	83

troduced a synthetic element which was considered undesirable. The effect is relatively unimportant.

Figure 3 shows the data in a form which expedites interpolation for periods of other lengths than those considered. These curves were derived from the duration curves of Figure 2 by showing period lengths as abscissas and plotting curves to represent particular frequencies of occurrences. The curves of maximum and minimum experience show, respectively, the highest and lowest precipitation-totals observed for periods of the various lengths in terms of average rainfall and therefore, represent slightly differing frequencies of occurrence. The intermediate curves show the frequency of occurrence of amounts of precipitation for various lengths of period. Binnie's values are shown as dotted lines in Figure 3. It is seen that only a few per cent of the observations lie outside of Binnie's values for the one-, two-, and three-year periods. For the longer periods, however, Binnie's curves differ markedly from the extremes experienced in Iowa, the discrepancy increasing rapidly with the length of the period.

Extension of results--Hazen showed that the coefficient of variability ranges from 0.14 to to 0.50 in the United States. For all but the southwestern quarter of the country, however, the range is from 0.14 to 0.30. In Iowa the average value at the stations used is 0.217. He stated that "Other things being equal the variations to be expected in any period at different localities will always be in direct proportion to the coefficients of variation in the respective places."

In investigating the use of the Iowa base-curves in estimating the deviations from the average, in other parts of the country, the coefficients of variability at Boston (Massachusetts), Cleveland (Ohio), Ogden (Utah), Portland (Oregon), and San Diego (California), were determined for the 60-year period ending with 1930, substantially the same period covered by the base-curves. The same procedure used in determining the base-curves was then applied to each of these five records and the results tabulated.

These observed values were then compared with predicted values obtained by adjusting the base-deviations throughout the range of period length and frequency in proportion to the ratio of variability-coefficients. For example, if the coefficient at the station under consideration were twice the base-coefficient (0.217), the predicted deviations from the mean would be obtained by doubling the base-deviations. Although this process involved the use of deviations from the mean, the corresponding ratio to the long-time average was used in the final comparison. Thus, positive and negative deviations of ten per cent were expressed as 1.10 and 0.90 respectively. The base values are given in Table 1, and the comparisons of observed and predicted values at the five stations are given in Tables 2 to 6. In these Tables a zone of good agreement, in general five per cent or less of the long-term average, is included between the heavy lines drawn through the Table.

The close agreement between observed and predicted values, particularly in the range of low rainfalls where the most important practical applications occur, considerably exceeded the authors' expectations. The San Diego values (Table 6) provide the only exception to the good agreement found at the selected stations. This may signify that the extension of the Iowa base to an arid region in which the variability of rainfall is greatly in excess of that prevailing in most of the United States and in which nearly all the yearly precipitation occurs during a four-month period will not give satisfactory results. Further investigation is needed to delineate the region to which the Iowa base cannot successfully be extended, as well as to further substantiate the good agreement indicated by the results obtained for Boston, Cleveland, Ogden, and Portland.

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A STATISTICAL METHOD FOR ESTIMATING THE RELIABILITY OF A STATION-YEAR RAINFALL-RECORD

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A very urgent need exists among hydrologists and engineers for the best possible measure of the average frequency of high-intensity rainstorms. This is a particularly difficult determination for those intense storms for which the average length of time between occurrences is as great or greater than the average length of rainfall-records. The station-year method was proposed for the estimation of the average frequencies of such storms for any area, from a long synthetic record built up from many short records for stations in the area.

In any real sense it is not possible to obtain more years of experience of rainstorms merely by placing end to end contemporary records from several stations. However, because certain storms which happen to occur at some stations in an area and not at others, during the period of record, will be included in the record for the area, the station-year record made up from the records of all stations may give a better approximation to the true average frequency for the area than the record from a single station. In other words, the average frequency of, say, rains of four inches or more in 24 hours will be more accurate if derived from the combined ten-year records of 100 stations (1,000 station-years) than if derived from the ten-year record of only one station.

The reliability of the average frequency determined by the station-year method depends upon several factors concerned with the characteristics of rainstorms. One of these factors, that pertaining to the areal extent of storms in relation to the spacing of recording stations is discussed in this paper.

The distribution of precipitation within a storm-area is such that several stations are likely to receive similar amounts of rain in any period. This fact is well illustrated by a rainstorm which occurred on August 9, 1937, in the Muskingum Watershed of Ohio (see Fig. 1). In this 8,000 square mile area the Section of Climatic and Physiographic Research of the Soil Conservation Service has in operation about 500 recording rain-gages, spaced at intervals of approximately four miles. A precipitation-map drawn from the records of these gages for that date shows that five stations in an area of 80 square miles received 3.5 inches or more of rain in a 24-hour period. If these were the only occurrences of such intensities during the entire year, the station-year method, from this one year of record, would give the average frequency of such storms for the area as five storms per 500 stations per year, or one such storm every 100 years, and would assume that this value was based upon five experiences of such a storm, whereas there was in fact only one such storm. This dependence between stations, or in other words, this duplication of storms in the records is one reason why 1,000 station-years of record derived from 100 stations, each with ten contemporary years of record, is not equivalent to a 1,000-year record at one station.

The reliability of an average-frequency value depends upon the number of independent occurrences from which the average was determined. A 100-year record for one station might yield 25 occurrences of rains of six inches or more in 24 hours. One such storm would be expected once in $100/25 \pm 1/\sqrt{25} = 4$ years ± 20 per cent or ± 0.8 year. Suppose, however, that this 100-year record was derived from ten-year records for each of ten stations and, also, that the 25 occurrences were experienced in only five storms. In this case the average frequency should be given as $(10/5 \times 10)/25/5 \pm 1/\sqrt{25/5} = 4$ years ± 40 per cent or ± 1.6 years.

The problem of determining the valid length of a station-year record and of estimating the reliability of the average frequency resolves itself into one of determining the amount of dependence between stations. The method used to determine the amount of dependence is adapted from a statistical technique devised by J. Bartels [see 1 of "References" at the end of paper] and proposed originally for testing persistence in observations taken at equal time-intervals. This technique is based on the fundamental theorem of probability that in a series of random numbers the following relationship holds

$$\sigma(y) : \sigma(1) = 1 : \sqrt{y}$$

or

$$\sigma(y) = \sigma(1)/\sqrt{y}$$

where $\sigma(y)$ is the standard deviation of groups of y-values, and $\sigma(1)$, the standard deviation of all the values in the series. If, however, a certain number, d, of these y-values are identical or dependent, then

$$\sigma(y) = \sigma(1)/\sqrt{y/d}$$

and

$$d = [\sigma(y)/\sigma(1)/\sqrt{y}]^2$$

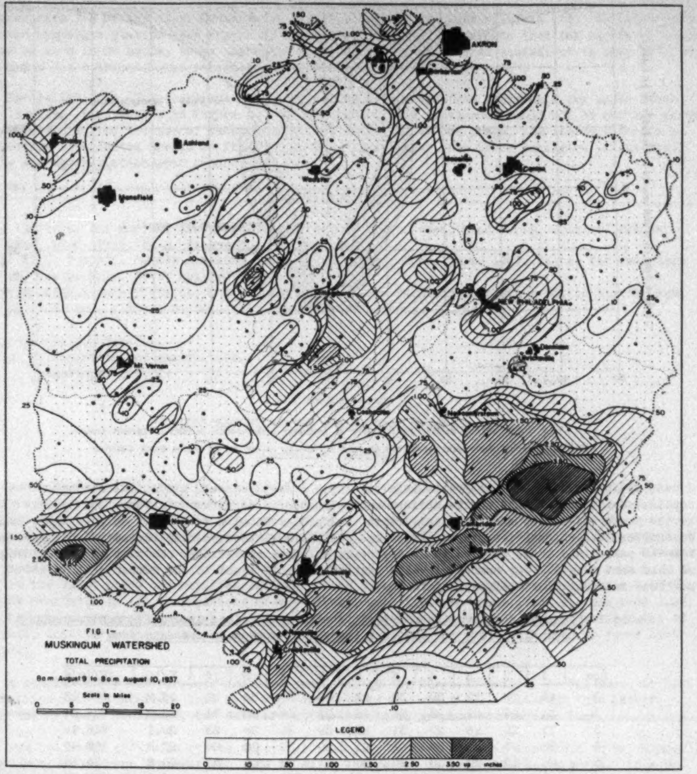
AKRON
Canton
Ashland
Wooster
Mansfield
Mt Vernon
New Philadelphia
Dennison
Coshocton
Newcomerstown
Cambridge
Newark
Zanesville
Crooksville
Scale in Miles
0
5
10
15
20
LEGEND
0
.10
.50
1.00
1.50
2.50
3.50 up
inches

FIG 1—
MUSKINGUM WATERSHED
TOTAL PRECIPITATION
8 a.m. August 9 to 8 a.m. August 10, 1937

To show how this technique may be applied to a set of rainfall-data, a table, simulating a table of rainfall-data, was built up from random numbers, with the additional condition that the number of dependent stations was known. This was done by constructing a chart of ten horizontal squares, representing ten rainfall recording stations, and ten vertical squares, representing ten years of data--100 squares in all. The squares were numbered from 1 to 100, beginning with 1 to 10 for the first horizontal row, etc. Random numbers from 1 to 100 were then drawn from a table [2], each draw representing one rainstorm of a certain intensity, which was recorded in the year and at the station corresponding to the number drawn. A check was made in the square corresponding to the number and, in addition, checks were made in the four following squares. In this manner dependence between the values in the rows was introduced, and since for each draw, or storm, five stations recorded approximately the same amount, one should be able to apply the equation for the determination of d and find the number of dependent stations to be equal to five. In all, 500 such random numbers were drawn, so that the total number of checks was 2,500, or on the average 25 checks per square. The actual number of checks in each square, representing the total occurrences of this intensity at each station for each year, was then obtained. These numbers are substituted in the squares of Table 1.

Standard deviations were calculated and the value of d was determined. As shown below Table 1

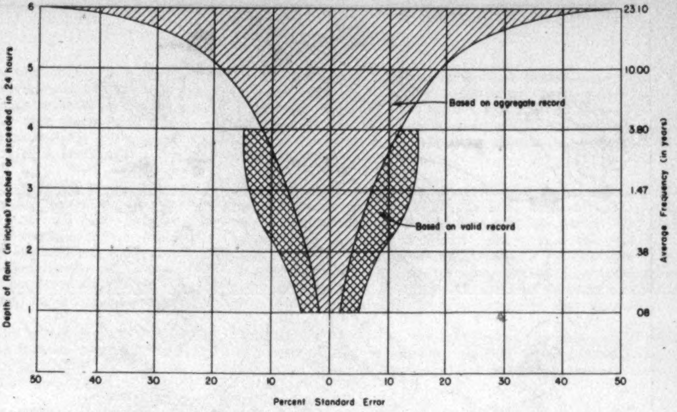

Fig. 2. Graph showing percent standard error in frequency determination of rains of certain amounts, based on aggregate station-year record, and on valid station-year record for an area in South Carolina

d came out to be 4.8 ± 1.4, approximately five as one would expect, since five was the number of duplicated values. Then d may be defined as the number of dependent stations, and 10/5 or 2, will be the number of independent stations. This interpretation of d is correct only if there is no dependence between rows, or years. Tests are available for determining the amount of dependence between years, and these applied to sets of rainfall-data showed that practically no dependence of this sort exists. The number of valid station-years is equal to the number of independent stations multiplied by the years of record.

Table 1--Tabulation of numbers of checks in squares obtained by drawing random numbers from 1 to 100, and checking five squares in each row each draw

Row	1	2	3	4	5	6	7	8	9	10	$\Sigma/10$	$(\Sigma/10)^2$
1	28	30	29	30	30	28	29	30	32	29	29.5	870.25
2	34	29	29	25	25	18	23	24	30	31	26.8	718.24
3	31	29	28	27	31	33	32	31	26	23	29.1	846.81
4	25	26	22	24	23	21	20	27	26	24	23.8	566.44
5	20	19	17	20	20	20	21	20	22	27	20.6	424.36
6	29	35	33	28	24	23	23	27	28	27	27.7	767.29
7	28	26	24	21	19	19	17	14	20	27	21.5	462.25
8	27	27	26	20	15	15	11	16	21	22	20.0	400.00
9	27	32	29	26	25	22	21	25	24	24	25.5	650.25
10	26	25	21	24	25	26	25	28	26	29	25.5	650.25
Total	Sum of individual values squared = 64,720										250.0	6356.14

Calculation of number, d, of duplicated values in Table 1

$$\sigma(10) = \sqrt{6356.14/10 - (250.0/10)^2} = \sqrt{10.614}$$

$$\sigma(1) = \sqrt{64,720/100 - (2500/100)^2} = \sqrt{22.200}$$

$$d = [\sqrt{10.614} \times \sqrt{10}/\sqrt{22.200}]^2 = 4.8 \pm 1.4$$

This method of determining the number of independent stations, and hence of obtaining a better estimate of the valid station-years and of the reliability of the average, has been applied to rainfall-data from several sections of the United States. The results of the calculations for an area in South Carolina are illustrative but cannot be considered to represent the entire country. The determinations of d were made for 24-hour rains of one inch or more, two inches or more, three inches or more, and four inches or more for 15 stations, each with 20 years of record (1917-1936). These stations are in the 2°-square of latitude and longitude corresponding to Quadrangle 7-H of the Miami Conservancy District's study of storm-rainfall [3]. In comparison with the aggregate station-year record of 300 years, values of d indicate that for storms of three inches or more in 24 hours, these aggregate years should be considered equivalent to only 76 ± 26 years, and for storms of four inches or more, 180 ± 77 years.

The reliability of the average-frequency values determined from these data for South Carolina is shown graphically in Figure 2. Calculations of the dependence could not be carried above the four-inch storms because of lack of data. As indicated by the graph, the standard errors of the average frequencies should be from 1.3 to 3.0 times larger than those estimated on the basis of the aggregate station-year record.

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PROGRESS REPORT ON THE ANALYSIS OF RAINFALL-DATA

Carroll F. Merriam

In the very brief period that I have the right to impose upon the time of this meeting, there is only opportunity for a hurried review of some of the progress that has been made during the last year. It is impossible to go into theory, interesting as it may be, as the time can best be spent in giving a glimpse of the many facets of the general problem of getting the most out of our past rainfall-records.

In the first place the detailed results of the study of rainfall in the Susquehanna Valley are now available in the Transactions of the Pennsylvania Electric Association for the year 1937. This report summarizes the analysis of some 200 rainfall-stations, and presents a revised map of rainfall, together with a table of estimated average rainfall for the entire area by years back to 1891.

A similar study for Maryland has been conducted by the Maryland Academy of Sciences, so that the total number of stations that have been examined is now about 400. Although this latter study has not been published, the data are all available in graphic and tabular form.

The principal criticism of the report of last year is that which is contained in Mr. Kincer's paper in these current Transactions, and to which the reader is respectfully referred. As a result of this presentation of the subject from an entirely different point of view, I have since given considerable study to the reconciliation of our conclusions which are so completely diverse. It is plain that if he had accepted the conception of an inconsistency which I had attempted to convey, he would never have adopted in his analysis a method by which inconsistencies, in the sense that I have used the word, are obscured rather than brought to light. In comparing these views the reader should bear this fact carefully in mind, and appreciate that after all we are not dealing with the same fundamental ideas.

The second criticism is that the estimated mean rainfall does not agree with the arithmetic average of the figures for the individual years. We are all familiar with the fact that averages change rather capriciously with the extension of data by the inclusion of subsequent years of record, particularly when the latest additions are either unusually high or low. A nine-year average might be 39.27 inches, but when we add a year of extraordinary drought, as in 1930, the average falls to 38.05 inches. The question is whether there is some way of more closely approximating the true mean.

Let us suppose that we have built a rain-gage capable of holding ten years' rainfall, and that its location is so inconvenient that we can afford to visit it but once a year. At the end of each year we might take our annual reading and plot the results as in Figure 1.

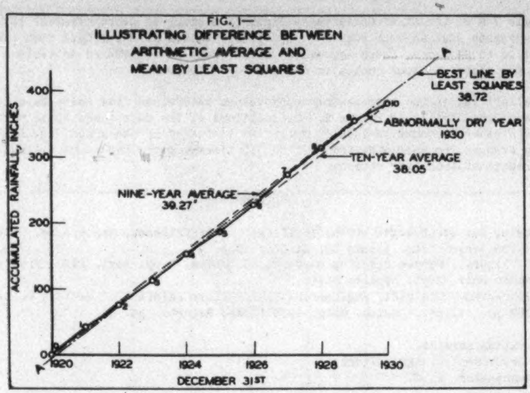


FIG. 1— ILLUSTRATING DIFFERENCE BETWEEN ARITHMETIC AVERAGE AND MEAN BY LEAST SQUARES

It will be seen that the slope of a line connecting any two points, as for example 0 and 9, represents the average between these years, and that this is materially different from the average between 0 and 10. It will furthermore be seen that to determine an average requires but two observations, the intermediate ones being neglected. We should consider whether this neglect is throwing away useful data.

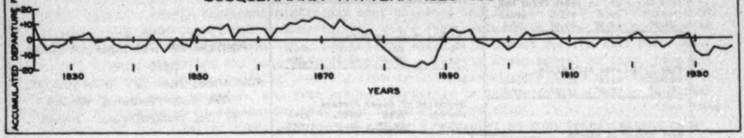
Looking at our problem from a little different point of view, we see that we are interested in the probable future course of the broken line connecting our observations. Naturally we cannot expect that the same values of rainfall will repeat themselves in succeeding periods of years, but we can expect at least a continuation of the same general trend. Thus what we really want to find is the trend of the observed segment rather than the slope of a line connecting the two extreme points. A mathematician would suggest finding this by method of least squares. Fortunately, to do this does not involve the laborious computations required to solve the general case, for thanks to the fact that the values of the abscissae are equally spaced, the process is greatly simplified to the extent that the numerical work can readily be handled on an ordinary calculating machine.

One of the great limitations in the study of rainfall of the past is that there are comparatively few long-time records, and furthermore, those that do exist almost invariably show distinct inconsistencies which make them practically useless in showing past trends. In order to overcome this dearth of material at least to some extent, a standard of comparison, or in effect a composite record, has been pieced together in several stages back to 1825. This is not, as might be supposed, simply an average of the records that happen to be available from time to time during these years, as this would certainly introduce glaring errors arising from the fact that gages which read prevailing high or low would affect unduly the averages for the years in which they happened to be included. The process necessitated step by step the development of tentative standards against which each gage was in turn compared, and then a new standard more refined than the first was developed based upon the preliminary standard. This process was continued until the final result is now available in tabular form by years from 1825, and from 1865 the years have been broken down to months.

When this standard was integrated by drawing a mass-departure curve, Figure 2, one outstanding feature came to light, an unusual period of rainfall-deficiency far more pronounced than any that have occurred during the 114 years studied. From 1875 to 1885 there is accumulated a total subnormal departure of approximately 30 inches followed curiously by several years of extraordinary rainfall which quickly made up much of the deficiency. This same phenomenon may be shown by means of 20-year running averages as in Figure 3.

I should like to take this opportunity in passing to show in circles the corresponding results had the means for the 20-year periods been determined by least squares instead of by arithmetic average. The resulting curve is clearly less subject to accidental fluctuations and, therefore, brings out more positively the true value that is sought.

FIG. 2—MASS-DEPARTURE CURVE OR FIRST INTEGRATION OF RAINFALL SUSQUEHANNA PATTERN 1825-1936 INCL.



When this outstanding deficiency was first noticed, it at once aroused suspicion as to the accuracy of our method. There was the discomforting thought that somehow through the combining of different gages we had produced a false effect. We then plotted accumulated-departure curves for all of the gages individually and found this same effect distinctly in each. An even more extensive search far outside of the watershed showed also in relatively distant gages distinct evidence of a pronounced period of dry years.

It is interesting to surmise why this prolonged period of subnormal rainfall should not have made a more lasting impression. In the first place there was no one year in which the lack of rain was comparable with 1930, and even in the driest year the greatest departure came in the spring. Also water-supply was then not the critical problem that it is today, and, furthermore, there was obviously lack of extensive hydrologic data by which it could have been seen by comparison that the rainfall was deficient for a long period.

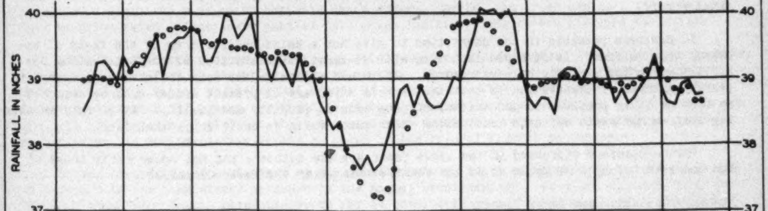
Whether such a thing will occur again is an interesting conjecture, but there is one thing that is evident, that under present conditions it would certainly cause a tremendous drain on the reserve water-supply in populous cities.

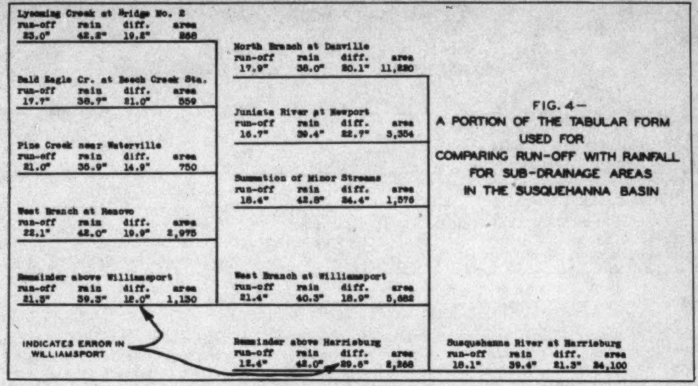
It may be pure coincidence, but interesting nevertheless to note, that the two periods of outstanding floods on the Susquehanna, 1889 and 1936, both came at a time of high rainfall preceded by subnormal precipitation resulting in considerable accumulated deficiency. It would be interesting to know whether there is some balance which once upset by drought tends to restore itself by making up for lost time.

Again referring to Figure 3, it is perhaps comforting to note that no pronounced trend downward is evident. There are, as will be seen, rather radical irregularities in precipitation, but when viewed from this angle there is certainly no startling proof of a progressive dwindling away.

These variations in water-supply have intrigued a number of investigators to examine their cyclic nature together with any possible relation to sunspot-activity. It is hoped that this composite record may serve as basic data for any that care to experiment with methods of analysis. Rainfall is, of course, by no means as satisfactory from this point of view as stream-flow, but

FIG. 3—TWENTY-YEAR RUNNING ARITHMETIC AVERAGE (BROKEN LINE) AND TWENTY-YEAR MEANS DETERMINED BY LEAST SQUARES (CIRCLES) FOR A 114-YEAR COMPOSITE RECORD OF RAINFALL FOR THE SUSQUEHANNA BASIN





Lycoming Creek at Bridge No. 2

run-off	rain	diff.	area
23.0"	42.2"	19.2"	268

Bald Eagle Cr. at Beech Creek Sta.

run-off	rain	diff.	area
17.7"	38.7"	21.0"	559

Pine Creek near Waterville

run-off	rain	diff.	area
21.0"	35.9"	14.9"	750

West Branch at Renovo

run-off	rain	diff.	area
22.1"	42.0"	19.9"	2,975

Remainder above Williamsport

run-off	rain	diff.	area
21.3"	39.3"	18.0"	1,130

North Branch at Danville

run-off	rain	diff.	area
17.9"	38.0"	20.1"	11,220

Juniata River at Newport

run-off	rain	diff.	area
16.7"	39.4"	22.7"	3,354

Summation of Minor Streams

run-off	rain	diff.	area
18.4"	42.8"	24.4"	1,576

West Branch at Williamsport

run-off	rain	diff.	area
21.4"	40.3"	18.9"	5,682

Remainder above Harrisburg

run-off	rain	diff.	area
12.4"	42.0"	29.6"	2,268

Susquehanna River at Harrisburg

run-off	rain	diff.	area
18.1"	39.4"	21.3"	24,100

FIG. 4— A PORTION OF THE TABULAR FORM USED FOR COMPARING RUN-OFF WITH RAINFALL FOR SUB-DRAINAGE AREAS IN THE SUSQUEHANNA BASIN

unfortunately reliable records do not extend back many years, certainly not far beyond the time when the United States Geological Survey gave the great stimulus to the pioneer work. Rainfall-records extend further into the past, but as stated before contain many inconsistencies which may make trouble. It is natural to expect that this composite will be freer from such defects and, therefore, may be more suitable for this purpose.

Passing now to the application that has been made of last year's study of rainfall, the same method has been very successfully used for the analysis of runoff-records. The plots show that the ratio that each gage bears to the standard is, as a rule, far more definite than in the case of rainfall, and the breaks in the curves are much more distinct. The next step is to combine the results of the two by means of the schematic diagram shown on page 25 of the 1930 report of the Pennsylvania Hydraulic Power Committee. Figure 4 is a sample taken from the general chart of the entire river-basin, somewhat simplified by the combining of minor sub-drainage areas.

Taking the drainage-area as a whole, the first sign of possible discrepancy in the determinations of average runoff is any radical departure from the general level of the others. The second step is to enter all values on a map to see whether the abnormal ones fit any apparent scheme, such as prevailing abundant runoff from any particular section. A third test to compare each remainder, or residual area lying above any gage but below others, with runoff for the lower gage. A fourth sign of trouble is when one residual area is materially different from the one next below it.

So much for runoff alone, the test is then made even more critical when we introduce rainfall, for by subtracting runoff from rainfall any error in runoff is accentuated by comparison. Again looking at the drainage-area as a whole, these differences run peculiarly uniformly and a plot of the geographic distribution shows rather well-defined trends, the departures from which immediately stand out and thus point to the gages that are in all probability giving false estimates of the total runoff.

It has been possible in the short time to give but a kaleidoscopic view of the field of endeavor that is open. It is hoped that this will at least give some idea of the interesting possibilities in the analysis of past records. It should emphasize the need of continuing effort to perfect methods of observation so that the secrets which are at present hidden will be unlocked as soon as it is possible to get sufficient data both in quantity and quality. It is only in this way that we may reach definite conclusions which must remain in doubt until that time.

(Note--Opinions expressed in the above paper are the author's and not necessarily those of the American Geophysical Union or of the Pennsylvania Water and Power Company.)

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DETERMINATION OF THE DEPENDABILITY OF RAINFALL-RECORDS BY COMPARISON WITH NEARBY RECORDS

J. B. Kincer

In all investigations involving precipitation-records it is of the utmost importance that the basic data accurately represent conditions that obtained through the years of record. Under the climatological service of the Weather Bureau, where observations are taken on a cooperative basis at literally thousands of stations, and through which millions of records have been collected and summarized during the past half century, we naturally would expect to find, here and there, some erroneous data, resulting possibly from faulty gage-exposure, lack of comparability through changes in gage-location, and even errors chargeable to the personal equation in observational work. While it is recognized that such basic errors do exist, it is believed that they are generally of much smaller magnitude and not nearly so frequent as some investigators have been led to believe. We believe in most cases that sharp changes in rainfall-trends are due to natural causes.

In an attempt to detect errors in records by comparison with data for nearby individual stations, or with an area-standard established from a combination of stations, the first question for decision, obviously, is the method to be used. Detecting errors by such standards of comparison is not nearly so simple a matter as at first may appear. It is fraught with great difficulties and many lurking pitfalls are by the wayside. Certainly the correction of a record should be undertaken only after every phase of the problem that might have a bearing on the matter has been thoroughly considered.

The method most extensively employed in recent years is that by Carroll F. Merriam in his study of records in the Susquehanna Basin. Here there was set up, from records found to be fairly consistent through the period of observations, a standard for the area, as a kind of yardstick for measuring the validity of other records. In the primary examination of data for the establishment of a "standard" Mr. Merriam found that the overwhelming majority of records showed inconsistencies or sharp breaks, here and there, because of which, as a general principle, they were looked upon with suspicion, on the assumption that a rainfall-record, to be dependable, should show reasonable consistency throughout its course. He says in a paper presented at the last meeting of this Union, "Not being able to ascribe sudden changes in average rainfall thus indicated by abrupt breaks in the general trend to natural causes, the most plausible explanation seems to be an artificial alteration in the conditions of observation." Is this assumption valid? As a matter of fact, an overwhelming majority of rainfall-records, not only in the Susquehanna Valley as found by Mr. Merriam, but in all other parts of the United States, and in every country of the World, whether made by volunteer observers or trained meteorologists, show sharp breaks from time to time through the period of years covered. In view of this, the question arises as to whether we should ascribe such breaks, in general, to natural causes--vagaries of rainfall-distribution--or to faulty observations. As this is a universal conditions wherever rainfall-records have been made, it would appear hardly logical to assume that all but a very few of the multiplied thousands of observers throughout the World have gone wrong.

Mr. Merriam in his primary or first test of validity of records for construction of a standard yardstick employed the well-known device of accumulated departures from normal from year to year. In 1923 Marvin examined the long-period rainfall-records of New England and elsewhere in the same manner, and found sharp breaks and marked variations in trends quite similar to those in the Susquehanna Basin (see Fig. 1). Marvin's paper appeared in the Monthly Weather Review of March 1923. We find such conditions not only in individual records, but in groups of records. Figure 2 shows for 12 long-record stations in the Great Basin, constructed from the accumulated departure-device, the same characteristic sharp breaks. Again, the trend may be a long-time one, such as illustrated in the lower part of this graph for eight long-record stations in Indiana.

Another method which shows, graphically, trend-tendencies, emphasizes sharp breaks in records, and is very convenient in comparing one with another, is the well-known moving-average device. This may be based either on the actual amount of rainfall or on the percentages of normal. The former shows trends and breaks irrespective of the long-time average, while the latter reduces the the data for the several stations to be compared to a comparable base. For our present purpose we have used the moving-average device as shown in the several figures to follow.

In the setting up of a standard we may question the validity of appraising the dependability of a record by its consistency as shown by the annual precipitation. We might ask what such consistency connotes. Annual rainfall-trends may be considered as composites made up of seasonal, or even monthly, trends. Naturally, if the seasonal trends are largely in opposition, one to another, the result will smooth out into a consistent annual. The record for Reading, Pennsylvania, was given a No. 1 rating because of this consistency. Let us examine this record more critically.

Figure 3 shows the seasonal trends for Reading. The grapn clearly indicates that the consistency in the annual is due largely to oppositions and inconsistencies in the several seasons of the year. Note the marked alternating convergence and divergence of the several trends, which smooth out into an annual consistency. Figure 4, showing the annual trend for the long record at Cumberland, Maryland, in conjunction with the seasonals, makes this point clearer. The Cum-

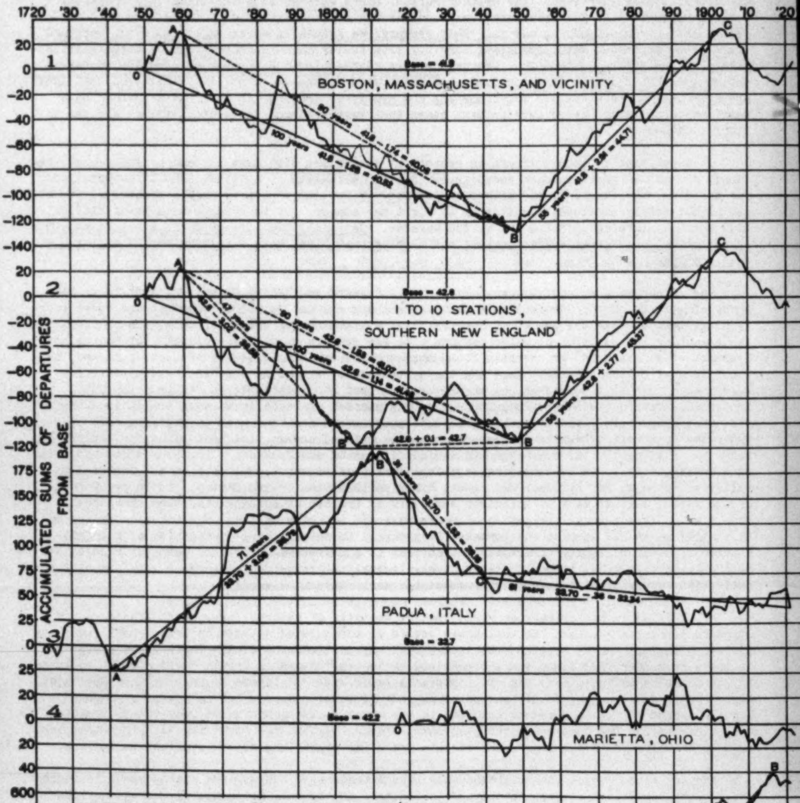


FIG. 1

berland record covers 66 years, but we have included only a portion to illustrate the matter it is desired to emphasize. Here we find, prior to 1889, a consistent trend in the annual, and also for a long period beginning about 1898. However, there is an intermediate period with a very sharp break, covering about ten years. An examination of the accompanying seasonal trends shows just why the annual was consistent during some periods and inconsistent during others. Note, for example, the almost complete opposition in the trends in summer and fall, beginning with 1898.

This brings us to a very pertinent question--shall we accept the period of consistent annual

Accumulated Departure of Precipitation From Normal

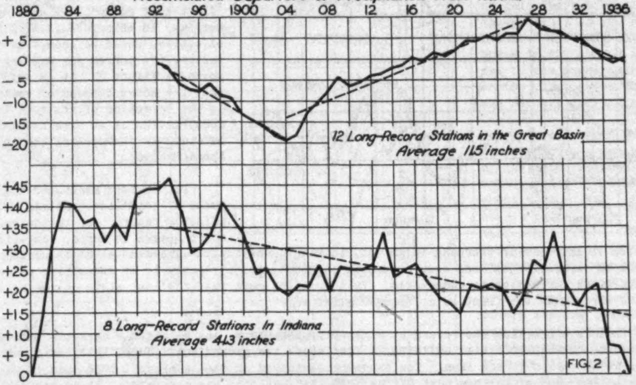


FIG. 2

READING, PA.

5-year moving averages of percent of normal precipitation

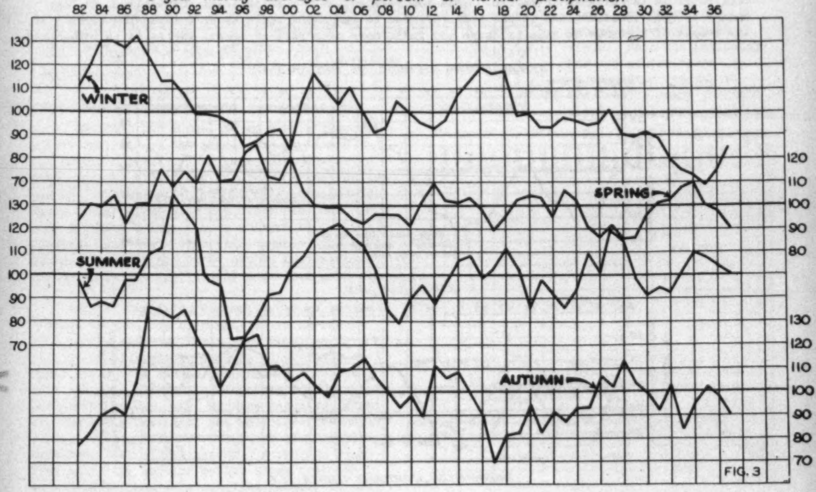


FIG. 3

5 Year moving averages of percentage of Normal Precipitation Cumberland, Md., covering consistent and inconsistent trends
Based on records for 66 Years, 1872-1937

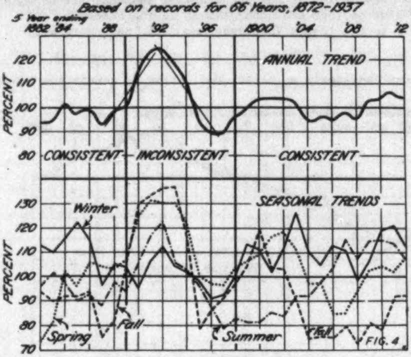


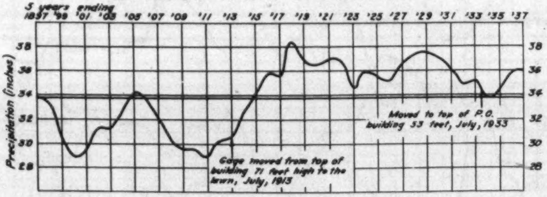
FIG. 4

trend as dependable when it is made up of inconsistent seasonal trends, and the other as unreliable? Or, in view of the fact that all seasons are in agreement in the latter case, should we accept it as real, with classification as "just a vagary in the distribution of rainfall?" Suppose we were examining the rainfall in summer or autumn instead of the annual, and were seeking records for a "standard" by the "first-test method," as used by Mr. Merriam, in which consistency is the objective, would we accept as satisfactory the curves for Reading as shown in Figure 3? It is emphasized that the above discussion refers only to the selection of stations for the construction of an area standard-trend, but this is fundamental in the Susquehanna Project.

It has been mentioned that every phase or condition that may naturally affect a record, say in the matter of sharp breaks in trend, long-time tendencies to increase or decrease in amount, lack of quantitative agreement with its neighbors, etc., should be carefully examined before passing judgment, and certainly in an exhaustive manner before an arbitrary correction is undertaken. This is to be sure that the last stage of the record may not be worse than the first in a representative way.

In this connection the record at Binghamton, New York, may be cited. Here the rain-gage was exposed on the top of a high building 71 feet above the ground until July, 1913, when, after a fire, it was moved to the lawn. The exposure continued on the lawn until 1933 when it was again moved to the roof of the new Post Office when that building was completed. It so happened that immediately after removal from the roof to the lawn in 1913 there was a marked increase of rainfall shown in the Binghamton record. This was related, with a superficial examination, to the change of gage-location, and it has been referred to again and again as an outstanding example of the result of a change in gage-exposure and faultiness of a roof-exposure. While it is recognized

5-YEAR MOVING AVERAGES OF PRECIPITATION



BINGHAMTON, N.Y.

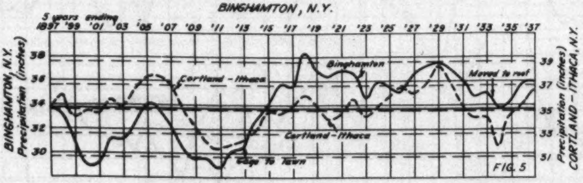


FIG. 5

BINGHAMTON, CORTLAND-ITHACA, N.Y.

that rain-gages should be exposed on the ground for best results, upon a careful examination it has been found that the change in location in this case had little to do with this marked increase in rainfall. It was a natural phenomenon.

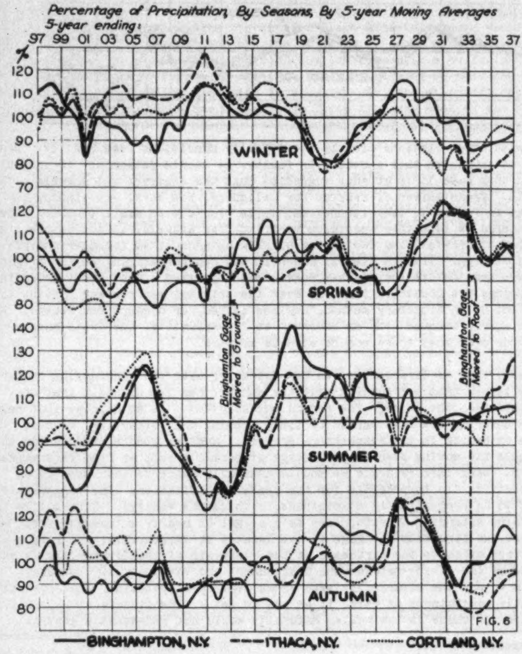


FIG. 6

Figure 5 compares the trend at Binghamton with that of the combined records at Cortland, near Binghamton in the same valley, and at Ithaca, not far away. A glance at this chart at least leaves doubt as to the assumption that the sharp break in the record was due to the cause ascribed. In the first place, there was a sharp decline in the Binghamton record while the gage was still on the roof, and, as a matter of fact, the recovery had actually set in before removal to the lawn. In the second place, it is noted that since return to a roof-exposure in 1933 up through 1937 there has again been a rather sharp upward trend. However, before passing judgment, our examination should go further than this. Figure 6 shows the seasonal trends separately for the three stations under comparison, Binghamton, Cortland, and Ithaca. A marked agreement is obvious. Note the following: For the winter there is shown, after removal to the lawn, a sharp downward trend in all three stations; also, clearly, the annual trend was influenced almost entirely by the conditions in summer. Why should removal of the gage show a downward trend for the winter and an upward trend for the summer? The validity can not be questioned because of agreement with the other records. In view of these things, there appears to be little question that the severe indictment of the Binghamton record is not borne out by the evidence in the case. It was largely a natural phenomenon.

Another cause for suspecting or rejecting records is the lack of quantitative agreement with nearby records. This also requires very careful consideration, because topographic or other influences may be responsible. We shall note two such cases brought to light in the Susquehanna study. Mr. Merriam rejected, rightly, the early period of the Wellsboro, Pennsylvania, record, because of lack of quantitative agreement with others nearby. A cursory examination of the tabular matter shows that the questionable data were confined largely to the winter season. A charting of the records for

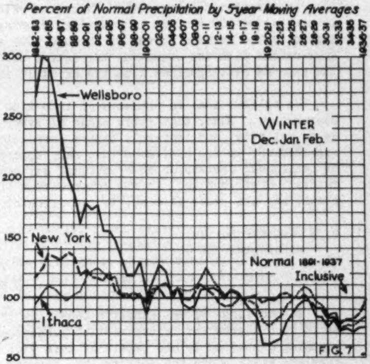


FIG. 7

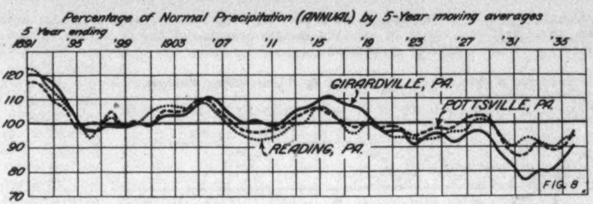


FIG. 8

this season in comparison with other stations clearly shows faulty conditions (see Fig. 7). However, it is not sufficient to establish that a record is faulty; we should determine, if possible, wherein the fault lies. In this case it is at once suggested that the observer had trouble with his measurements of snowfall. Trend-curves of records for Wellsboro, New York, and Ithaca, for the winter seasons are given in Figure 7; they further emphasize probable snowfall trouble. When we refer to the original records we find this suspicion verified; for example, if the observer had in January, say, seven inches of snowfall and two inches of rain he added them together and reported nine inches of precipitation. This is clearly shown in his own handwriting on the original records, which, however, were made before the Weather Bureau was organized. What shall we do in a case of this kind? Two things are possible, first go over the original records and convert the snowfall into water-equivalent by an arbitrary method, say one to ten, or discard the record. Mr. Merriam did the latter. We agree, because with an observer so irresponsible, one could hardly have confidence in his records even when there was no snow to measure.

However, in another case, that of Girardville, we can not agree with the conclusions reached. Here again the amounts were larger than surrounding stations and Mr. Merriam suggests that measurements of snowfall may have been the cause. The record was adjusted. It so happens that near Girardville are two records covering the same period of time, Reading and Pottsville, both of which are given clear bills of health in the Susquehanna Project. When we reduce these three to a comparable basis and compute the moving average percentage of normal trend, we find very marked agreement, shown in Figure 8. With this evidence we should naturally conclude that some topographic or other natural condition is responsible for the heavy precipitation. The topographic map of this region shows it to be predominantly mountainous, with narrow valleys. Girardville is in such a valley with mountains extending on both sides to a height of nearly a thousand feet above the valley-floor. Also, we find that the gage is not located at Girardville, but rather back in the mountains some four miles to the northeast at the reservoir of the Girardville Water Company, and within one mile of the divide between the Susquehanna and the Schuylkill basins. This answers our question as to why the Girardville gage shows heavy precipitation, and after all for a country as predominantly mountainous as this, if we are to get a true picture of rainfall, we need some mountain stations as those in the valley naturally would not represent a general average for the region.

In a recent paper entitled "Notes on rainfall in the Susquehanna Valley," July 21, 1937, a tabulation of 200 records is presented showing, among other things, the average rainfall as published by the Weather Bureau up through 1930 and adjusted data based on the comparison of the gage-record with the standard, the latter said to be "the best estimate of the true catchment of the gage as distinguished from the rainfall that would be expected for the locality."

We believe that in projects of this kind the climatologist and hydrologist should work in cooperation. Also, that every phase of every record should be examined before judgment is passed as to validity, and certainly before corrections are applied arbitrarily. With such a thorough examination, we are convinced that many apparent inconsistencies, such as above discussed, would be cataloged, instead of observational deficiencies, as "just patches in the crazy-quilt of nature's rainfall-distribution," in the construction of which she is a past master, a condition all too well known, for his peace of mind, to the climatologist.

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Washington, D. C.

DISCUSSION

At the close of Mr. Kincer's paper the presiding officer called upon Mr. Merriam for comment. He in turn stated that after careful consideration of Mr. Kincer's presentation he did not retract anything contained in his papers of this year and the year before. On giving this matter more complete study since the meeting, Mr. Merriam is still of the opinion that in spite of the wide divergence in views expressed they can, nevertheless, be satisfactorily reconciled.

WIND-SHIELDS FOR PRECIPITATION-GAGES

Charles F. Brooks

The aim of a precipitation-gage is to obtain a sample of the rain or snow that falls on to the ground. Thus, a gage with its orifice flush with the ground would be ideal were it not for the splash from the ground near by, the drifting of snow, or occasional floodings or burying. The splash can be eliminated by surrounding the gage with a sufficiently extensive surface of honeycomb or brush or fine wire-netting; but the drifting of snow and flooding or burying can be only partially controlled, and with difficulty. So, the standard practice is to elevate the orifice of the gage above the splash (one foot) or above most of the drift (three or more feet). But this introduces a new problem: the disturbance of the falling precipitation, especially when snow, by the wind-eddies over the gage.

When the decrease of caught precipitation with height of the gage above the ground was found to be due to the wind various means of sheltering or shielding gages were adopted. Placing a gage a moderate distance to leeward of a hedge, fence, grove, or building, enough to be once or twice as far from the object as it is high, "improves" the catch over that in an exposed position, usually making it closer to the true precipitation. Such shelters are subject to change of height (growth of trees) or destruction, and they are not homogeneous; therefore, some kind of a standard wind-shield attached directly to the gage is, at least nominally, preferred.

F. E. Nipher, of St. Louis, devised a trumpet-like shield in 1878, which 60 years of experimenting with other forms has failed to improve upon, except in minor details. Tests in wind-tunnels as well as experiments in the open have proved the superiority of a shield of the Nipher type over other forms. The open flare of the trumpet is exposed upward, with the precipitation-gage inside it and with its lip level with the outer edge of the trumpet. In this position, that portion of the wind that would strike the gage is diverted downward and around the base of the shield and gage, leaving essentially undisturbed that part of the wind through which the rain or snow is falling just before it reaches the gage. The widest part of the flare need be no more than 12 inches from the gage.

Experiments with various forms of Nipher shield for several years on Blue Hill and Mount Washington have shown that gages adequately protected from the wind thereby can, even in strong winds, catch within ten percent under what appears to be the true snowfall, and perhaps within two per cent of the true rainfall. An unshielded gage may be 75 per cent or more deficient in snow catch and five or ten per cent or more off in rain-catch.

Ten different designs of shields have been tried, of which three were decided failures, and only one--the last--was a real success. This one was the nearest to Nipher's design!

The foregoing is essentially an abstract of a comprehensive paper by the author, including extensive annotated bibliography, on the "Need for universal standards for measuring precipitation, snowfall, and snow-cover." This paper, now in press, constitutes pages 7 and following of the Transactions of the International Commission of Snow and Glaciers, Edinburgh, September, 1936. Reprints have been ordered, and copies will be sent to members of the Union on request.

The following general summary of "standard" Nipher shields and specifications of their common features and a description of a new "standard" which seems to be an improvement over any yet used may be of interest to American observers.

Note on "standard" Nipher shields

There are at present several types of so-called standard Nipher shields. The most widely used are the Russian and Norwegian types which are 30 inches in diameter and slope 45° or a little more from the horizontal closing around the base of the gage. The top of the shield is 12 inches from the edge of the six-inch gage it surrounds, a distance considered sufficient to prevent splash from breaking rain-drops from getting into the gage. Kurmangalin at Omsk used this shield with an additional ring of sheet-metal extending horizontally, approximately 12 inches outward from the edge of this shield. Billwiller's Swiss type is 27-1/2 inches in diameter and 19-1/2 inches high. It slopes steeply 52° in order to provide a large discharge-opening for snow at the bottom, this opening being 18 inches in diameter about a six-inch gage. Salter of Great Britain modified this shield by reducing its vertical height to ten inches and the lower collar to a diameter of eleven inches about a five-inch gage. The slope of Salter's pattern is about 50° from the horizontal. The top of his shield is nine inches (nearest point on slope, 6-1/2 inches) from the edge of the gage, and is considered sufficient to prevent splash reaching the gage.

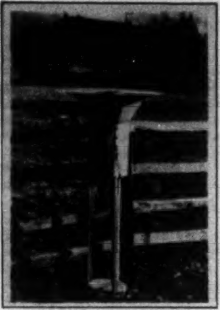

FIG. 1—HARVARD BLUE HILL OBSERVATORY 1938 TYPE OF MODIFIED NIPHER WIND-SHIELD FOR PRECIPITATION-GAGES FOR STANDARD 8-INCH GAGE OF U.S. WEATHER BUREAU

Nipher's original shield had eight inches from the top of the gage to the top of the shield (5-2/3 inches to nearest point), a distance considered insufficient to prevent splash reaching the gage.

In consequence of the author's experiments which gave reduced snow-catches, even with a cone having an angle of attack of but 35°, it seems from the results of wind-tunnel experiments that a horizontal ring is essential if the air-currents across the gage are to be made to move horizontally and without much turbulence.

Specifications for an improved wind-shield

Outer horizontal rim--Not less than three inches wide (Nipher), not more than 12 inches on large shield (Kurmangalin after wind-tunnel tests by Bastamoff), equivalent to eight inches reduced to relative size of original Nipher shield 25 inches in diameter. Four or five inches seems a reasonable compromise. The material must be of solid sheet-metal to prevent the wind from blowing through it. To prevent splash, fine screening can be placed a small distance above the sheet-metal. But since this would obstruct the wind, it seems best to slope the outer ring by a small amount, say, 10° downward toward the gage, which will provide space for the screening below the level of the top of the shield, and, indeed, may obviate the need for screening since rain would never strike it nearly vertical on the windward side of the gage. Also, such a flat cone is much more rigid than a horizontal ring.

Nearness of shield to orifice of gage--No part closer than 5-2/3 inches unless covered with raised screening or brush (Nipher found this necessary at 5-2/3 inches). Salter considered it unnecessary at 6-1/2 inches. In view of the reduced splashing possible from a shield sloping 60° from the horizontal, a distance of 5-2/3 inches of this shield from the orifice may be permissible without screening.

Slope and extent of main body of shield--Not less than 35° from horizontal (Brooks), probably not less than 52° (Billwiller) or 60° or more. If the shield and gage are to be placed perpendicular to a hill or mountain slope, the shield must be steep so that even on the downhill-side it can still discharge snow. To prevent up-currents on the windward side of the gage the shield must be carried down to the base of the gage (Billwiller).

Discharge space for snow--Not less than one inch all clear; two inches found ample for 35°-sloping shield 30 inches in diameter. With steeper slopes discharge need not be so large, though Billwiller's has nearly six inches. So 1-1/2 inches seems adequate for steeply sloping shield five to ten inches in width and having but slight obstruction in the discharge-space.

Knocker--Needed to keep snow moving out of shield. (Wind-vibration of whole shield relied on by Lütschg.)

Height--Bottom at least eight inches above the surface; top at least three feet above a snow-surface, to be above most drift.

The Harvard Blue Hill Nipher shield of 1938

A shield will now be described which appears to be the closest compromise of all the desiderata above listed for the protection of a cylindrical gage eight inches in diameter and 23-3/4 inches high, and which presents no more obstruction to the wind in its upper ten inches than would a Nipher shield enlarged by one-fifth to increase it from its original diameter of 25 inches to the 30-inch diameter of the proposed shield.

Its over-all diameter at the top is 30 inches and at the bottom 11 inches. It stands 23-3/4 high, the same as the standard United States Weather Bureau cylindrical snow-gage eight inches in diameter. The shield and gage included should be fastened to legs which will bring its base to a height of at least 12 inches from the ground or accumulated snow on the ground, so that there will be free passage under the shield and gage for the wind diverted by the shield. Also, this height will insure against the catch of drifting snow, except on very windy occasions. Angle irons (1 1/4-

inch), or wood of the same thickness, would provide a snug hold for the gage inside the lower pipe-portion of the shield.

In detail, the shield consists of an outer nearly horizontal rim of sheet-metal, 5-3/32 inches wide, sloping 10° from horizontal toward the gage, the outermost circle of this rim being 30 inches in diameter. To its inner edge is attached a truncated cone of sheet-metal nine inches wide, extending downward from the horizontal at an angle of 60°. The upper, wider, diameter of this cone is 20 inches and the lower, 11 inches. The lower edge of this cone is fastened to a vertical pipe 11 inches in diameter and 15 inches high.

To hold these parts together, six pieces of strap-iron are used, evenly spaced around the circle, running from the outermost edge of the horizontal rim to the bottom of the pipe. The supporting legs, three in number, extend from the top of the vertical pipe downward, and are fastened on the inside.

Hanging from each angle iron, at a junction of the flat rim and the sloping cone, by a cord or chain five inches long, are six disks of wood about one-half inch thick and four inches in diameter, fitted with wind-catching vanes below, the function of which is to tap the shield when blown by the wind, thereby tending to joggle loose any snow adhering inside the cone. In a wind of five to ten meters per second, snowflakes showed that the wind was well diverted downward under the lip of the shield, and snow fell readily into the gage without apparent disturbance.

In summer, at least, the flat rim should be covered, at a height of half an inch or more over the innermost portion, by wire-screening with mesh of at least eight to the inch. This screening may be continued horizontally out over the steeply sloping portion of the shield or sloped down into the shield.

If a gage of smaller or larger body-diameter is used, the shield may be made smaller or larger in proportion, and the height can vary with the height of the gage. However, the distance of the inner edge of the flat outer rim should not become less than 6-1/2 inches, nor the snow-discharge space less than 1-1/2 inches for use where snow falls.

Precipitation-measurements with the 1938 shield

A shield of this description, except that it is 32 inches in diameter and at a height of six feet above the ground, was installed on the summit of Blue Hill on March 19, 1938. In the period to April 30 the eight-inch gage that it protects caught 3.18 inches of precipitation while 20 feet away another eight-inch gage, height two feet, with a Nipher shield 56 inches in diameter caught 3.22, or one per cent more. Of the 22 twelve-hour periods of measurable precipitation the difference between the two gages was 0.02 twice and 0.01 four times. Although two cases of snowfall were included, comparative observations will be continued through a greater range of rainfall-intensities and wind-velocities this summer and into the snow-period of next winter.

Another shield of this type, 30 inches in diameter and about an eight-inch gage at five feet above the ground, was exposed in an open pasture at the base of Blue Hill (Fig. 1) at a distance of 20 feet from another eight-inch gage at five feet surrounded by a shield 36 inches in diameter with a three-inch horizontal outer rim and a cone 30 inches in diameter sloping at 35° from the horizontal downward toward the 12-inch snow-discharge pipe at the bottom of the shield. A double layer of 1/4-inch mesh rat-wire covered the top of the latter shield to two inches from the rim of the gage. From March 20 to April 18 the catch of the new shielded gage was 2.03 inches and of the other 2.01 inches, omitting one storm during which the rat-wire became solidly covered with wet snow and sleet, so that sleet bounced from this surface into the gage. The 1938 shielded gage on Blue Hill for the same periods caught but 1.65 inches, or 19 per cent under the catch at the base, a difference which must be largely owing to a difference in precipitation at the two stations during this period of predominantly northeasterly-wind rainfall. The base-station, which is three-fourths of a mile northwest of the summit, would be favored by a tendency for the northeast wind to diverge over the summit and to converge in passing around the base of the hill. The lesser wind at the base would also favor a better catch and reduced evaporation. Though there might have been more insplashing from the one-inch closer shield at the base-station, the consistency between the two gages at the base precludes much error on this score. Similarly, the essential agreement among the five gages exposed on the summit bars any idiosyncracy of the 1938 shielded gage there as being responsible for its lower catch.

The base-station has been discontinued until next winter, and the shield is being sent to the summit of Mount Washington for comparisons there.

Should any reader wish to make such a shield, detailed notes on materials, cutting and drilling and assembly, with construction diagrams, by H. G. Dorsey, Jr., of the Blue Hill staff, will be sent on request. Drawings of this shield, including removable anti-splash screening, have been made by the Soil Conservation Service, Coshocton, Ohio.

Harvard University, Blue Hill Observatory,
Milton, Massachusetts

RESULTS FROM EXPERIMENTAL RAIN-GAGES AT COSHOCTON, OHIO

Herbert S. Riesbol

Satisfactory shields and enclosures for the standard United States Weather Bureau type of rain-and-snow gage are essential to the accurate measurement of precipitation by gages placed in open areas. By using a limited number of widely distributed gages, it is usually possible to place them in properly sheltered sites. However, when the number and distribution-density of the gages are increased, it is often extremely difficult to find suitable exposures.

In connection with the watershed-studies being conducted by the Soil Conservation Service of the United States Department of Agriculture near Coshocton, Ohio, where rain-and-snow gages are distributed at intervals of about one-third mile over privately owned watersheds of several thousand acres area, many gages are necessarily exposed directly to wind-action. In order that equipment may be developed for improving the catch of gages so exposed, an experimental plot has been established in the watershed-area and equipped with several standard rain-and-snow gages on which various types of shields and enclosures are being tested.

Certain basic conditions of occurrence of precipitation were kept in mind in selecting the various shields and enclosures: (1) Rain falling practically vertically without influence of wind; (2) rain striking the gage at various angles due to the influence of wind occurring at various velocities and coming from any direction (this included, of course, the condition of rain falling in highly turbulent air); (3) rain falling when temperatures at the gage are below freezing, causing the rain to freeze when it strikes the gage; (4) frozen rain such as hail and sleet, subject to wind-conditions mentioned above; (5) rain falling at such high intensities as to cause excessive splashing within the gage-receiver; (6) snow falling with different degrees of "dryness" or moisture-content causing it to adhere to the gage-receiver or shield in different ways; (7) snow falling subject to wind of different velocities and from different directions and also to various degrees of air-turbulence.

It was decided at the beginning of the rainfall-studies at Coshocton that the United States Weather Bureau non-recording rain-and-snow gage would be adopted as a standard since local measurements could then be correlated with other records obtained in the United States.

A search of the literature was made to determine what types of shields and enclosures had been tested by previous investigators, and those which had received greatest recognition were selected for initial study. Practicable considerations of distributing rain-gages over the large watersheds required that each gage be surrounded by a fence to prevent damage by stock and that each fenced site occupy the smallest possible land-area so as not to interfere with the operations of the individual farmers. Because of this it was decided that the standard enclosure for a standard non-recording rain-gage would consist of a woven-wire fence four feet high. placed on the sides of an eight-foot square. Each gage is placed in a metal tripod which makes it possible to level the receiver of the gage and place the receiving edge about 36 inches above the ground-surface.

A general view of the layout of the experimental gages in the meteorological plot may be gained from the accompanying panoramic photograph (Fig. 1). A description of the individual gages follows:

MA--This is a standard gage and tripod placed at the center of a square plot which is 24 feet on each side. A tight board-fence, four feet in height, is constructed on the sides of this plot. The object of this enclosure is to cut ground-turbulence to a minimum (see lower left corner, Fig. 1).

MB--This is the standard field-unit consisting of non-recording gage, tripod, and woven-wire enclosure (see right center, Fig. 1).

MC--A masonry lined pit, four feet in diameter and three feet deep, serves as the enclosure for the standard non-recording gage on tripod. When the receiver is in place its top is at the center of the pit and flush with the surface of the ground (see Fig. 1). The object of this type of enclosure is to give a true picture of the actual precipitation reaching the ground-surface and to provide maximum freedom from air-turbulence in the boundary-layer at the Earth's surface.

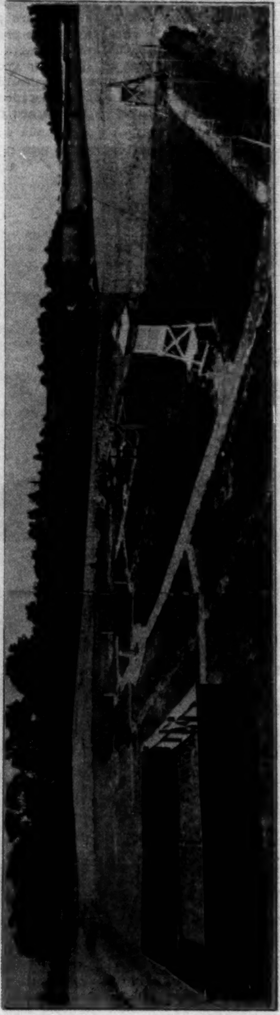

FIG. 1—PANORAMIC VIEW OF METEOROLOGICAL PLOT

MD--The construction of the enclosure for this gage consists of a pit four feet by five feet in surface-area and three feet deep. The gage is placed at the center of the pit with the top of the receiver flush with the ground-surface. The opening which surrounds the receiver is covered with a cocoa-brush mat to simulate natural sod and to prevent the splash which would occur with a tight and hard cover of lumber or metal. A narrow opening is left between the edge of the mat and the circumference of the receiver to facilitate drainage of water which may accumulate on the surface of the mat (see Fig. 2).

ME--This is a standard non-recording gage in a standard tripod and has neither enclosure nor shield (it is between the Fergusson recording gage and the pit-gages in Fig. 1).

MF--This is a standard non-recording gage in tripod and without enclosure (see Fig. 3). The receiver contains a multiple vane-type shield as developed by Hall. The object of the vanes is to present an approximately uniform catchment-area to precipitation carried by strong winds.

MG--This gage is identical with MF with the exception of slots which have been cut in the vertical receiver-vanes with a view to reducing the buffer-action inherent to the vertical vanes (see Fig. 4).

MH--In this case the standard non-recording gage is equipped with an adaption of Nipher shield. There is no enclosure around the gage. The object of the shield is to convert cross-currents and updrafts into reverse eddy-flows which will create a turbulent but relatively calm region above the gage-receiver (see Fig. 5).

MRr--This is the standard Fergusson weighing and recording rain-and-snow gage. Neither shield nor enclosure is used (see Fig. 6).

All the above-mentioned gages were placed in operation by the first of June 1937. On November 1, 1937, the receivers were removed from all gages for snow-measurements and at the same time the vane-type shields and cocoa-mat were removed. The data reported upon in this article will therefore be limited to the rainfall-measurements made during the five months ending October 31, 1937.

In Table 1 is listed the total catch of each of the experimental gages for each month of the five-month period and the total catch of each gage for the entire period. The arithmetic mean of the catch of all gages for each month is also presented. Examination of the total catch of each brings out the fact that the gages fall into two distinct groups. Into one group falls all the gages which have shields installed around the receiver or which are installed in pits--the catch of precipitation by each of the gages in this group being higher than the average catch of all the experimental gages. Into the second group fall those gages which have no protection at all and those which are protected only by fences of various types--the catch of precipitation by each of the gages in this group being below the average catch of all the experimental gages. The average catch of the pit and shielded group is 24.12 inches for the five-month period as compared to 22.02 inches for the non-shielded group. This difference of 2.10 inches between the average catch of the two groups is larger than the difference between any pair of gages in the same group and it is therefore probable that the difference between groups is significant and attributable to methods of gage-protection. This condition is also emphasized by the fact that in no case does the average catch of a gage protected by shield or pit fall into the low group nor is the same true of the other group. Gage MB represents the standard non-recording field-installation while MC--the open pit--should theoretically measure the actual precipitation at ground-surface. On this basis it was decided that the deviation of catch of each of the experimental gages would be computed by months and by storms from gages MB and MC.

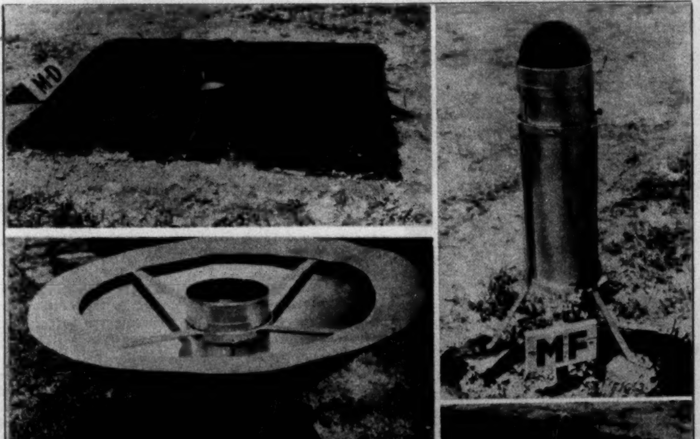

FIG. 2—MD, STANDARD RAIN-AND-SNOW GAGE IN PIT WITH COCOA-BRUSH MAT COVER FIG. 3—MF, STANDARD RAIN-AND-SNOW GAGE EQUIPPED WITH HALL VANE-TYPE SHIELD FIG. 4—MG, STANDARD RAIN-AND-SNOW GAGE EQUIPPED WITH SLOTTED-VANE SHIELD FIG. 5—MH, STANDARD RAIN-AND-SNOW GAGE EQUIPPED WITH NIPHER SHIELD FIG. 6—MRF, FERGUSSON RECORDING RAIN-AND-SNOW GAGE

Table 1--Rainfall in inches for each gage by months
(Underlined values indicate that the precipitation as measured by the given gage for that month is over the average for the month as obtained from all the experimental gages)

Month	Board-fence enclosure	Standard wire enclosure	Open pit	Cocoa-mat pit	No pit, no enclosure, no shield	Hall shield	Slotted Hall shield	Nipher shield	Fergusson recorder	Arithmetic average
	Gage									
	MA	MB	MC	MD	ME	MF	MG	MH	MRr	
Jun.	8.80	8.56	10.33	10.93	8.71	9.60	9.39	9.46	8.29	9.34
Jul.	5.86	5.73	6.77	6.43	5.99	6.59	6.32	6.05	5.77	6.17
Aug.	2.70	2.63	2.78	2.58	2.74	2.64	2.60	2.92	2.67	2.69
Sep.	0.94	0.90	0.94	0.93	0.90	0.92	0.86	0.94	0.87	0.91
Oct.	4.13	4.03	4.32	3.80	3.96	4.30	4.12	4.09	3.89	4.07
Total	22.43	21.85	25.14	24.67	22.30	24.05	23.29	23.46	21.49	23.18

Table 2--Deviation of catch in inches of each gage by months from MB, standard wire enclosure

Month	Item	Gage							
		MA	MC	MD	ME	MF	MG	MH	MRr
Jun.	Rain	8.80	10.33	10.93	8.71	9.60	9.39	9.46	8.29
MB = 8.56	Deviation, D	+0.24	+1.77	+2.37	+0.15	+1.04	+0.83	+0.90	-0.27
	D in per cent	2.80	20.67	27.68	1.75	12.15	9.69	10.51	3.15
	Average deviation[a] = 0.946 inch or 11.05 per cent								
	Standard deviation = 1.200 inches or 14.01 per cent								
Jul.	Rain	5.86	6.77	6.43	5.99	6.59	6.32	6.05	5.77
MB = 5.73	Deviation, D	+0.13	+1.04	+0.70	+0.26	+0.86	+0.59	+0.32	+0.04
	D in per cent	2.27	18.15	12.21	4.54	15.01	10.29	5.58	0.70
	Average deviation[a] = 0.492 inch or 8.59 per cent								
	Standard deviation = 0.596 inch or 10.40 per cent								
Aug.	Rain	2.70	2.78	2.58	2.74	2.64	2.60	2.92	2.67
MB = 2.63	Deviation, D	+0.07	+0.15	-0.05	+0.11	+0.01	-0.03	+0.29	+0.04
	D in per cent	2.66	5.70	1.90	4.19	0.38	1.14	11.12	1.52
	Average deviation[a] = 0.094 inch or 3.58 per cent								
	Standard deviation = 0.127 inch or 4.82 per cent								
Sep.	Rain	0.94	0.94	0.93	0.90	0.92	0.86	0.94	0.87
MB = 0.90	Deviation, D	+0.04	+0.04	+0.03	000	+0.02	-0.04	+0.04	-0.03
	D in per cent	4.45	4.45	3.33	000	2.22	4.45	4.45	3.33
	Average deviation[a] = 0.030 inch or 3.33 per cent								
	Standard deviation = 0.032 inch or 3.55 per cent								
Oct.	Rain	4.13	4.32	3.80	3.96	4.30	4.12	4.09	3.89
MB = 4.03	Deviation, D	+0.10	+0.29	-0.23	-0.07	+0.27	+0.09	+0.06	-0.14
	D in per cent	2.48	7.28	5.71	1.74	6.70	2.24	1.49	3.48
	Average deviation[a] = 0.156 inch or 3.88 per cent								
	Standard deviation = 0.179 inch or 4.44 per cent								

[a] Without regard to sign

In Table 2 the plus or minus deviations of each of the experimental gages from gage MB for each of the five months have been computed. These deviations, which are in inches of rainfall, have also been converted to deviation in per cent. The average wind-velocity in miles per hour for each day of the four months, July, August, September, and October, is presented in Table 3. These data were obtained from a three-cup anemometer placed 18 inches above the ground-surface in the same plot with the experimental rain-gages. Unfortunately the anemometer was not in place during the month of June and the velocity-records therefor cannot be correlated with the critical storms which occurred during that month. Table 4 contains the same information as Table 2 except that deviations are computed with reference to the open gage MC.

It is believed that the study of monthly and seasonal totals of precipitation as measured by the various experimental gages will not tell the full story of their action since the differences produced by meteorologic conditions peculiar to each storm would be ironed out. Six storms were therefore selected as typical of the major storm-types which occurred during the five-month period. Velocity and direction of wind for the days on which these storms occurred are listed in Table 3. For information concerning the duration of storm-rainfall, and the rate and duration of rate of rainfall during the three most intense periods of each storm, see Table 5.

Table 3--Average wind-velocity and prevailing direction per day in miles per hour

Day	Jul.		Aug.		Sep.		Oct.	
	Vel.	Dir.	Vel.	Dir.	Vel.	Dir.	Vel.	Dir.
1	5.9	NW	4.0	N	2.0	SE	3.4	W
2	8.8	SW	2.4	E	2.5	SW	3.6	NE
3	2.2	W	1.8	NW	2.8	S	4.2	E
4	4.8	SW	4.2	S	2.1	S	5.0	SE
5	2.7	W	1.4	SW	4.6	NE	5.5	SW
6	3.4	NE	1.8	SW	4.4	E	6.6	W
7	1.8	W	3.0	SW	2.7	NW	8.1	W
8	2.2	SW	3.3	S	2.5	SE	8.3	W
9	6.6	SW	3.2	SW	2.0	N	2.2	E
10	3.6	SE	1.4	SW	4.0	S	3.6	W
11	7.2	SW	10.4	W	6.7	NW	3.0	S
12	2.4	NW	4.6	W	4.2	NW	8.5	SW
13	2.3	E	4.6	NE	7.3	N	5.5	N
14	4.2	SW	1.8	W	5.2	W	6.8	NW
15	7.3	SW	4.8	E	6.6	W	3.6	NW
16	9.0	SW	1.7	S	4.6	NW	5.7	SE
17	7.5	W	3.8	S	4.0	NW	5.6	SE
18	3.9	NW	1.2	N	5.4	SW	9.6	SE
19	2.2	E	3.0	SW	6.4	N	7.9	S
20	4.2	NE	3.7	W	3.7	W	10.2	SW
21	2.6	E	3.0	SW	4.2	E	8.6	W
22	2.4	SW	3.0	W	2.7	S	3.3	N
23	3.6	S	2.5	NE	3.1	S	9.0	NW
24	1.8	S	2.8	N	3.4	S	9.4	SW
25	6.0	S	4.6	E	5.1	S	5.1	S
26	7.8	SW	1.0	NE	5.7	N	8.8	S
27	3.6	S	3.4	N	8.5	NE	10.0	N
28	2.6	W	2.7	E	3.4	NE	9.4	N
29	3.4	SW	3.6	E	3.6	SE	5.4	S
30	2.0	SW	3.5	S	4.3	S	4.7	W
31	2.8	SW	2.2	SE	---		4.2	N
Average	4.2	SW	3.2	SW	4.3	S	6.3	W

In the following paragraphs the results from each gage will be briefly discussed with reference to the data listed and described herein.

MA, board-fence enclosure--In total rainfall for the five-month period this gage was typical of the other non-shielded gages in that its total catch was below the average of the nine experimental gages. However, for the last three months of this period it is noted that the catch of this gage was in each case slightly above the average (see Table 1). For the five months the monthly deviations of MA from MB were positive in sign and varied between 2.27 and 4.45 per cent, which can hardly be considered as a significant variation. For the six storms on which data are herein presented, it is noted that the variation of MA from MB ranged between zero and a positive 5.92 per cent. On the basis of these data it may be said that the action of the board-fence enclosure is practically identical with that of the standard-wire enclosure. It is expected, however, that results from snow-measurements during the winter may produce more significant differences.

ME, no shield or enclosure--This gage is also included in the group of non-shielded gages whose total catch for the five months was below the average of all experimental gages. In only one of the five months did it slightly exceed the average (see Table 1). The deviations of ME from MB for the five months varied from negative 1.74 to positive 4.54 per cent while for the six storms these deviations varied between negative 3.70 and positive 7.40 per cent. On the basis of these data it appears the gage without either enclosure or shield and the two fenced gages MA and MB all three gave results which cannot be said to possess significant difference one from the other.

MRr, Fergusson recorder--The catch of this gage was the lowest of all the nine experimental gages for the five-month period and for the monthly totals and storm-totals was consistently near to being the lowest. The deviations of MRr from MB for the five months varied from negative 3.48 to positive 1.52 per cent while for the six storms these deviations varied between negative 5.55 and positive 2.50 per cent. It may probably again be said in the case of this gage that the deviations are not significant and that the results from gages MA, MB, ME, and MRr may be considered as equivalent. It has been suggested, however, that the rather consistent low catch of gage MRr may be explained in part by the sloping shoulders which surround the gage just below the receiver (see Fig. 6), it being possible that these shoulders cause updrafts which force some rain away from the mouth of the receiver. This same tendency of the Fergusson recording gage to be low in catch has been noted in our numerous field-installations where it is set side by side with a non-recording gage in a woven-wire enclosure.

MC, open pit--The catch of rainfall by this gage was the largest of any of the experimental gages for the five-month period. For each month and for each storm the catch of this gage always ranked either first or second. The deviation of the catch of this gage from that of the standard wire-enclosed gage, MB, varied from a positive 4.45 to a positive 20.67 per cent for the five monthly totals. For the six storms the deviation varied from positive 1.85 to positive 57.78 per cent. The unusual deviation of 57.78 per cent occurred for the storm of June 14 and 15 when the maximum rainfall-intensity of the year was reached at 4.20 inches per hour for a ten-minute period and which was also accompanied by a rather high wind. Another feature of the catch of this gage was the consistency with which it maintained its relative position of either first or second in

Table 4--Deviation of catch in inches of each gage by months from MC, open pit

Month	Item	Gage							
		MA	MB	MD	ME	MF	MG	MH	MRr
June	Rain	8.80	8.56	10.93	8.71	9.60	9.39	9.46	8.29
MC = 10.33	Deviation, D	-1.53	-1.77	+0.60	-1.62	-0.73	-0.94	-0.87	-2.04
	D in per cent	14.81	17.13	5.80	15.68	7.06	9.09	8.42	19.75
	Average deviation[a] = 1.262 inches or 12.22 per cent								
	Standard deviation = 1.36 inches or 13.17 per cent								
July	Rain	5.86	5.73	6.43	5.99	6.59	6.32	6.05	5.77
MC = 6.77	Deviation, D	-0.91	-1.04	-0.34	-0.78	-0.18	0.45	-0.72	-1.00
	D in per cent	13.44	15.36	5.02	11.52	2.66	6.65	10.63	14.77
	Average deviation[a] = 0.677 inch or 10.00 per cent								
	Standard deviation = 0.74 inch or 10.93 per cent								
Aug.	Rain	2.70	2.63	2.58	2.74	2.64	2.60	2.92	2.67
MC = 2.78	Deviation, D	-0.08	-0.15	-0.20	-0.04	-0.14	-0.18	+0.14	-0.11
	D in per cent	2.87	5.39	7.19	1.44	5.03	6.47	5.03	3.96
	Average deviation[a] = 0.130 inch or 4.68 per cent								
	Standard deviation = 0.139 inch or 5.00 per cent								
Sep.	Rain	0.94	0.90	0.93	0.90	0.92	0.86	0.94	0.87
MC = 0.94	Deviation, D	0.00	-0.04	-0.01	-0.04	-0.02	-0.08	0.00	-0.07
	D in per cent	0.00	4.25	1.06	4.25	2.13	8.51	0.00	7.45
	Average deviation[a] = 0.032 inch or 3.46 per cent								
	Standard deviation = 0.043 inch or 4.68 per cent								
Oct.	Rain	4.13	4.03	3.80	3.96	4.30	4.12	4.09	3.89
MC = 4.32	Deviation, D	-0.19	-0.29	-0.52	-0.36	-0.02	-0.20	-0.23	-0.43
	D in per cent	4.39	6.71	12.04	8.33	0.46	4.63	5.32	9.95
	Average deviation[a] = 0.280 inch or 6.48 per cent								
	Standard deviation = 0.316 inch or 7.31 per cent								

[a] Without regard to sign.

Table 5--Rainfall-characteristics of major storms
(These storms have been analyzed and deviations computed with reference to gages MB and MC, respectively, in Tables 6 and 7)

Storm	Duration		Rate of rainfall, inches per hour
	Rainfall hours	Rate, minutes	
June 9+10	12	4	1.80
		8	0.67
		6	1.00
June 14+15	7-1/2	10	4.20
		2	3.60
		5	2.28
June 21+22	21	11	1.85
		23	1.15
		5	2.88
Aug. 27	1-1/2	8	2.40
		10	1.08
		30	0.54
Oct. 4+5	5	5	1.92
		7	0.77
		12	0.60
Oct. 19	18	28	0.19
		54	0.18
		104	0.16

amount both for months and individual storms. The nearest approach to it in consistency of catch was the Fergusson recorder which varied between fifth and ninth in rank for the five monthly totals.

MD, cocoa-mat-covered pit--This gage is second only to MC in total catch for the five-month period. However, for individual months and storms it was the most erratic of all the experimental gages, ranging from first to ninth in rank both for months and storms. The deviation of this gage from MB varied from negative 5.71 per cent to positive 27.68 per cent for the monthly periods and from negative 10.61 to positive 42.22 per cent for the individual storms. With relation to gage MC these ranges of deviation were from negative 12.04 to positive 5.80 per cent and negative 15.10 to negative 1.97 per cent for months and storms respectively. Observation of this gage during storm-periods indicates that the erratic catch is probably due to two causes: (1), Cross-currents of air passing over the surface of the cocoa-mat appear to deflect rain which should enter the gage-receiver; (2) during very intense periods of rainfall it is possible that water ponds on the surface of the cocoa-mat and that the rain striking this pond-surface causes considerable splash, part of which may enter the receiver.

MF and MG, Hall and slotted Hall shields--The catch of these two gages was very similar. MG, the slotted shield, caught less rainfall than MF, the standard Hall shield, and this minus deviation was very consistent. The exact reason for this difference between vanes slotted and not slot-

Table 6--Deviation of catch in inches of each gage by storms from MB, standard-wire enclosure

Storm	Item	Gage							
		MA	MC	MD	ME	MF	MG	MH	MRr
June 9+10	Rain	1.49	1.52	1.49	1.45	1.51	1.45	1.57	1.41
MB = 1.45	Deviation, D	+0.04	+0.07	+0.04	0.00	+0.06	0.00	+0.12	-0.04
	D in per cent	2.76	4.82	2.76	0.00	4.14	0.00	8.28	2.76
	Average deviation[a] = 0.046 inch or 3.17 per cent								
	Standard deviation = 0.059 inch or 4.07 per cent								
June 14+15	Rain	1.43	2.13	1.92	1.45	2.06	2.04	1.52	1.32
MB = 1.35	Deviation, D	+0.08	+0.78	+0.57	+0.10	+0.71	+0.69	+0.17	-0.03
	D in per cent	5.92	57.78	42.22	7.40	52.60	51.11	12.59	2.22
	Average deviation[a] = 0.390 inch or 28.89 per cent								
	Standard deviation = 0.495 inch or 36.67 per cent								
June 21+22	Rain	3.58	3.97	3.77	3.54	3.72	3.64	3.89	3.53
MB = 3.58	Deviation, D	0.00	+0.39	+0.19	-0.04	+0.14	+0.06	+0.31	-0.05
	D in per cent	0.00	10.89	5.31	1.12	3.91	1.68	8.65	1.40
	Average deviation[a] = 0.147 inch or 4.12 per cent								
	Standard deviation = 0.197 inch or 5.50 per cent								
Aug. 27	Rain	0.84	0.85	0.81	0.81	0.80	0.81	0.90	0.82
MB = 0.80	Deviation, D	+0.04	+0.05	+0.01	+0.01	0.00	+0.01	+0.10	+0.02
	D in per cent	5.00	6.25	1.25	1.25	0.00	1.25	12.50	2.50
	Average deviation[a] = 0.030 inch or 3.75 per cent								
	Standard deviation = 0.042 inch or 5.25 per cent								
Oct. 4+5	Rain	1.08	1.10	1.06	1.04	1.08	1.06	1.10	1.02
MB = 1.08	Deviation, D	0.00	+0.02	-0.02	-0.04	0.00	-0.02	+0.02	-0.06
	D in per cent	0.00	1.85	1.85	3.70	0.00	1.85	1.85	5.55
	Average deviation[a] = 0.022 inch or 2.04 per cent								
	Standard deviation = 0.029 inch or 2.68 per cent								
Oct. 19	Rain	1.36	1.39	1.18	1.28	1.64	1.52	1.32	1.28
MB = 1.32	Deviation, D	+0.04	+0.07	-0.14	-0.04	+0.32	+0.20	0.00	-0.04
	D in per cent	3.03	5.30	10.61	3.03	24.24	15.15	0.00	3.03
	Average deviation[a] = 0.106 inch or 8.05 per cent								
	Standard deviation = 0.147 inch or 11.14 per cent								

[a] Without regard to sign.

ted is not apparent, but it has been suggested that it may be due to slight changes in the aerodynamic action of the eddy-chambers formed by the vanes or possibly to change in evaporating surface and specific heat of the material as introduced by cutting away a portion of the vanes. When gage MF is compared with MC, the open pit, the deviations for the five monthly totals are found to vary between negative 0.46 and negative 7.06 per cent, which can hardly be called a significant amount although a tendency toward a lesser catch is indicated. For the six storms the deviations varied from negative 6.29 per cent to positive 17.98, the latter being an erratic figure for which no explanation can now be offered. With reference to gage MB, standard-wire enclosure, the deviations for months range between positive 0.38 and positive 15.01 per cent while for individual storms they vary between zero and positive 52.60 per cent. In general, it seems that the vane-type of shield as placed in the gage-receiver gives results quite comparable to the open-pit enclosure.

MH, Nipher shield--In total catch for the five-month period the gage equipped with Nipher shield gave results of an insignificant amount over the average of the nine experimental gages. However, the gage was quite consistent in that this same small excess over the average of the nine gages was maintained in four of the five monthly records. The deviations of this gage with respect to MB varied from positive 1.49 to positive 11.12 per cent for the five monthly records. With respect to gage MC these deviations varied between positive 5.03 per cent and negative 10.63 per cent. The deviations for storms varied from positive 5.88 per cent to negative 28.63 per cent, with respect to gage MC. Gage MH was slightly below the average of the other gages in its ability to maintain a uniform position in rank as established by amount of catch.

In order to make a fair evaluation of the relative value of the various enclosures and shields described it is necessary to point out that gages MG and MG using the vane-type shields and MC and MD in the pit-enclosures were valueless in the winter since they would not handle snow. In the case of the vane-type shields it was necessary to remove them when the receivers were removed for snow-measurement while the pit-gages drifted full of snow following every snow-storm accompanied by wind.

Table 7--Deviation of catch in inches of each gage by storms from MC, open pit

Storm	Item	Gage							
		MA	MB	MD	ME	MF	MG	MH	MRr
June 9+10	Rain	1.49	1.45	1.49	1.45	1.51	1.45	1.51	1.41
MC = 1.52	Deviation, D	-0.03	-0.07	-0.03	-0.07	-0.01	-0.07	+0.05	-0.11
	D in per cent	1.97	4.60	1.97	4.60	0.66	4.60	3.29	7.23
	Average deviation[a] = 0.055 inch or 3.62 per cent								
	Standard deviation = 0.062 inch or 4.08 per cent								
June 14+15	Rain	1.43	1.35	1.92	1.45	2.06	2.04	1.52	1.32
MC = 2.13	Deviation, D	-0.70	-0.78	-0.21	-0.68	-0.07	-0.09	-0.61	-0.81
	D in per cent	32.86	36.61	9.85	31.92	3.28	4.22	28.63	38.02
	Average deviation[a] = 0.494 inch or 23.19 per cent								
	Standard deviation = 0.575 inch or 27.00 per cent								
June 21+22	Rain	3.58	3.58	3.77	3.54	3.72	3.64	3.89	3.53
MC = 3.97	Deviation, D	-0.39	-0.39	-0.20	-0.43	-0.25	-0.33	-0.08	-0.44
	D in per cent	9.82	9.82	5.03	10.83	6.29	8.31	2.02	11.08
	Average deviation[a] = 0.314 inch or 7.91 per cent								
	Standard deviation = 0.336 inch or 8.46 per cent								
Aug. 27	Rain	0.84	0.80	0.81	0.81	0.80	0.81	0.90	0.82
MC = 0.85	Deviation, D	-0.01	-0.05	-0.04	-0.04	-0.05	-0.04	+0.05	-0.03
	D in per cent	1.18	5.88	4.70	4.70	5.88	4.70	5.88	3.53
	Average deviation[a] = 0.039 inch or 4.59 per cent								
	Standard deviation = 0.041 inch or 4.82 per cent								
Oct. 4+5	Rain	1.08	1.08	1.06	1.04	1.08	1.06	1.10	1.02
MC = 1.10	Deviation, D	-0.02	-0.02	-0.04	-0.06	-0.02	-0.04	0.00	-0.08
	D in per cent	1.82	1.82	3.64	5.45	1.82	3.64	0.00	7.27
	Average deviation[a] = 0.035 inch or 3.18 per cent								
	Standard deviation = 0.042 inch or 3.82 per cent								
Oct. 19	Rain	1.36	1.32	1.18	1.28	1.64	1.52	1.32	1.28
MC = 1.39	Deviation, D	-0.03	-0.07	-0.21	-0.11	+0.25	+0.13	-0.07	-0.11
	D in per cent	2.16	5.03	15.10	7.91	17.98	9.35	5.03	7.91
	Average deviation[a] = 0.122 inch or 8.78 per cent								
	Standard deviation = 0.141 inch or 10.14 per cent								

[a] Without regard to sign.

Conclusions

The data covered by this article includes too short a period to establish definite conclusions. Interesting trends were outlined which are summarized as follows:

(1) The tight board-fence or woven-wire-fence enclosures used in this study seemed to create no significant effect upon the catch of the standard non-recording rain-gage.

(2) The catch of a gage was increased to the greatest extent by placing the gage in a pit with the receiver flush with the ground-surface to catch actual ground-rainfall.

(3) The gages with Hall and Nipher shields fell only slightly below the pit-gages in amount of catch.

(4) Since the pit-enclosures and vane-type shields are generally unsatisfactory during the winter it appears that the most logical solution to the problem of catch-improvement may be the further development and improvement of the Nipher type of shield with the object of obtaining results equivalent to the open-pit type of enclosure for rain-conditions.

New studies

During the winter of 1937-38 another experimental gage utilizing a snow-fence shield as developed by Alter was installed. The action of this shield and of the other experimental gages under snow-conditions will be analyzed later. A pit-type enclosure similar to those used in England will be installed during the spring of 1938 for comparison with the other pit-type enclosures. It is also planned to install additional shields of the Nipher type. It is hoped that the further observation and study of these experimental rain-and-snow gages will lead to the development of a simple unit which can be installed on the numerous precipitation-gages used in watershed-studies so that catch of maximum accuracy may be insured for all seasons of the year.

The following individuals collaborated with the author in the design and field-observation of the above described studies: W. D. Ellison, Superintendent, North Appalachian Watershed Studies, Soil Conservation Service, United States Department of Agriculture; W. A. Mattice, Associate Meteorologist, Soil Conservation Service, now with the Weather Bureau, United States Department of Agriculture; A. S. Guerard, formerly Junior Hydraulic Engineer, North Appalachian Watershed Studies, Soil Conservation Service, United States Department of Agriculture; H. Thom, Junior Hydraulic Engineer, Soil Conservation Service, United States Department of Agriculture; James Czel, Engineering Aide, North Appalachian Watershed Studies, Soil Conservation Service, United States Department of Agriculture.

All studies at the North Appalachian Watershed and Hydrologic Studies project are cooperative with the Ohio Agricultural Experiment Station whose representative in meteorological studies is Doctor J. D. Wilson.

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DISCUSSION

W. A. MATTICE (U. S. Weather Bureau, Washington, D. C.)--With reference to the so-called Nipner shield-installation shown by the author, it might be indicated that this is not a true Nipher shield. The pit-gage installation shows a broad masonry-rim around the edge, and the catch of both gages might be materially affected due to the splashing effect. The broad band left around the end of the Nipher shield is an excellent splashing surface as is the masonry rim of the pit-gage. In June, 1937, the so-called Nipher shield was found to include a flat sheet of metal only a few inches below the mouth of the gage and the pit-gage had considerable accumulated dirt which indicated much splashing. It was recommended that the flat surface of the Nipher shield be removed and that the pit-gage have grass up to the edge of the pit, thus reducing splashing.

It might be well to indicate that the author's treatment of the data is somewhat unorthodox statistically. For example, in certain of his tables he assumes the catch of a certain gage as a base and computes deviations from the base. By taking the root-mean-square of these deviations and calling it the standard deviation he departs considerably from the accepted definition of the standard deviation, as this term is determined from the arithmetic mean of the measures and not from an independent mean. The arithmetic average of the gages for June was 9.44, while the author has used 8.56, or an approximate difference of ten per cent.

I would like to add three reference-works to the bibliography of the paper, which are very important in treating of experimental exposures of gages and the use of shields:

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MONTHLY VARIATION OF RAINFALL-INTENSITIES IN WISCONSIN

Arno T. Lenz

The variation of rainfall-intensity expected for a given frequency with season of the year is an important factor in estimation of runoff from small drainage-areas such as those tributary to erosion-control dams. Calculations of probable runoff-rates in Wisconsin are ordinarily based on two assumptions, namely, (1) rainfall of maximum intensity will occur in mid-summer and (2) the maximum runoff from a small area will result from rainfall of maximum intensity.

To check the reasonableness of these assumptions for Wisconsin conditions, intensity-frequency studies by months were made of the complete records of six United States Weather Bureau stations located at Milwaukee, Madison, La Crosse, Wausau, and Green Bay, Wisconsin, and Duluth, Minnesota, located as shown on Figure 1. Storms were grouped by months, the records for all stations for any month being combined into a single record of 204 station-years.

The frequency of rainfall-intensities for each month in which excessive intensities occur is shown in Figure 2. The curves of Figure 3 show for storms of 100-year and 30-year frequencies that the intensity for rainfall-periods up to two hours increases from early spring to a maximum in July and decreases during the latter half of the year. In general the intensity to be expected in any short-time period during an intense rain in Wisconsin in April will be from one-half to one-third that which will be expected with the same frequency in July.

When the intensities for the month of April are compared with those of the same frequency for July, it is evident that if the coefficient of runoff for conditions in July is greater than 0.34, the runoff from storms in July will ordinarily be greater than that from storms in April even though there may be 100 per cent runoff in April.

Need for analysis by months--Increasing use of the rational method for estimating runoff to be expected from small drainage-areas tributary to soil-erosion control-dams and highway-culverts has led to greater consideration of the factors involved in the formula Q = CiA.

The rainfall-intensity factor, i, has been studied with reference to the probable frequency with which a certain intensity may be expected, a noteworthy contribution being that of the late David L. Yarnell [2]. Yarnell and others estimated the frequency at which maximum rainfalls in a given time-period could be expected. Structures to discharge runoff must of course be designed to carry the maximum runoff. In the northern part of the United States where rain often falls on snow and ice there is always the possibility that a moderately heavy rain falling on frozen ground, together with melting snow, will add up to runoff-values which might exceed those which would result from the more excessive rains expected in mid-summer.

It has been previously assumed that the maximum runoff from small areas is due to rainfall alone and that such rainfall does not occur at a time of the year when snow or frozen ground adds to the runoff. In general this assumption has been reliable but it also needs to be supported by a study of seasonal variation of magnitude of intense rains. Yarnell shows the number of excessive rainstorms per month for a 30-year period but the magnitude to be expected in any month is not given.

Calculations--In this study of the Wisconsin region the greatest rainfall in any 5-, 10-, 20-, 30-, 60-, 80-, and 120-minute period was determined for each storm. The records used are summarized in Table 1. In all, 875 storms were studied.

The storms were then tabulated by months, the records of the individual stations for each month being combined into a single record of 204 station-years and treated as a whole.

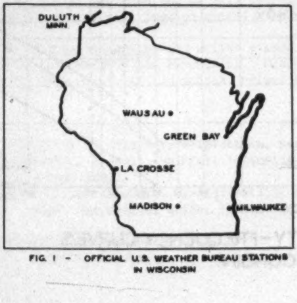

FIG. 1 - OFFICIAL U.S. WEATHER BUREAU STATIONS IN WISCONSIN

The merits of the station-year method of combining rainfall-records have been discussed by Dr. C. W. Thornthwaite [3] and others and need not be entered into here. It may be noted from Figure 1 that the six stations are fairly evenly spaced and uniformly distributed over the area considered. Moreover, the location of the state with respect to storm-paths and its geographical uniformity are such that a random distribution of storms is a reasonable assumption.

For each month the maximum intensity which occurred at any station in a given time-interval was considered the storm to be expected once in 204 years. The second largest intensity was assumed to be expected twice in 204 years or of a frequency of 102 years. The calculated values of intensity and frequency are shown in Figure 2. Smooth curves were drawn through the points to form the expected frequency-curves for the various time-periods.

There were no rains of intensity greater than one inch per hour in any 5-minute period or longer during the months of December, January, and February at any of the stations and only three storms in March, all of them being of short duration.

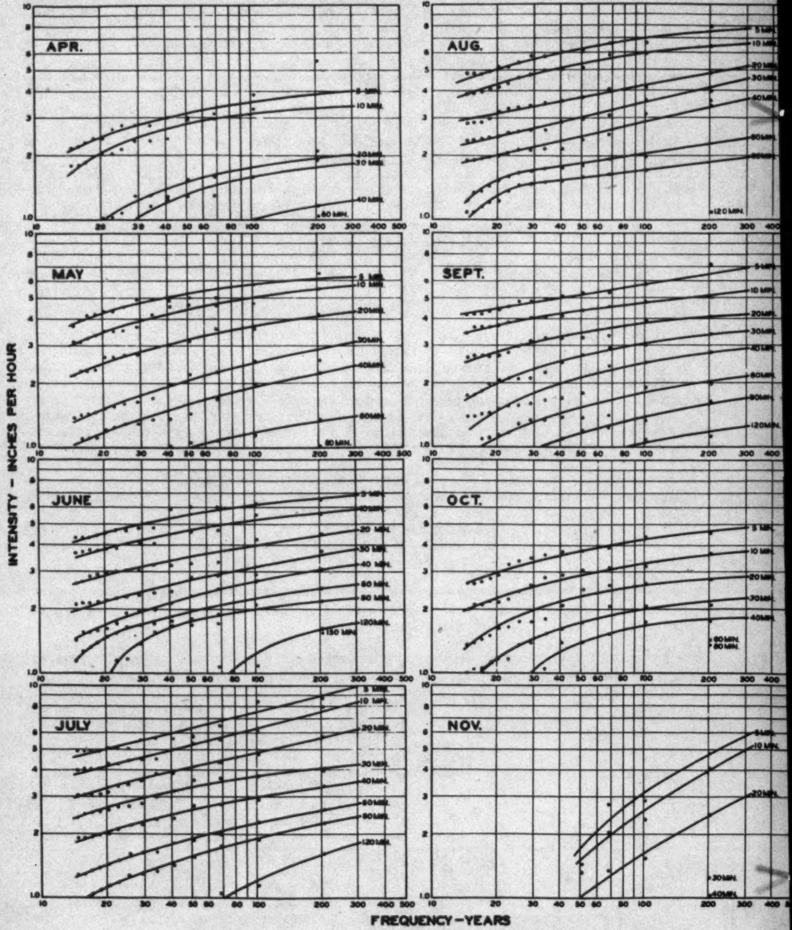

FIG.2-RAINFALL INTENSITY-FREQUENCY CURVES FOR WISCONSIN

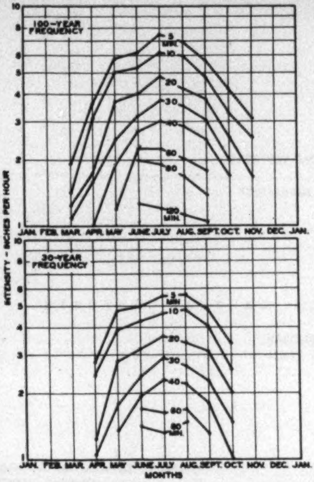

FIG. 3—MONTHLY INTENSITY-CURVES OF WISCONSIN RAINFALL

From the monthly curves of Figure 2, values of intensity at the various time-periods were read off for frequencies of 100 years and 30 years and plotted on Figure 3.

Conclusions--The curves of Figure 3 indicate that the intensity to be expected once in 100 years, or 30 years, during the winter, is only a fraction of that to be expected in mid-summer. For example, it will be noted that the maximum rainfall for any 30-minute period with a 30-year frequency has an intensity of nearly three inches per hour and occurs in July. Under conditions in April, the probable intensity would be slightly more than one inch per hour. This would permit, theoretically at least, a runoff-coefficient in April three times that assumed in July with no greater runoff. Thus if the runoff-coefficient for July was 0.34 or greater, the July runoff should exceed that expected from a storm in April of the same frequency with a coefficient of 1.00

Similar ratios may be computed for other time-periods. Inspection will show, however, that the ratio of intensities in July to those in April is about three to one for all periods except those of five and ten minutes, for which the ratio is about two to one.

It should be observed that if the runoff-coefficient assumed for a small area under conditions in July is materially less than 0.34 it is possible that rain on frozen ground in March or April may produce runoff greater than that expected from rain of maximum intensity in mid-summer.

The effect of melting snow may be expressed mathematically as

$$Q = CiA + Q_s$$

where Q_s is the discharge due to the melting snow. On any given small area with a short time of concentration Q_s may be neglected and if the product of C and i for July is larger than the same product in April, the greatest runoff will be expected in July. As the size of the area increases, however, and the time of concentration increases, the product of C and i per unit of area will decrease and Q_s will increase. The maximum runoff will then be caused by a moderate rain on frozen ground covered with snow.

Table 1--Summary of intense rainfall-records

Station	Period of record	Length of record
		Years
Milwaukee, Wisconsin	1897 - 1937	41
Duluth, Minnesota	1897 - 1937	41
La Crosse, Wisconsin	1902 - 1937	36
Green Bay, Wisconsin	1902 - 1937	36
Madison, Wisconsin	1905 - 1937	33
Wausau, Wisconsin	1916 - 1932	17
	Total station-years	204

Acknowledgements--The writer wishes to acknowledge assistance given by Professor J. G. Woodburn, who reviewed the manuscript. Messrs. A. A. Lemke, thesis student, E. C. Brzeskiewicz and W. E. Davy, N. Y. A. assistants, helped in the calculations and preparation of curves

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University of Wisconsin,
Madison, Wisconsin

AMERICAN GEOPHYSICAL UNION

Nineteenth Annual Meeting

MINUTES GENERAL ASSEMBLY AND SECTIONS

Washington, D. C.
April 27-30, 1938

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MINUTES OF THE GENERAL ASSEMBLY, NINETEENTH ANNUAL MEETING (1938)

BUSINESS SESSION

The business session of the general assembly of the American Geophysical Union was held Friday, April 29, 1938, in the Library of the National Academy - Research Council Building, Washington, D. C. The session was called to order at 12:04 p.m. by the Chairman, N. H. Heck, there being 42 members present [see Appendix A].

The minutes of the eighteenth annual meeting on April 30, 1937, were approved as mimeographed and distributed after that meeting.

The General Secretary presented an abstract of his report for the past year [see Appendix B]; this report was accepted and ordered filed.

The Chairman read a summary of activities of the past three years [see Appendix C]; this summary was accepted and ordered filed.

Reports of standing committees

Chairman P. C. Whitney of the Membership Committee reported informally. He stated that the Committee acted on 123 proposals for membership during the past year and that the Union has now over a thousand members. He remarked that 1,500 has been set as the new goal.

Chairman H. D. Harradon of the Committee on Statutes and By-Laws read the following report which was accepted and ordered filed:

"At the meeting of the Executive Committee of the Union on January 21, 1938, the Committee on Statutes and By-Laws was instructed to consider a change in the Statutes and By-Laws of the Union which will permit the election of foreign associate members, and also to consider changing the Statutes and By-Laws to the effect that the Chairmen and Vice-Chairmen of the Union and its sections be designated as Presidents and Vice-Presidents in order to follow in this respect the practice of the International Union of Geodesy and Geophysics. The Committee on Statutes and By-Laws gave consideration to the matter and made the necessary changes in the Statutes and By-Laws. It also changed the word 'Section' to 'Association' wherever it applied to the International Union of Geodesy and Geophysics. These changes were duly approved by the Executive Committee of the Union and a letter under date of February 26, 1938, was sent to all members of the Union so that the changes in question may be voted upon at this meeting."

Reports of special committees

Chairman W. T. Thom, Jr., of the Special Committee on Geophysical and Geological Study of Continents presented an abstracted report; it was accepted and the complete written report was ordered printed in the Transactions [see Appendix D].

Chairman R. M. Field of the Special Committee on Geophysical and Geological Study of Oceanic Basins made a brief report; it was accepted and the complete written report was ordered printed in the Transactions [see Appendix E].

Chairman William Bowie of the Planning and Project Committee made a brief report; it was accepted and ordered filed [see Appendix F].

Chairman H. E. McComb of the Special Committee on Time-Signal Service read the following report which was accepted and ordered filed:

"This Committee has continued to keep in touch with individuals and organizations using Naval Observatory time-signals as broadcast by Naval Communications Service. Members of the Committee have conferred with the Bureaus of the Navy Department concerned relative to limited special time-signal schedules and with regard to desirable minor changes in broadcast frequencies. The Navy Department has kindly consented to furnish special signals for an astronomical and gravimetric party in Alaska during the coming season. These signals will be broadcast on a frequency of 9425 kilocycles. Increased power for this signal has been proposed and, if adopted, should eliminate or reduce present interference from a foreign station operating on or near this frequency.

"The recent increase in the number of signals broadcast daily by the Navy Department makes it possible at the present time to receive signals at every hour of the day on frequencies of 113 or 9425 kilocycles, or both."

Chairman Frank Neumann of the Special Committee to Cooperate with International Commission for the Study of Tidal Waves read the following report which was accepted and ordered filed:

"All reports on tidal waves collected to date by members of the Committee have been forwarded by the Chairman to the President of the International Commission.

"In response to a circular letter distributed in December 1937 by the President of the International Commission, the members of the American Committee were requested by its Chairman to furnish information which might be useful in investigating the nature and form of tidal waves (particularly information relating to the use of special apparatus) so that a joint reply could be prepared. A reply was made based on the information thus received. It stated in substance that because of the expense involved no systematic effort has been made to determine tidal waveforms other than offered by the study of normal tide-gage records but that these were admittedly inadequate for the purpose because of slow paper-speed. It was fully recognized that observations with special equipment were highly desirable but that the cost of constructing and servicing the required type of apparatus was prohibitive. The hope was expressed that, in view of the almost complete absence of instrumental data, continued efforts would be made to interest mariners and others in reporting tidal wave-phenomena at sea and along our coasts.

"It is believed that members of the Union may at times find themselves in a position to aid materially in this effort and they are urged to contact members of the Committee on Tidal Waves when occasions arise."

Chairman J. A. Fleming of the Special Committee to Consider Establishment of an American Journal of Geophysics presented the following report which was accepted and ordered filed:

"The Committee has not yet found the means to establish the proposed journal. We have, however, been able to continue publication of the Transactions and I think that they afford an admirable means of communicating the work of the Union. We think it would be desirable to enter into some system by which we could send out individual articles and print them promptly when manuscripts are received. These would offer a great many advantages, would spread the period of preparing copy over the whole year, and would give the author reprints immediately in any number wanted. It would also permit the assembling of papers of interest to any particular individual according to his wishes rather than in one large volume containing some articles in which he is not interested. With such a plan we could obtain favorable rates of publication so that the cost would not be much more than that of our Transactions and at the same time we could probably increase the number of pages beyond those contained in our annual Transactions."

Resolutions

The Committee on Resolutions [H. N. Eaton (Chairman), A. G. McNish, and E. W. Eickelberg] then submitted through Chairman Eaton the following six resolutions, each of which was unanimously adopted upon motion:

(1) RESOLUTION ON MAGNETIC SURVEY OF THE LAND AND WATER AREAS OF THE UNITED STATES (submitted by Planning and Project Committee, Section of Terrestrial Magnetism and Electricity, and Section of Oceanography)

WHEREAS, The need for more complete and more accurate magnetic data is constantly increasing by reason of the extension of navigation by air and by sea and of scientific and commercial exploration by geophysics, and

WHEREAS, Many of the present stations on land, including repeat-stations, are no longer suitable for magnetic observations owing to local disturbance occasioned by the growth of cities and towns, and

WHEREAS, No adequate observations have been obtained in waters adjacent to the United States and its territories and possessions since the destruction of the Carnegie in 1929, and

WHEREAS, This lack of observations on sea and land is a serious handicap to the construction of accurate magnetic charts over land and sea, therefore be it

RESOLVED, That the American Geophysical Union and the Committee on Geophysics of the National Research Council strongly urge that a complete resurvey be made of the continental United States and its possessions and territories, this survey to include the establishment of additional stations where needed and a series of basic repeat-stations in places not likely to be subject to local disturbance in the future, and be it further

RESOLVED, That every effort be exerted by the United States Government to procure, through one of its agencies, a non-magnetic vessel capable of continuing the work of the Carnegie, and be it further

RESOLVED, That copies of this resolution be sent to the President of the United States, the Secretary of the Navy, the Secretary of the Interior, the Secretary of Commerce, and the Director of the Coast and Geodetic Survey.

(2) RESOLUTION ON PERMANENCE OF LOCATION OF METEOROLOGICAL STATIONS (submitted by Section of Hydrology)

WHEREAS, It is recognized that meteorological instruments at closely adjacent locations do not give identical records owing mainly to differences in exposure inherent to the location, and

WHEREAS, Seemingly minor changes in location frequently have an effect on the record of an instrument when it is moved, equivalent to the discontinuance of one record and the beginning of a new record, and

WHEREAS, Investigations of long-term trends, sequences, and variations of various meteorological observations depend on the records of instruments continuously maintained without interruption at one location for a long period, and

WHEREAS, Many scientific investigations and engineering projects are dependent on the trends shown by such long-term records, therefore be it

RESOLVED, That it is the recommendation of the American Geophysical Union and the Committee on Geophysics of the National Research Council that meteorological instruments and stations should not be moved or interfered with in any instance in which it can be reasonably avoided, and be it further

RESOLVED, That copies of this resolution be sent to the Secretary of the Treasury, the Secretary of Agriculture, the Chief of the United States Weather Bureau, and the Supervising Architect of the Department of the Treasury.

(3) RESOLUTION ON GEOPHYSICAL SURVEY OF CENTRAL ATLANTIC STATES (submitted by Planning and Project Committee)

WHEREAS, The Eastern United States is a region of unusual geological interest, and

WHEREAS, The American Geophysical Union believes that the comparative use of different methods of geophysical prospecting will supply valuable data for the geological interpretation of that region, therefore be it

RESOLVED, That the American Geophysical Union and the Committee on Geophysics of the National Research Council urge that a cooperative attempt be made to obtain experimental magnetic and gravitational observations along several profiles across the Appalachian Mountains and the Atlantic Coastal Plain, and be it further

RESOLVED, That copies of this resolution be sent to the President of the United States, the President of the Carnegie Institution of Washington, the Director of the United States Coast and Geodetic Survey, the Director of the United States Geological Survey, the President of the Geological Society of America, the President of the American Association of Petroleum Geologists, the President of the Society of Exploration Geophysicists, the Chairman of the Geophysical Section of the American Institute of Mining and Metallurgical Engineers, the Presidents of the University of North Carolina, Lehigh University, and the Massachusetts Institute of Technology, and the State Geologists of North Carolina, Virginia, West Virginia, Maryland, Pennsylvania, and New Jersey.

(4) RESOLUTION OF COMMENDATION OF THE STEERING COMMITTEE FOR METEOROLOGICAL RESEARCH PROJECTS (submitted by Section of Meteorology)

WHEREAS, The Steering Committee for Meteorological Research Projects under the Bankhead-Jones Act has judiciously encouraged fundamental meteorological research, therefore be it

RESOLVED, That the American Geophysical Union and the Committee on Geophysics of the National Research Council heartily commend the Committee for this contribution to geophysical research, and be it further

RESOLVED, That copies of this resolution be sent to the Secretary of Agriculture and to the Chief of the United States Weather Bureau.

(5) RESOLUTION ON COOPERATION OF SMITHSONIAN INSTITUTION IN FURNISHING MEETING FACILITIES FOR AMERICAN GEOPHYSICAL UNION (submitted by Committee on Resolutions)

WHEREAS, The meetings of the National Academy of Sciences utilized all the available space in the National Academy and Research Council Building on Wednesday, April 27, 1938, and

WHEREAS, The administrative staff of the Smithsonian Institution made available space and projection-facilities in the National Museum Building for the sections of Geodesy, Seismology, and Terrestrial Magnetism and Electricity on this date, therefore be it

RESOLVED, That the American Geophysical Union and the Committee on Geophysics of the National Research Council express their appreciation of this generous cooperation in furnishing these facilities, and be it further

RESOLVED, That a copy of this resolution be sent to the Secretary of the Smithsonian Institution.

(6) RESOLUTION ON COOPERATION OF UNITED STATES GEOLOGICAL SURVEY IN FURNISHING A MEETING PLACE FOR AMERICAN GEOPHYSICAL UNION (submitted by Committee on Resolutions)

WHEREAS, The United States Geological Survey made available its Director's Conference Room for the use of the American Geophysical Union on April 30, 1938, therefore be it

RESOLVED, That the American Geophysical Union and the Committee on Geophysics of the National Research Council express express their appreciation of this courtesy extended by the United States Geological Survey, and be it further

RESOLVED, That copies of this resolution be sent to the Secretary of the Interior and to the Director of the United States Geological Survey.

Elections

Chairman William Bowie presented a report for the Committee on Nominations nominating R. M. Field and W. C. Lowdermilk for President and Vice-President of the Union, respectively, for three years from July 1, 1938. There being no other nominations from the floor, R. M. Field and W. C. Lowdermilk were unanimously elected President and Vice-President of the Union, respectively, for three years from July 1, 1938. (The three-year term of General Secretary J. A. Fleming continues through June 1940.)

Sections of the Union reported the election of the following officers for three years from July 1, 1938:

Geodesy--President, C. H. Swick; Vice-President, R. M. Wilson (the three-year term of Secretary W. D. Sutcliffe continues through June 1940)
Seismology--President, J. B. Macelwane; Vice-President, H. E. McComb (the three-year term of Secretary Frank Neumann continues through June 1939)
Meteorology--President, R. H. Weightman; Vice-President, C. W. Thornthwaite; Secretary, H. D. Harradon
Terrestrial Magnetism and Electricity--President, E. O. Hulburt; Vice-President, E. A. Eckhardt; Secretary, E. W. Eickelberg
Oceanography--President, C. O. Iselin; Vice-President, G. F. McEwen; Secretary, P. C. Whitney
Volcanology--President, E. G. Zies; Vice-President, Adolph Knopf; Secretary, James Gilluly
Hydrology--No elections (terms of present officers, President L. K. Sherman, Vice-President J. E. Church, and Secretary K. H. Beij, continue through June 1939)

Statutes and By-Laws

The proposed amendments to the Statutes and By-Laws of the Union in accordance with notice mailed to all members February 26, 1938, were adopted as circulated [for details, see report of Committee on Statutes and By-Laws given on p. 556].

Seventh Triennial Assembly

J. A. Fleming reported that arrangements have been made for eleven committees in connection with the Seventh Triennial Assembly of the International Union of Geodesy and Geophysics which is to be held during September 1939 at Washington. Preliminary circulars and provisional programs have been prepared and distributed to foreign colleagues and countries and institutions interested, and also to each member of our Union. Since the international meeting will be held in Washington in 1939, it was moved and carried that the annual spring meeting of the American Geophysical Union may be omitted in 1939 at the discretion of the Executive Committee. In this connection, the General Secretary stated it would be well to bear in mind that arrangements should be made that members who do not attend the international meeting be supplied with copies of papers and other publications issued at the time of the Assembly; a break in continuity of annual Transactions is undesirable.

The meeting adjourned at 1:04 p.m.

SCIENTIFIC SESSION

The scientific session, beginning at 2:33 p.m. with 147 members and guests present [see Appendix A], was devoted to a symposium on the physics of volcanic processes as printed in these Transactions [pp. 7-43].

The Committee on Meetings [W. W. Rubey (Chairman), R. M. Field, R. W. Davenport, and A. G. McNish] was commended for the excellent program developed, resulting in the great success of the nineteenth annual meeting of the Union as evidenced by the papers published in this present volume of Transactions.

The meeting adjourned at 5:30 p.m.

The minutes of the meetings of the Sections of the American Geophysical Union, April 28-30, 1938, are given in attached Appendix G.

(Signed) Jno. A. Fleming, General Secretary,
American Geophysical Union

APPENDIX A--RECORD OF 534 MEMBERS AND GUESTS IN ATTENDANCE AT GENERAL ASSEMBLY OF THE UNION AND AT SESSIONS OF ITS SECTIONS, NINETEENTH ANNUAL MEETING, APRIL 27-30, 1938

(1 = Business session of General Assembly; 2 = Scientific session of General Assembly; 3 = Joint meeting of Sections of Meteorology and Oceanography; a = Geodesy; b = Seismology; c = Meteorology; d = Terrestrial Magnetism and Electricity; e = Oceanography; f = Volcanology; and g = Hydrology.)

Adams, L. H. (2,a,f)
Adams, O. S. (2,a,e)
Aldrich, J. D. (2)
Allen, Alice S. (2)
Allen, C. T. (a)
Allen, E. T. (2,f)
Allen, R. A. (3,c)
Alpher, R. O. (all)
Anderson, B. J. (e)
Anderson, N. G. (3,c)
Ash, A. D. (g)
Avers, H. G. (a,e)
Bainbridge, W. H. (a,e)
Baker, A. A. (2)
Baker, M. V. (g)
Balk, R. (2,f)
Balsam, Ella (a,d)
Barksdale, H. C. (1,2,g)
Barnes, F. F. (f)
Bartsch, P. (1,e)
Bascom, Florence (2,f)
Bastin, E. S. (2)
Beij, K. H. (1,g)
Bemis, A. C. (3,c)
Benfell, V. S. (a)
Bent, A. E. (3,c)
Bernard, M. (1,g)
Beutner, E. L. (e)
Bevan, A. (g)
Birch, F. (2,b)
Birch, H. (f)
Bird, B. (g)
Birdseye, C. H. (a)
Blair, W. R. (c)
Blaisdell, F. W. (g)
Blake, A. (b)
Bloch, I. (g)
Blumenauer, R. E. L., Jr. (3)
Blythe, R. H., Jr. (g)
Bodle, R. R. (b)
Boland, W. A. (3)
Bonner, J. P. (g)
Booker, H. G. (d)
Bortzman, L. E. (3)
Bowen, N. L. (2,f)
Bowie, W. (1,2,a)
Boyer, M. C. (g)
Braaten, N. F. (a)
Bradford, G. (e)
Bradley, W. H. (2,g)
Bramlette, M. N. (2)
Brashears, M. L., Jr. (g)
Bratty, M. M. (f)
Brooks, C. F. (1,3,c,g)
Brooks, H. P. (g)
Brown, C. B. (f,g)
Brunner, G. J. (2,b,f)
Bryan, G. S. (e)
Bryhn, Martha A. (d)
Bryson, R. P. (2,3,f)
Burbank, W. S. (2,f)
Butterfield, A. D. (a)
Byers, H. R. (3)
Cady, R. C. (g)
Calkins, F. C. (2,f)
Callaghan, E. (2,f)
Callaway, E. B. (3,c,d,e)
Capello, J. J. (all)
Cederstrom, D. J. (2)
Cheney, W. L. (d)
Choate, H. L. (3,c)
Church, P. E. (3)
Claire, C. N. (a)
Clapp, P. F. (3)
Clarke-Hafstad, K. (2,3,c,g)
Clifford, C. J. (a)
Clinite, R. G. (a)
Cloos, E. (2,f)
Clough, H. W. (3)
Coffin, L. A. (3)
Colbert, L. O. (e)
Collins, E. B. (e)
Collins, W. D. (g)
Conger, P. S. (e)
Cook, H. L. (g)
Counts, R. C., Jr. (3)
Covert, R. N. (3)
Cowie, G. D. (a,e)
Cox, G. N. (b,d,g)
Criswell, W. H. (e)
Crofutt, C. B. (3)
Crosby, I. B. (g)
Crowl, G. H. (2,a,b,e,f)
Dalrymple, T. (g)
Daly, R. A. (2,b,f)
Davenport, R. W. (1,g)
Davids, H. G. (2)
Davidson, J. M. (g)
Dawbaugh, W. R. (3)
Day, A. L. (2)
Decker, J. W. (3,f)
Demorest, M. (2,3,a,b,c,g)
Derby, W. N. (e)
Dibe, R. A. (f)
Dietz, D. (d)
Dike, P. A. (2)
Disney, L. P. (e)
Dobson, G. C. (g)
Donnay, J. D. H. (f)
Dorsey, H. G. (e)
Dove, W. N. (2,a,d,g)
Drane, B. J. (g)
Duerksen, J. A. (2,a,b,e)
Duncan, D. A. (2)
Du Pré, M. (g)
Duvall, C. R. (a,d)
Eaton, H. N. (1,g)
Eckel, E. B. (f)
Eckhardt, E. A. (a,b,d)
Edlefsen, N. E. (1,g)
Eickelberg, E. W. (2,3,b,c,d,e)
Eisenlohr, W. S., Jr. (g)
Ellis, J. R. (a)
Ellison, W. D. (g)
Emmons, G. (3,c)
Emory, F. A., Jr. (3)
England, J. (a)
Erskine, H. M. (g)
Ewing, M. (1,a,b,e)
Fentress, G. L. (a)
Ferguson, G. E. (g)
Ferguson, J. H. (3,c)
Fewell, J. M. (e)
Fiedler, A. G. (g)
Field, R. M. (1,2,3,a,b,e,g)
Fishel, V. C. (g)
Fleischer, M. (2,f)
Fleming, J. A. (all)
Fogel, C. H. (d)
Forbush, S. E. (d)
Forrest, K. (g)
Frank, B. (g)
Frazier, A. H. (g)
Friend, A. W. (d)
Friis, H. R. (e,g)
Garner, C. L. (1,2,a,e,g)
George, J. J. (3)

Gibson, R. E. (2,f)
Gilchrist, L. (2)
Gilmore, J. E. (a)
Gish, O. H. (1,3,c,d)
Glass, Jewell J. (2,f)
Goddard, E. N. (f)
Goodridge, R. S. (1,g)
Goranson, R. W. (2,f)
Graton, L. C. (2,f)
Gregg, W. R. (3)
Greig, J. W. (2,f)
Griggs, D. (2,f)
Griggs, Laura T. (2)
Griggs, R. F. (2)
Griggs, R. T. (f)
Grimminger, G. (3)
Guild, P. W. (f)
Gunn, R. (a,b)
Guthrie, F. D. (2)
Hafstad, L. R. (d)
Hamantas, C. (3)
Hammer, S. (2,a,d)
Hand, I. F. (3,c)
Hanley, J. B. (f)
Harkness, F. B. (g)
Harradon, H. D. (1,2,3,a,c,d,e)
Harrison, L. P. (3)
Harrold, J. V. (g)
Harrold, L. L. (g)
Hartnell, G. (2,b,d,f)
Hartwell, O. W. (g)
Hathaway, G. A. (3,g)
Haynes, Mrs. A. H. (c)
Heck, N. H. (all)
Heinrich, R. R. (2,b,f)
Hellweg, J. F. (a)
Helmkamp, E. F. (e)
Hemple, H. W. (2,a,e)
Henderson, J. T. (c)
Hendrix, W. C. (2,a,d,g)
Henry, T. R. (a)
Hermansen, E. (d,g)
Heroy, W. B. (2,3,f)
Hess, H. H. (2,a,e,f)
Heydenburg, N. P. (d)
Hildebrand, S. F. (3)
Hill, H. F. (g)
Hill, L. K. (g)
Hodgson, E. A. (b,d)
Horner, W. W. (g)
Horton, A. H. (g)
Horton, R. E. (g)
Hoskinson, A. J. (a,e)
Howe, H. H. (d)
Howe, J. W. (g)
Hoyt, W. G. (g)
Huff, T. F. (d)
Hulburt, E. O. (d,e)
Hull, B. (g)
Humphreys, W. J. (3,c)
Hunt, C. B. (2)
Hunt, Mrs. I. L. (2)
Hurd. W. E. (3,f)
Hursh, C. R. (g)
Hutchinson, H. B. (3)
Ingerson, E. (2,f)
Ireland, A. (b)
Isam, L. W. (3,c)
Iselin, C. O. (3,e)
Ives, R. L. (a)
Jacobs, C. E. (g)
Jaggar, T. A. (2,a,b,f)
Jarvis, C. S. (g)
Joerg, W. L. G. (e)
Johnson, E. A. (d)
Johnson, J. W. (g)
Johnson, T. H. (d)
Johnson, W. R., Jr. (2,d,f,g)
Johnston, H. F. (1,2,3,c,d,e)
Johnston, W. D., Jr. (f)
Jones, B. E. (g)
Jones, F. V. (3)
Jones, L. A. (g)
Joyce, J. W. (d)
Judson, L. V. (1,a)
Kahl, J. H. (e)
Kalbfleisch, A. C. (a)
Kaufman, H. P. (a,e)
Keith, A. (1,2,a,d,f)
Kellar, J. G. (e)
Kelly, W. A. (g)
Kenrick, G. W. (3,a,c,d)
Kenworthy, P. W. (c)
Keulegan, G. H. (g)
Kimball, B. F. (g)
Kincer, J. B. (3,g)
King, A. L. (g)
Kirby, S. S. (d)
Kissam, P. (a)
Kline, A. B. (3)
Kline, S. R. (g)
Knapp, D. G. (d)
Knapp, R. T. (g)
Knarr, A. J. (3,c)
Knopf, A. (2,f)
Knopf, Eleanora B. (2)
Kohler, F. B. (g)
Korff, S. A. (d)
Koschmann, A. H. (2,f)
Kotok, E. J. (g)
Kretschmar, Miss W. (2)
Kretschmar, W. F. (2)
Krimgold, D. B. (g)
Krupen, P. (b)
Kuiskeru, K. (f)
Lambert, W. D. (1,2,a)
Landsberg, H. (1,3,b,c)
Lane, A. C. (2,f,g)
Lane, E. W. (g)
Langbein, W. B. (g)
Larrabee, J. H. (e)
Laws, J. O. (g)
Lazenby, A. J. (g)
Leach, H. R. (g)
Leahy, L. R. (3,e)
Ledig, P. G. (2,d)
Lee, F. W. (2,3,b,f)
Leggette, R. M. (g)
Lennehan, C. M. (3,c)
Lenz, A. T. (g)
Lieber, P. (g)
Liferock, M. (3,c,e)
Little, D. M. (3,c)
Littman, J. (e)
Lockhart, W. M. (3)
Lohman, K. E. (2)
Lohman, S. W. (g)
Longwell, C. R. (2,a,f)
Lorz, J. G. (d)
Loughead, H. J. (g)
Loughlin, G. F. (2,b,f)
Love, S. K. (g)
Lovering, T. S. (2,f)
Lowdermilk, W. C. (g)
Luber, P. (f)
Luce, R. F. (e)
Ludy, A. K. (1,2,b,d)
Lynch, W. A. (b)
Macelwane, J. B. (2,b,f)
MacMaugh, F. G. (a)
Magruder, J. H., Jr. (3)
Maltby, W. R. (d)
Mansfield, G. R. (2)
Marble, A. H. (2,f)
Marble, J. P. (2,f)
Maris, H. B. (1,d)
Marsden, R. D. (g)
Martin, L. (3,e,f)
Martin, W. N. (a)
Mather, L. B., Jr. (2,f)
Mattice, W. A. (3)
Mavis, F. T. (g)
Mayhew, H. B. (e)
McComb, H. E. (1,b,d)
McCombs, C. (a)
McDonough, S. J. (b)
McKay, E. C. (a)
McNish, A. G. (1,2,b,d)
McNish, Mrs. L. H. (d)
Mears, A. H. (3)
Medina, G. (e)
Meginnis, H. G. (g)
Meinzer, O. E. (1,2,g)
Mendenhall, W. C. (2)
Merriam, C. F. (g)
Meyers, R. C. (d)
Michael, S. J. (e)
Michelson, A. (3)
Middlebrook, T. A. (g)
Middleton, H. E. (g)
Middleton, W. E. K. (3,c)
Miller, L. F. (3)
Milton, C. (2,f)
Minshall, N. E. (g)
Miser, H. D. (2,f)
Mitchell, C. L. (3,c)
Mitchell, G. S. (3)
Mitchell, H. C. (a)
Moats, P. L. (2,d)
Moler, R. C. (a)
Montgomery, R. B. (3,c,e)
Moots, E. E. (a,b,g)
Morey, W. H. S. (a)
Morgan, H. R. (a)
Munns, E. N. (g)
Murphy, R. C. (3)
Murts, H. C. (g)
Musgrave, G. W. (g)

Myers, A. J. (3,g)
Namias, J. (3)
Nelson, J. H. (b,d)
Ness, A. T. (e)
Nettleton, L. L. (2,a,b)
Neumann, F. (1,2,b)
Noble, W. M. (g)
Nolan, T. B. (2,f)
Norris, C. (a)
Norton, K. A. (d)
O'Brien, B. (c)
Olsen, C. E. (e)
Olsen, L. (g)
Owen, J. E. (g)
Page, B. L. (a)
Page, L. F. (3)
Paige, S. (2,a,b,f,g)
Pardee, J. T. (b,f,g)
Park, C. F., Jr. (2,f)
Parkin, E. J. (a)
Parr, A. E. (3)
Paul, D. M. (g)
Paulsen, C. G. (g)
Pawling, J. (a)
Peacock, F. L. (e)
Pear, C. B., Jr. (3,c)
Peirce, L. B. (g)
Pekeris, C. L. (3,c)
Peterson, B. J. (g)
Phelps, A. (3,e)
Pierce, C. H. (3,g)
Pierce, L. T. (3)
Piggot, C. S. (d,e)
Poling, A. C. (a)
Posnjak, E. (2)
Postley, O. C. (2)
Potter, W. D. (g)
Poulter, T. C. (3,b)
Powers, Edith (3)
Powers, R. (3)
Rabinow, M. L. (2)
Rakestraw, N. W. (2,e)
Ramirez, J. E. (2,3,b)
Ramser, C. E. (g)
Rappleye, H. S. (a)
Reid, H. F. (2,a,b)
Reily, D. P. (3)
Reymer, S. E. (b)
Reynolds, W. F. (a)
Rice, D. A. (a)
Rich, J. L. (2,f)
Richards, F. L. (e)
Richards, R. W. (f)
Richardson, G. B. (f)
Riehboch, A. R. (3)
Riesbol, H. S. (g)
Ritchie, D. C. (2,a)
Roberts, K. L. (g)
Roberts, R. B. (d)
Robinow, N. Z. (g)
Rockwood, H. (g)
Roman, I. (1,2,b,d,f,g)
Rooney, W. J. (1,2,3,b,d,e)
Ross, C. P. (2,f)
Ross, C. S. (2,f)
Rossby, C. G. (3,c)
Rubey, W. W. (all)
Rutledge, D. H. (a)
Salteman, E. J. (3)
Samuels, L. T. (3)
Sarle, C. F. (3)
Sashoff, S. P. (3,c)
Saville, T. (g)
Sayre, A. N. (g)
Schairer, J. F. (2,f)
Schaller, W. T. (2,f)
Schmitt, W. L. (3,e)
Schureman, P. (e)
Scott, W. E. (2,3,a,d)
Sellards, E. H. (3,g)
Senior, J. (e)
Shalowitz, A. L. (e)
Shand, S. J. (2,f)
Shepard, E. R. (b)
Sherman, K. L. (2,d)
Sherman, L. K. (g)
Sherry, B. J. (3,c)
Shofnos, W. (a)
Sims, J. H. (g)
Singer, A. D. (2,d)
Slade, J. J., Jr. (g)
Slocum, G. (c)
Small, J. B. (a)
Smith, K. W. (3)
Smith, N. (d)
Smith, P. A. (2,e)
Smith, W. E. (g)
Snyder, F. F. (g)
Sohon, F. W. (2)
Solot, S. B. (3,c)
Soucek, E. (g)
Soule, S. B. (g)
Speert, J. L. (a,g)
Spencer, A. C. (2)
Spicer, H. C. (2,b,d,f)
Spilhaus, A. H. (3)
Stair, R. (3,c)
Stallings, J. H. (g)
Stearn, J. L. (a)
Stechschulte, V. C. (2,a,b)
Steiner, W. F. (d)
Stephenson, E. B. (3,c)
Stephenson, K. (2,a,b,d,f)
Stetson, H. T. (1,2,3,c,d)
Stevlingson, D. J. (3)
Stickley, A. R. (3)
Stone, R. G. (3)
Stose, G. W. (2)
Straley, H.W., III (2,b,d,f,g)
Straub, L. G. (g)
Strong, H. M. (f,g)
Studd, P. F. A. (e)
Sutcliffe, W. D. (1,a)
Swenson, B. (g)
Swick, C. H. (1,2,a,e)
Tannehill, I. R. (3,c)
Tatge, Eleanor (2)
Thiessen, A. H. (3)
Thoenen, J. R. (b)
Thom, H. (3,g)
Thom, W. T., Jr. (1,2,a,b,d,f)
Thomson, M. T. (g)
Thompson, T. G. (3,e)
Thone, F. (3,f)
Thornthwaite, C. W. (3,g)
Torreson, O. W. (1,2,d)
Trager, E. A. (2)
Trask, P. D. (2,e)
Tunell, G. (2,f)
Tung, C. L. (a)
Turner, C. A. (a)
Tuve, M. A. (d)
Twenhofel, W. H. (1,2)
Underwood, P. H. (a)
Urry, W. D. (2)
Vacquier, V. (2,b,d)
Van Orstrand, C.E. (2,a,b,f,g)
Vardy, Mrs. S. M. (c)
Vaughan, T. W. (1,2,3,e)
Vestine, E. H. (2,3,d)
Vhay, J. S. (2,f)
Vine, A. C. (1,a,b)
Wait, G. R. (d)
Walker, H. D. (a)
Wallis, L. G. (2)
Wallis, W. F. (2,d)
Warner, L. A. (f)
Watt, W. G. (3,e)
Weber, G., Jr. (g)
Weightman, R. H. (3,c)
Welden, J. B. (e)
Wells, F. G. (2,f)
Wenzel, L. K. (g)
Westland, A. J. (2,b,f)
Wexler, H. (3)
Whitney, P. C. (1,3,e)
Whitten, C. A. (2,e)
Wiggin, B. L. (3)
Willett, H. C. (3,c)
Williams, G. R. (g)
Williamson, R. O. (a)
Willingham, R. P. (a)
Willis, J. E. (3,a)
Wills, J. E. (g)
Wilson, R. M. (a)
Windes, S. E. (b)
Wood, F. B. (3)
Woodberry, Marjorie (2)
Woodburn, J. G. (g)
Woodworth, R. W. (a)
Woolard, E. W. (3,c)
Woollard, G. P. (a,b)
Woster, H. C. (g)
Wright, C. A. (g)
Wright, F. E. (2,f)
Zies, E. G. (2,f)
Zisman, W. A. (2)
Zoch, R. T. (3)

APPENDIX B--REPORT OF THE GENERAL SECRETARY OF THE AMERICAN GEOPHYSICAL UNION TO THE GENERAL ASSEMBLY, NINETEENTH ANNUAL MEETING, APRIL 29, 1938

The minutes of the eighteenth annual meeting held April 30, 1937, were mimeographed and distributed to all members of the Union, to authors of papers presented at the meeting, and to officers of the International Union of Geodesy and Geophysics. No suggestions for changes or corrections have been received. In Science of July 30, 1937, the General Secretary reported on the eighteenth annual meeting.

Since April 30, 1937, five members of the Union have died: S. L. Moyer, date of death unknown, affiliated with the Section of Hydrology; Lasley Lee, November 15, 1937, affiliated with the Section of Hydrology; R. S. Patton, November 25, 1937, affiliated with the sections of Geodesy, Seismology, Terrestrial Magnetism and Electricity, and Oceanography; William A. Lamb, February 9, 1938, affiliated with the sections of Meteorology and Hydrology; George E. Hale, February 21, 1938, affiliated with the sections of Geodesy, Meteorology, and Terrestrial Magnetism and Electricity.

The total membership of the Union April 29, 1938, is 1,007--a net gain of 79 during the past year. An alphabetical address-list of members showing also their affiliations with sections was prepared and distributed to all members as of date February 9, 1938.

The Executive Committee held two meetings since the last report (May 26, 1937, and January 21, 1938). Copies of the minutes of these meetings were distributed to members of the Executive Committee and to officers of the sections.

The necessary arrangements were made for the nineteenth annual meetings of the Union and of its sections in Washington April 27, 28, and 29, 1938, the program for the scientific sessions including over 130 papers and progress-reports. To facilitate discussion, abstracts of papers to be presented at the symposium of the General Assembly were planographed and distributed before the meeting to members of the Union and others interested.

With the approval of the Executive Committee of the Union, the Committee on Meetings arranged for a smoker for the whole Union through a subcommittee composed of C. S. Jarvis (Chairman), S. H. Lohman, and Gordon Williams. It was held in the evening of April 28 at the Cosmos Club Auditorium and was attended by 125 members and guests. Short talks on the proposed assembly of the International Union of Geodesy and Geophysics to be held September 1939 in Washington were given by the Chairman and General Secretary. A fee of 50 cents per person was collected to pay for cigars, cigarettes, and refreshments. After all expenses were paid, a balance of $7.00 was turned over to the General Secretary of the Union to be retained as a fund for future smokers.

The Section of Hydrology of the Union held a regional meeting at Davis, California, January 7 and 8, 1938, at which 16 papers were presented on the effects of forest and range management on runoff and erosion and other factors affecting the hydrology of watersheds. Eighteen reports and papers on problems in snow-surveying were presented before the Western Interstate Snow-Survey Conference on the afternoon of January 8. There was a regional meeting of the Section of Hydrology at Spokane, Washington, December 28 and 29, 1937, in affiliation with the Northwest Scientific Association.

The General Secretary edited and prepared for publication the manuscripts of 138 papers and reports presented at the eighteenth annual meetings of the Union and of its sections held April 28, 29, and 30, 1937, at Washington, and some 35 papers and reports, not elsewhere published, presented June 21 to 26, 1937, during the meetings at Denver of the Section of Hydrology and of the South Continental Divide Snow-Survey Conference (the two sessions of June 22 were devoted to a symposium on watershed-management held jointly with the Society of American Foresters, and the nine papers presented were published in the Journal of Forestry for November 1937). Eighteen hundred bound copies of Part I (pages 1 to 264) and Part II (pages 265 to 664) of the Union's Transactions for 1937 were planographed. Sixteen hundred and eighty-six copies of these Transactions have been distributed to members of the Union, to authors of papers presented, to foreign geophysicists including members and non-members of national committees for geodesy and geophysics, to institutions and scientific societies, to review periodicals, to persons on the regular mailing-list of the National Research Council, and by sale. An unusually large number of reprints were purchased by authors.

The publication of the Transactions for 1937 was made possible through allotment of $400 by the National Research Council for maintenance of the Union, annual dues from members of the Union, contributions solicited by the Section of Hydrology, contribution from the Carnegie Institution of Washington, and proceeds from sales.

A Bibliography of Hydrology of the United States for 1935 and 1936 (78 pages) was compiled by the Secretary of the Section of Hydrology, and 1000 copies were planographed from master-copy prepared by the General Secretary's office. Four hundred copies were furnished to the Secretary of the International Association of Scientific Hydrology for incorporation in the International Bibliography, 100 copies were delivered to the National Resources Committee of the Department of the Interior, and 500 copies were distributed to members of the Section of Hydrology. The National Resources Committee paid the cost of publication of this Bibliography.

The American Geophysical Union, through its Special Committee on Geophysical and Geological Study of Oceanic Basins, arranged a "Symposium on the geophysical exploration of the ocean-bottom" held November 26, 1937, at Philadelphia during the autumn general meeting of the American Philosophical Society Nine members of the Union took part in this symposium and in the round-table discussion which followed. The papers and discussion have been published in the proceedings of the American Philosophical Society for April 1937.

The National Research Council has included in its estimates for the fiscal year ending June 30, 1939, a grant of $400 to the Union. After consulting with the General Secretary's office, a budget totaling $5050 has been submitted by the Budget Committee of the Union for expenses of publication and maintenance during that fiscal year. The estimated income includes: (1) Balance in bank July 1, 1938, $600; (2) income from 1938 dues, $2850; (3) grant from National Research Council, $400; (4) sales of Transactions and reprints, $1200.

An Auditing Committee examined the accounts of the Union as of date December 31, 1937, and reported the receipts and disbursements exactly as stated in the General Secretary's report of that date.

Following action of the Executive Committee January 21, 1938, a letter was mailed to each member of the Union regarding proposed amendments to the Statutes and By-Laws to permit the election of foreign associate members and to permit the designation of the chairmen and vice-chairmen of the Union and of its sections as presidents and vice-presidents to follow in this regard the practice of the International Union. These proposed amendments are to be voted upon today.

The Union continued its cooperation with the Division of Physical Sciences of the National Research Council in the preparation of special bulletins on geophysical methods, instruments, results, etc. The manuscripts of, and illustrations for, the 13 chapters (over 900 manuscript pages with 284 illustrations) for the manual on terrestrial magnetism and electricity have been completed and will be submitted to that Division in June 1938 for publication as volume VIII of the series "Physics of the Earth." Good progress is being made in the preparation of manuscripts for the manual on hydrology.

A Planning and Project Committee has been appointed by the Executive Committee. Its function is to consider and forward favorable action in the attack on geophysical problems by the Federal Government, State governments, universities, colleges, technical schools, and private research institutions. A circular letter was planographed and distributed to all members of the Union urging them to outline and submit projects in the various branches of geophysics which justify greater attention. Many constructive replies and valuable suggestions were received. A summary of the problems has been prepared by Chairman Bowie of the Committee [see Appendix F].

The Union has received an anonymous gift of $1500 for a post-doctorate fellowship to round out and complete, at Lehigh University under Dr. Maurice Ewing, the special geophysical research on which Dr. George P. Woollard has been engaged for the past two years under the guidance of the Union's Special Committee on Geophysical and Geological Study of Continents. The results of researches in this connection have been reported to several sections of the Union at the nineteenth annual meeting.

The Special Committee on Geophysical and Geological Study of Continents reported that 175 geologists and geophysicists took part in a field-conference last summer in the Bighorn Basin. The importance of establishing gravity, magnetic, and other systems of bench-marks was emphasized as a Federal obligation. Once these systems are set up, we may expect each of the states to take up detailed geophysical work. This Committee also reported rapidly growing interest in the relationship of geophysics and geology and in the field of applied geophysics.

The Special Committee on Geophysical and Geological Study of Oceanic Basins reports the British Navy, Cambridge University, and various societies in Great Britain are actively cooperating in the forwarding of geophysical and geological study of oceanic basins. In the conferences at London early in 1938 Chairman Field of that Committee took active part. Already definite plans have been made by the British Admiralty for submarine cruises for the determination of gravity. Much progress may be anticipated during the next year by the cooperating British agencies.

The Union maintained its relations with the international body. The first circular to delegates and guests, English-French and English-Spanish provisional programs, and other pertinent matter were planographed and distributed to the representatives of governments and organizations adhering to the International Union of Geodesy and Geophysics, to all other governments and organizations interested in geodesy and geophysics, and to all members of the American Geophysical Union. The International Union anticipates that this Assembly will reflect in its proceedings the importance and value in human endeavor of geodesy and geophysics, and that it will further cement and enhance international cooperation so essential to the development of natural resources.

Interest in geophysics is greatly increasing throughout our country, and in this the efforts of our Union have had substantial share. The Union may look forward to further and rapid growth in this interest through the activities of the special committees on Geophysical and Geological Study of Oceanic Basins and of Continents, and the newly formed Planning and Project Committee.

(Signed) Jno. A. Fleming, General Secretary
American Geophysical Union

APPENDIX C--REMARKS OF RETIRING CHAIRMAN, AMERICAN GEOPHYSICAL UNION, APRIL 29, 1938

A good precedent has been established in not having an annual address from the Chairman. However, it seems to me that a brief review of accomplishment and of failures of accomplishment can well be presented at the end of a three-year term of office. This serves the purpose of a useful review and a guide to the future. The progress of the Union as a whole is of great interest but it is necessary to confine my attention to certain features.

(1) Growth of the idea of a Union--Exemplified in various ways as by joint meetings and by success of a number of joint committees mentioned in General Secretary's report.

(2) Growth of membership--Our steady growth is evidenced by the present membership of more than 1,000.

(3) International Assembly in Washington--The invitation to the International Union, made at Edinburgh in 1936, to hold its Seventh Triennial Assembly at Washington in 1939, was accepted and plans are now well developed and organization of committees is partially effected.

(4) Publication problem--Thus far, the establishment of the proposed Journal has not been feasible. The steady increase in length of the *Transactions* indicates the great desirability of realization of an American Journal of Geophysics.

(5) Organization suitable to increased membership--Some progress through providing a clerical assistant to the General Secretary has been made. The duties devolving on the General Secretary, who also has given generously of his time in editing our *Transactions*, are entirely too heavy.

(6) Development of Section of Hydrology--The Section of Hydrology has developed rapidly, especially through the holding of regional meetings in the West. This Section is making a most important contribution in matters of vital importance.

(7) Broad view of aims in geophysics--The rapid development of scientific and economic geophysics has made necessary a Planning and Project Committee.

The principal needs may be summarized as follows:

(a) Continual development along lines indicated
(b) Solving of problems of organization and publication
(c) Increase in membership, with 1,500 as aim in 1939

I am a strong believer in the future of the Union and am sure that it is going to continue to make a most important contribution to the public welfare. I wish to express my appreciation of the fine cooperation that I have received from officers and members and my enjoyment of my duties under these conditions.

(Signed) N. H. Heck, Chairman,
American Geophysical Union

APPENDIX D--REPORT OF SPECIAL COMMITTEE ON GEOPHYSICAL AND GEOLOGICAL STUDY OF CONTINENTS

W. T. Thom, Jr., Chairman

L. H. Adams
N. L. Bowen
W. Bowie
G. C. Branner
R. T. Chamberlin
E. Cloos
M. Ewing
J. A. Fleming
O. H. Gish
W. R. Gregg
B. Gutenberg
M. K. Hubbert
S. F. Kelly
E. S. Larsen
F. W. Lee
C. R. Longwell
J. B. Macelwane
G. R. Mansfield
O. E. Meinzer
L. B. Slichter
H. R. Wanless
B. B. Weatherby

The Special Committee on Geophysical and Geological Study of Continents has now completed its third year. During this time it has determined how its activities should relate to, and supplement, those of preexisting agencies interested in geophysical and geological study of continental areas. It has contributed to, or sponsored, a number of test-case researches, aimed at the testing of various appliances and techniques of research, at the determination of the relative effectiveness of different methods and instruments, and at the improvement of methods for the geologic interpretation of geophysical data. It has also made such contributions as it could to such geophysical-geological studies of North American areas as have been undertaken by the Commission on Continental and Oceanic Structure of the International Union of Geodesy and Geophysics.

The work completed by the Committee during the past year, its activities now in progress, and its recommendations for future work may be summarized as follows:

Work completed during past year

Gravitational-geological study of New England--As described in Longwell's paper [p. 84] given during the current sessions of the Union, his collaboration with the United States Coast and Geodetic Survey in a geophysical-geological study of New England has brought out striking and most interesting gravitational-structural correlations. This study reveals that the local gravity-anomalies in New England are systematically related to fairly deep-seated, regional structural features of the Earth's crust, and also demonstrates the continuity and unity of the gravitational-structural "picture" of New England with that of the Appalachian Mountain System from the Canadian border at least to Georgia.

Professor Longwell's study shows that the belt of large negative anomalies, found by Woollard to extend from Lake Champlain, southwestward to Georgia, parallel to the strike of the Appalachian folding, is, in turn, paralleled through western New England by a north-south "positive strip" which crosses western Massachusetts and most of Connecticut, and then turns southwestward to join the previously described belt of "gravity highs" from New York City to Trenton to Philadelphia to Washington to Central Virginia to Central Carolina. East of this "positive strip" the gravity-values in New England diminish to a less-marked "negative strip" which extends north and south through the Narragansett Basin (of Rhode Island and Massachusetts)--presumably passing southward offshore, probably to reappear again in the area of large negative anomalies shown to exist in the Norfolk-Dismal Swamp Area by the gravity-profile from Petersburg to Cape Charles described by Swick.

Plainsboro-Normandy Beach (New Jersey) seismic profile--The seismic profile from Plainsboro to Normandy Beach, New Jersey, described in the paper presented at the present sessions by Woollard, Ewing, and Johnson [pp. 98-107], has been a further major contribution both to geological knowledge and as a demonstration as to how such geophysical-geological studies should be devised and conducted. The project was jointly sponsored by the New Jersey Geological Survey, Lehigh University, and by this Committee of the American Geophysical Union, and was executed by Drs. Ewing and Woollard, with support from the State Geological Survey. This seismic traverse (like that from Petersburg to Cape Charles) has not only given an index to local subsurface conditions but has provided a line from which a similar seismic traverse can be projected across the Continental Shelf (and possibly out into the depths of the Atlantic).

The results of this seismic survey, checked by previous magnetometer reconnaissance-work, and more particularly by evidence available from drill-holes along the traverse-line, are of great scientific interest, as they show the continuation of the Triassic block-fault system beneath the Coastal Plain sedimentary cover, afford an indication of the magnitude of the topographic relief of the "basement"-surface of the sub-Coastal Plain, and give a further suggestion as to the existence of belt-like zones of intrusives embedded in the invisible "basement"-rocks, at once parallel to the Appalachian folds, and to the gravity-anomaly "strips" mentioned above.

Meetings and conferences--During the year, many members of the Committee have attended and taken part in geophysical-geological meetings and programs, among which those of the Society of Exploration Geophysicists, the American Institute of Mining and Metallurgical Engineers, and the American Association of Petroleum Geologists may be especially mentioned.

Other studies which the Committee considers especially interesting are mentioned below, even though these have been executed without the Committee's participation in them.

Virginia-Carolina gravity-profile--A new gravity-profile, parallel to the Petersburg-Norfolk one, and extending from Lunenburg County, Virginia, to Kitty Hawk, near Cape Hatteras, has been established during the past year by the United States Coast and Geodetic Survey at the request of Drs. Miller and Ewing of Lehigh University.

Bethlehem (Pennsylvania) gravity-profile--A north-south gravity-profile, passing through Bethlehem, Pennsylvania, and crossing the main Appalachian "negative-anomaly strip" was also established during the past year by the United States Coast and Geodetic Survey.

Pittsburgh-Washington gravimetric and magnetic profiles--During the past year, detailed profiles were run between Pittsburgh and Washington by members of the research staff of the Gulf Research and Development Company, using the gravimetric and magnetic balances. The results of this work, described by Dr. L. L. Nettleton at the October (Pittsburgh) meeting of the American Association of Petroleum Geologists, show that observed gravitational and magnetic anomalies correlate closely with the depth to, and structure of, the basement-rocks of this region, and apparently indicate that the folding of the surficial sedimentary rocks does not extend downward into the subjacent "basement."

Sierra Nevada "root-conditions"--An analysis of available seismic data, made by Dr. Perry Byerly of the University of California, appears to show that the Sierra Nevada has a downward projecting "root" of granitic rocks--a conclusion previously reached by Professor A. C. Lawson on gravitational and geological grounds. To quote Byerly's conclusion: "Lawson's conclusions as to a deep downward protuberance of the granitic mass of the Sierra Nevada in the vicinity of Mount Whitney is supported by seismometric data."

Determination of trend and dip of fault-surface by seismic analysis--From analysis of the amplitudes and first motion of the earthquake of July 6, 1934, Dr. Byerly has also been able to predict the position, trend, and dip of the fault-surface of the fault on which the slip productive of this quake occurred. The fault in question lies in the offshore area northwest of Cape Mendocino and is closely related to, or is a continuation of, the San Andreas fracture-system.

Geophysical-geological survey of Schleswig-Holstein--Detailed gravimetric and seismic maps of Schleswig-Holstein, because of their general as well as local interest, were reprinted in the April 1939 issue of the Bulletin of the American Association of Petroleum Geologists by Kauenhowen. The especially important point regarding the seismic map accompanying this article is its demonstration of what many have previously inferred on more or less insufficient evidence--namely, that seismic travel-times are not equal in all directions within a given region, but show appreciable variations according to the "grain" of the local structure in the rocks through which the waves are being propagated. To quote from Kauenhowen's comment apropos this map: "In a strict sense, this map does not represent an ordinary contour-map, but a map of equal times of arrival of seismic waves, that is, of the seconds needed by the seismic energy to arrive at a distance of four km from the shot-point. The times of arrival were determined by refraction-fan shooting and were checked by time-distance profiles. A very characteristic feature revealed by this work is the existence of at least four north-northeast trending, horst-like ridges, each of which is approximately 50 km long and six km wide....."

Kauenhowen also goes on to point out that the seismic map is descriptive of the structure of the first "high-speed" bed in the local sedimentary column, whereas the accompanying gravimetric map is markedly dissimilar, apparently because the gravitational effects are descriptive of a different and much lower "floor."

Work in progress and test-areas under study

Alabama-Florida-Georgia-Coastal Plain Region--Through the collaboration of the State Geologists of Alabama, Florida, and Georgia with the United States Coast and Geodetic Survey, a large number of gravity-profiles are now in progress of establishment. The stations comprising these profiles are being located near deep wells, at significant formational outcrops, or in relation to magnetic "highs" and "lows" as revealed by previous magnetometric surveys--in order that the regional pattern of gravity-anomalies may be determined, and the correlation of the gravity-anomalies with magnetic anomalies and with basement-structure may be ascertained. Through these new gravity-surveys, a "tie" will be effected with the Gulf-West Indian net of marine and land stations, and a partial tie will be made with the gravity-profile system now available for the Central and North Atlantic Coast States.

Washington-Pittsburgh Region--Experimental magnetic and gravitational studies are being continued in the Washington-Pittsburgh Region partly to ascertain the surficial configuration of the "basement"-complex and partly to determine the form and position of the intrusive masses embedded in its older component metamorphics. Intensive studies of the structure of the metamorphics and intrusive rocks of portions of the Region in central Maryland are being carried forward by Cloos and others.

Illinois Basin Region--Much geophysical and geological work has been and is being done in the Illinois Basin, both by the Illinois Geological Survey and by oil-company parties and others. Much deep drilling has been and is being done in the Region and its seismicity is in considerable degree observable from the seismological observations in and near St. Louis and elsewhere near the Region. It is, therefore, one which may prove highly advantageous for the calibration of instruments and the testing of methods. Hence, Wanless has been making very comprehensive well-log studies of formational variations within this Basin, in connection with his broader correlation-studies which, in turn, provide important sidelights upon the time and manner of development of this Basin.

Big Horn Basin Region--Structural studies are being continued in the Big Horn Basin Region along gravity-profiles already established by the United States Coast and Geodetic Survey, and it is planned to supplement this work with further magnetometric and second-order gravimetric profiles during 1938-39, if possible.

Test-area studies projected

New England Region--Additional gravity-profiles are needed in northern and eastern New England, and along the axis of Long Island, and many second-order gravity-observations and magnetic traverses are likewise needed.

New Jersey-Carolina Region--The New Jersey Geological Survey and United States Coast and Geodetic Survey have arranged for gravity-profiles along the Plainsboro-Normandy Beach seismic traverse southward to intersect the Norfolk-Petersburg seismic-and-gravity profile, and continue at least across North Carolina. A gravity-profile perpendicular to the coast, and to this coastal profile, is also planned along a line between Ocean City, Maryland, and Washington, D. C.

Louisiana-Texas Coastal Plain Region--The Louisiana-Texas Coastal Plain Region is a particularly favorable one for further geophysical-geological studies aimed at the ascertainment both of regional crustal structure, at the structural evolution of the Region during Cretaceous and post-Cretaceous time, and at the "border-conditions" along the present and former margin of the Gulf of Mexico. Using outcrop and deep-well information and such geophysical data as may be publicly available, it is planned to select a north-south profile-line near the Louisiana-Texas State-Line for seismic, magnetic, and gravitational traversing. This line should be extended southward into the Basin of the Gulf of Mexico, and northward to, or nearly to, the outcrop of the bases of the Cretaceous formations in north-central Texas.

North Dakota Region--The North Dakota Geological Survey is greatly interested in the establishment of gravitational and magnetic bench-mark networks in the State--both as an index to regional subsurface conditions and as an aid to detailed commercial exploration aimed at the location of possible oil-field structure. Through a "tie" between such a study of this Dakota Basin area with the Black Hills-Yellowstone profiles, very interesting comparisons and correlations will be possible.

Sierra Nevada Region--Studies of the Sierra Nevada Region by Cloos, Lawson, and Byerly indicate the desirability of further geophysical studies at critical geological localities in this Region.

Coast Range-San Andreas Region--Numerous areas in western and southwestern California afford particularly good areas for testing geophysical methods and instruments, as well as for the revelation of geologic structure by geophysical means. Work in this Region has, however, been well organized for a number of years, and hence this Committee has been individually interested, rather than collectively active, in this particular sector.

Studies recommended in preparation for 1939 Assembly of International Union of Geodesy and Geophysics

"Basement-surface" map and model--A basis for direct visual correlation between geophysical anomaly-systems and crustal structure can best be provided by simultaneous reference to (1) maps showing quantitatively the regional distribution of gravity-anomalies, (2) a map of the United States showing (quantitatively) the structure of the near-surface stratified rock covering the Continent, and (3) a map and model showing the configuration and something of the composition of the "basement-complex" beneath the sedimentary mantle.

Map (2) is nearing completion, through the agency of the Tectonics Committee of the Division of Geology and Geography of the National Research Council and under the guidance of Professor Longwell, and copies of it may, it is hoped, be available for distribution by September, 1939. Also, as reported previously, a contour-map showing the configuration of the "basement-surface" has been compiled for a large part of the United States (and a rough relief-model, made from a preliminary version of this map, was exhibited at the International Union meetings in Edinburgh in 1936). Revision and amplification of this "basement"-map is now both possible and highly desirable as a result of much new information which has been collected, and the construction of a new and more "finished" relief-model seems correspondingly desirable.

Regional geophysical-anomaly maps--Several of the regional maps showing geophysical anomalies on a quantitative basis (such as were referred to under (1) of the foregoing section) have already been constructed. It is, however, highly important that this map-collection be expanded and that an adequate collection of "type" maps (showing known correlations between geophysical mapping and ascertained geologic structure) be accumulated--both for the use and information of this Union's membership and as bases for effective exhibits at the 1939 meetings of the International Union.

Other Committee recommendations

Establishment of geophysical bench-mark systems--From both a scientific and practical point of view, the most important and urgent problem in North American geophysical-geological work is connected with securing adequate systems of geophysical bench-marks--particularly gravitational and, even more particularly, magnetic. Such bench-mark systems are even now imperatively needed both for commercial exploratory work and for supporting studies by State, Federal, and other research agencies, and this need is rapidly becoming more acute. Probably the greatest obstacle to the initiation of an adequate program of such geophysical bench-mark establishment will lie in the difficulty which may be encountered in persuading the necessary authorities that work which will cost so little will be worth so much.

This need for geophysical bench-mark systems was recognized by unanimous vote of the Association of American State Geologists more than a year ago, and since that time the wisdom of their action has been greatly emphasized by many happenings, especially in the States where subsurface exploration for, or production of, mineral resources has been in progress.

In connection with, or as a part of the work of, establishing a geophysical bench-mark or reference, it seems highly important that more seismograph-stations be established within the United States, and especially within the Rocky Mountain Region. It seems certain that better recording of the many minor earthquakes (which are known to be occurring, but which are too local and weak to be effectively recorded at existing stations) would give a great deal of important scientific information. Such records might also give timely warnings concerning the possibility of special engineering hazards in important areas.

It seems evident that the establishment of a series of seismograph-stations in the National Parks, and at many more State schools, would serve useful educational purposes, as well as provide guides to the location of active faults, and of faults which may be about to become active.

Map showing "active" faults--In connection with the seismograph-studies just mentioned, it is very desirable that the appropriate agencies of the several Rocky Mountain States, or the United States Geological Survey, severally or jointly, proceed with the compilation of maps showing the traces of faults, either known to be subject to present seismic activity or which are marked by fault-scarps of such freshness as to indicate that these faults are at least potentially active. Such maps would be highly serviceable both to the student, and to the public-works engineer.

Study of seismic travel-times in unhomogeneous terranes--It has been amply demonstrated that seismic wave-trains do not show equal velocities in all directions through near-surface rocks which have a definite structural "grain," which have an appreciable dip, have undergone unequal compaction, etc. It is therefore especially important in the exact "fixing" of epicentral positions for near-by and relatively weak earthquakes, that available travel-time data be further compiled, and that further experimentation be made (using quarry-blasts, or otherwise), whereby directional variations in both the intensity and rapidity of seismic-energy transmission be qualitatively determined for all major types of sedimentary, igneous, and metamorphic rocks under differing (and known) structural conditions.

Preferably this work should be done where detailed gravitational and magnetic studies of the terrane concerned can also be made.

Additional geophysical-geological profile-studies--The time is ripe for a great number of geophysical-geological profile-studies--many of which can be made by qualified graudate students under suitable guidance. Among the kind of profiles especially suited to execution by advanced students under expert instruction will be the surveying of magnetic, electrical, and second-order gravimetric profiles along lines connecting primary control-points or bench-marks.

Of particular interest along this line will be "repeat" surveys of engineering structures like the Fort Peck Dam, and detailed seismic studies of crustal yieldings induced by the weight of accumulating water--as, for example, at the locality of Boulder Dam, where a number of local but strong earthquake shocks have taken place since the Dam has been closed and the lake has begun to form.

Magnetic-gravitational-altitude studies of active volcanoes--Accumulated information regarding the behavior of active volcanoes (and especially those of Hawaii) has demonstrated the genetic relation between these vents and a major fault-system, the existence of significant rhythms in the activity of these volcanoes, and at least an apparent correlation between the 11-year sunspot-cycle and the periods of maximum rise-and-decline of local volcanic activity. These observations, coupled with geological and geochronological information regarding the eruptive cycles of extinct volcanoes, give exceedingly suggestive hints as to how volcanism may relate to other major Earth processes. And consequently, it seems very desirable that careful studies be made over a period of years at several typical volcanoes, in order to determine what variations in local magnetic and gravitational conditions take place during a lava-cycle, and to what extent the magnetic variations may be attributable to gas movements which precede, accompany, or follow the maximum rises of the lava-columns. As a corollary to this study, a similar set of observations made in the vicinity of a large "mud-volcano" (where periodic eruptives of mud, water, and natural gas occur) would be highly interesting. Apparently the activities, both of the Hawaiian volcanoes and of ordinary "mud-volcanoes," have much in common with the action of oil-wells which are "flowing by heads."

In order to determine the magnitude and rapidity of local crustal elevations and depressions which may result from the intumescence of the magmatic mass at a volcano's roots, it would be highly desirable also to secure and use one or more gravimeters (especially in connection with Hawaiian work). With the demonstration that modern gravimeters can be used to obtain a continuous record, showing changes in elevation of a few inches, it seems fairly certain that very interesting facts regarding the volcanic process could be gained by the employment of instruments of this type for the acquisition of more quantitative evidence over a period of time.

Princeton University,
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APPENDIX E--REPORT OF SPECIAL COMMITTEE ON GEOPHYSICAL AND GEOLOGICAL STUDY OF OCEANIC BASINS

R. M. Field, Chairman

D. C. Barton
W. Bowie
W. H. Bucher
M. Ewing
J. A. Fleming
C. A. Heiland
H. H. Hess
C. O. Iselin
Arthur Keith
J. B. Macelwane
H. A. Marmer
F. E. Matthes
F. P. Shepard
H. C. Stetson
H. U. Sverdrup
P. D. Trask
W. H. Twenhofel
P. Weaver
F. E. Wright

Since the last report of this Committee the geophysical exploration of oceanic basins has become more strongly international both as to scope and method. Until quite recently, the development of new techniques and the improvement of the old have been principally accomplished by purely private or national efforts, sometimes supplemented by international cooperation. With the creation of the Commission on Continental and Oceanic Structure by the International Union of Geodesy and Geophysics at its last Assembly at Edinburgh, there has been, naturally, an increasing tendency to:

(1) Encourage all maritime nations to take a more active interest in the geophysical exploration of the suboceanic lithosphere.
(2) Plan all expeditions from the international rather than from the purely institutional or national point of view. Thus there has been an increasing tendency toward the pooling of international ideas, equipment, and funds, including the cooperation of certain national bureaus, departments, and private institutions of one nation with those of another.

This growth of a distinctly more international approach toward the geophysical exploration of the suboceanic lithosphere has depended, not merely on the international interchange of ideas and their statement in terms of an international report, but primarily on the somewhat more arduous duty of planning and executing experiments and expeditions as "test-cases" of our ideas, hopes, and aspirations.

It will be readily understood that the geophysical exploration of oceanic basins necessitates the cooperation of governmental departments and bureaus, and the accomplishment of this cooperation is entirely dependent upon a thorough understanding and appreciation of governmental rules and procedure, including the temporary politics of each nation. The primary function of any national committee is therefore to promote full national cooperation in the pooling of ideas and resources in the prosecution of its plans, and to cooperate to the best of its ability in the plans of other national committees and organizations, especially in the case of the geophysical exploration of international "territory" such as the suboceanic lithosphere.

One of the outstanding problems of our American Committee has been clearly stated by Professor Walter H. Bucher as follows: "That further progress depends on the development of young men capable to carry on investigations with the newly developed methods. It should not be necessary, for example, to delay the seismic profiles parallel to the Norfolk Line, while Ewing extends the method to deep water. We need an adequate number of such lines before we can formulate ideas as to what they mean. Now that money and equipment are more readily available, we do not have the men."

Thus speaks a geological member of our Committee in his natural desire for further data, fundamental to major problems in diastrophism. While it may be argued that further rigidly controlled research is required in submarine seismic methods, especially by those who are responsible for their inception, the fact remains that we are long on ideas and short of men who combine a knowledge and interest in geophysics, marine engineering, and navigation.

Submarine topography--Without further discussion, at this time, regarding the theories as to the origin of submarine canyons, it seems that all are agreed as to the necessity, not only of further accurate data concerning the topography of submerged continental shelves, but also accurate topographic data on suboceanic areas. Naturally, opinions differ as to the most significant suboceanic areas to be mapped, but there seems to be a fairly uniform desire, on the part of structural geologists, for topographic maps of the West Indian Region, the North Atlantic, and portions of the North Atlantic Ridge. This is primarily because of circum-Atlantic stratigraphic and structural problems which, in turn, are directly related to all theories regarding the origin, permanency, and structure of oceanic basins. At the present time, the chief problem is not the organization of suboceanic surveys as much as the improvement of methods for topographic surveying at oceanic depths. Not only does this technique have to be perfected, but arrangements must be made for the possible cooperation of those agencies which are capable of doing the work. It is interesting to note that at present the rules and regulations governing the United States departments and bureaus are not such as to facilitate this necessary cooperation, and therefore the further development of suboceanic topographic mapping is being planned by the British Admiralty with some advice from the United States Coast and Geodetic Survey.

Marine gravity--As reported in the recent United States Navy-American Geophysical Union Expedition, the invention of the new crystal-chronometer by the Bell Telephone Laboratories has greatly improved the method of determining gravity at sea with the Vening Meinesz apparatus. This, in turn, has led to some discussion between Professor Maurice Ewing, Dr. B. C. Browne, and Dr. F. A. Vening Meinesz as to the modification of the Vening Meinesz apparatus itself. This discussion is particularly pertinent in that the Americans do not own such an apparatus and have had to import one each time they have undertaken a marine gravity-expedition. In summing up the most pressing physical requirements for continued marine gravity-surveys by our Committee in cooperation with the United States Navy, Dr. Ewing states that: "(1) The American Geophysical Union should acquire a pendulum-apparatus for measurement of gravity at sea, the apparatus to be fitted with auxiliary instruments which will provide data for making second-order corrections for motions of the submarine. (2) The American Geophysical Union should acquire a static gravimeter of the type best suited for marine gravity-measurements, to be checked against the pendulum on one or more cruises."

Professor H. H. Hess comments on the important areas for future gravity-surveys as follows: "Though no expeditions are being planned by our Committee at this moment, it might be well to have in mind all possibilities. The oceans contain only a half dozen island-areas of the Indies type which are likely to be associated with a strong negative-anomaly strip. The East and West Indies, and the Japanese Arc, have been, and are being, investigated. The Aleutian Arc, from Alaska to Siberia, the South Georgia Arc, and the New Guinea-New Zealand areas have not been investigated. It is also possible that large anomalies may be found southwest of Peru and west of northern Chile. The most feasible area for attack by purely American agencies would be the last named. Such an expedition might well be combined with a complete circuit around South America in which area gravity-observations are few, and the data thus obtained would be of particular value to geodesists and geologists. On rounding Cape Horn a few stations in the South Georgia Arc might be obtained to test for the presence of a negative strip. If such a long trip were out of the question, a more detailed study of the Peru-Chile Area would certainly be in order. It is of much importance that further gravity-work be done in the Marianne or Ladrone Group, to join the Japanese Arc and the East Indies. Also the area north of New Guinea, eastward through the New Hebrides to Fiji, New Caledonia, and New Zealand is of much significance, as well as the Aleutian Arc. The New Guinea, New Hebrides, New Caledonian, Fiji, New Zealand Area with the Tonga Deep is little known geologically, for the most part, and untested by gravity-observations. It is undoubtedly the most intriguing area for combined sounding- and gravity-observations, and probably offers the best possibility of important contributions to our science. The geophysical surveying of this area might best be accomplished by British or British-American cooperation. Outside of the West Indian Arc, in the Atlantic Ocean, scattered gravity-stations and soundings suggest slight undulations of the crust parallel to the Arc, with a wave-length of about 250 km. This, if true, would also be worth further investigation."

Professor Hess, therefore, makes the following specific suggestions and comments:

"(1) Will all island-arcs show a similar negative strip? The answer to this question depends partly on what is meant by 'Arcs.' The Hawaiian Islands might be considered an arc, by some, but this area does not have a negative strip. Some of the geologically older arcs probably will not show pronounced negative strips, that is, Philippines to Formosa, and along the Ryukyi Islands to south Japan.

"(2) The above indicates that it might not be such a good idea to have a gravity-survey around the Philippines. Vening Meinesz' observations near Manila showed practically no anomaly above the Philippine Deep. There probably was a negative strip there in Cretaceous time, but it has now largely disappeared.

"(3) I do not think it necessarily true that the negative strip should be considered entirely out of isostatic equilibrium, although it probably is to some extent. If the positives on either side be balanced against the negative strip, the whole structure can (perhaps) be considered to be in equilibrium. Certain mass-distribution with heavier than normal rocks near the base of the column and lighter than the normal near the top would also allow the negative strip to be nearly in equilibrium and still allow for its large negative anomaly. If a negative strip is greatly out of equilibrium, it could hardly persist throughout a geological period or a subdivision of the Tertiary."

Seismic methods--The adaptation of the induced seismic method to oceanic depths is being actively prosecuted under the continued direction of Dr. Maurice Ewing, and with the cooperation of the Woods Hole Oceanographic Institution, the Geological Society of America, the du Pont Company, and the United States Army. Following the first seismic records made in over 1000 fathoms, as recently reported before the Geological Society of London and the American Geophysical Union, it is expected that further and more comprehensive experiments will be performed during the late summer and early fall.

It is interesting to note in connection with the American experiments that Dr. E. C. Bullard reported on July 20, 1938, that he had just completed his first seismic survey, by the Ewing method, and with the cooperation of the British Admiralty. Bullard's results show a rapidly increasing thickness of sediments from the tip of Cornwall to the 100-fathom line, but he has requested that the specific data be withheld until he has completely checked his observations.

The further analysis of trans-Atlantic seismic waves in relation to comparative structural studies of the Atlantic, Pacific, and Continental segments of the lithosphere is being promoted by the "International Commission" with the cooperation of the International Association of Seismology. Dr. Helmut Landsberg is in charge of this project. Owing to the lack of oceanic seismic stations in the Atlantic, it has been suggested that a seismograph be installed at Bermuda. Plans are now being completed for such an installation under the management of the Bermuda Marine Biological Station with the cooperation of the Woods Hole Oceanographic Institution and the United States Coast and Geodetic Survey.

It has been suggested that a seismic profile be made across the negative strip in the Lesser Antilles as a further test of the "down-buckle theory." It is also suggested that small local earthquakes might be made use of for the same purpose.

Magnetism--In reference to marine magnetic surveys, Dr. John A. Fleming states:

"In connection with the work of our Committee, I think the 1938 report should emphasize the desirability of encouraging in any way possible the further magnetic survey of the oceans. Recent word from Admiral Edgell as to progress on the Research by the British Admiralty indicates she will be at work in the latter part of 1939. A recent communication from Dr. E. Bouveng, Chief of the Hydrographic Service of Sweden, states that that Office has at present under construction a small vessel for magnetic measurements in the waters surrounding Sweden. It is not entirely a non-magnetic boat, but the crude-oil engine and other iron masses of somewhat appreciable size will be at least five meters distant from the magnetic instruments. The boat will be 17 meters long. The Service hopes that the ship-deviations will be so small or at least constant that entirely satisfactory measurements may result. The first expedition of the new boat will start early this summer and will include measurements of the declination, horizontal intensity, and vertical intensity in the southern part of the waters surrounding Sweden. It is planned that in 1939 and 1940 the Hydrographic Service of Sweden and the Finnish Meteorological Central Office will prepare common measurements in the Gulf of Bothnia. Our Institution is cooperating with the Swedish Government through the loan of a compass-variometer on this vessel. Its operations, in view of its small size, will be confined to the study of magnetic anomalies in the coastal regions.

"As you know, a number of rumors have come that U.S.S.R. is building, or intends to build, a non-magnetic ship for magnetic observations in the Arctic Ocean. In view of the importance of continuity of accurate observations at sea to determine the isoporic movements, it seems our Committee ought to make an effort to encourage several other governments, including notably our own, to make provision of non-magnetic survey vessels."

Sedimentation--It is generally agreed that the most important single contribution that could be made to geological science, in the present state of our factual knowledge, would be the determination of the total thickness of sediments on the ocean-floor. It is believed that no other project within the scope of methods now developed could produce such valuable and far-reaching results. The determination of thickness might best be made in the Atlantic 500 miles from shore, about the latitude of northern Florida, and at depths of approximately 2500 fathoms where a broad expanse of flat ocean-floor could be found. The new seismic method should be used for this purpose, but at the same time cores should be taken with the Piggot sampler in order that the radioactivity and, hence, the age of the sediments could be determined for at least a few feet below the surface.

Importance of geophysics to submarine geology--Since the last report of this Committee, the American Philosophical Society has published the Symposium on the Geophysical Exploration of the Ocean Bottom, as arranged by our Committee and held in the Society's rooms in Philadelphia on

November 26, 1937. Copies of the Symposium may be procured either from the American Philosophical Society or from the Secretary of the American Geophysical Union. At the round-table discussion following the Symposium, Professor W. T. Thom, Jr., Chairman of the Special Committee on Geophysical and Geological Study of Continents, of the American Geophysical Union, spoke as follows:

"As a structural geologist, I have been especially interested in this afternoon's Symposium because of its demonstration that ways are now open for notable advances in my field of scientific interest. Students of structural geology have long been accumulating factual data bearing upon the three-dimensional form, space-pattern, and time-relationships of the deformational crustal features of the continental areas--and great advances have been made in these studies--but each time workers in the structural field have sought to make an analysis of facts in hand such as should disclose the causes and modus operandi of crustal deformation, they have arrived at several conflicting and more or less equally plausible hypotheses, and have found that the needed decisive evidence was beyond reach--because it must be sought in the then-inaccessible suboceanic regions. Consequently, I feel that great importance attaches to the demonstration that the combined use of geological and geophysical-geodetic methods now makes it possible to carry continuous exploratory traverses from the continental highlands to the oceans' depths--thereby revealing, among other things, the morphology of the sea-floor, the true nature of continental margins, the presence or absence of 'continental type' rocks in suboceanic areas, the form and extent of past and present geosynclines, and the possible existence of submerged portions of mountain systems--as, for instance, in the North Atlantic between the Appalachians and their apparent continuations in northwestern Europe.

"I have no doubt that workers in many other fields are similarly elated that the developing geophysical exploratory techniques are opening such extensive and important new areas of investigation. And I feel confident that we shall, within a few years, make great progress in substituting fact for theory in many important scientific directions. In order to obtain the great amount of new data needed, a comprehensive program of cooperative work will obviously be called for--especially since the problems involved transcend the capacities of the agencies of any one country. However, that the necessary facilities can be obtained, seems reasonably certain, both because of the broad general interest of the problems under attack and also because of their major scientific importance."

Acknowledgments--During the past year the work of our Committee has been greatly aided by the following institutions, and governmental bureaus and departments: The Bell Telephone Laboratories, the Research Department of the Gulf Oil Companies, the Woods Hole Oceanographic Institution, the Geological Society of America, the International Union of Geodesy and Geophysics (and its affiliated Association of Seismology), the du Pont Company, the United States Navy, the United States Army, and the United States Coast and Geodetic Survey.

Since there will be a new Chairman of this Committee when it renders its next report, the retiring Chairman wishes to express his sincere appreciation of the work of his fellow-members, and especially to those members who have not only provided valuable and original suggestions but who have also borne the brunt of carrying them out at a financial minimum by means of the cooperative method.

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APPENDIX F--REPORT OF THE CHAIRMAN OF THE PLANNING AND PROJECT COMMITTEE OF THE AMERICAN GEOPHYSICAL UNION, APRIL 29, 1938

The Planning and Project Committee of the American Geophysical Union is designed to assist members of the Union in getting their ideas as to worth-while work presented in an effective way. It is quite evident that an individual is not in a position to do as much towards initiating projects as is a whole organization.

A letter was sent to the members of the Union on November 20, 1937, by the Chairman of the Committee, requesting them to submit projects for consideration by the Committee. A list of the projects that have been submitted up to this time is included in this report.

After careful consideration, the members of the Committee have formulated a method by which the projects submitted to the Committee may be handled. When a project has been submitted by a member of the Union, it is considered carefully by the Secretary of the Committee, or by the Chairman, or by both, and, if it does not seem to be in proper form, the member submitting it is requested to make such changes as seem desirable to the officials of the Committee. When a revised plan or one that is in proper form initially is received, it is sent to the Chairman of one or more sections, depending upon the scope of the project, or to a member of the appropriate Standing Committee, with the request that it be given careful consideration by the Executive Committee of this Section or by the Chairman of the Committee and then returned to the Planning and Project Committee with recommendations. It is considered to be good policy for the Chairman of a Section to appoint a special committee for consideration of any one project that is submitted to it. It does not seem to be best to have a planning and project committee of the Section to cover all the matters that may be referred to it.

Occasionally a project will be of interest to two or more sections. In that case the Chairman of each would be communicated with, with the suggestion that perhaps a joint committee of the two sections should handle the project. Of course, most of the work will have to be done by correspondence because the Union has not sufficient funds to pay traveling expenses involved in meetings of Committee members at one place.

If a project is approved by the special committee of a Section and by the Executive Committee of the Section, it is returned to the Planning and Project Committee of the Union, which in turn considers the project. If the action of the Planning and Project Committee is favorable, the project then goes to the Executive Committee of the American Geophysical Union with the recommendation that it be approved by that body.

After a project has been approved by the Executive Committee, it is probably best for the Chairman or the General Secretary of the Union to write to the member or group of members proposing the project, telling of the action taken and asking what the Union and its Planning and Project Committee may do towards having the project put into effect.

Presumably the member or group of members proposing a project should take the initiative in having it adopted by one or more organizations or institutions. The Planning and Project Committee will do all that is in its power to follow up any move that may be made to have a project or plan put into effect. But necessarily with many projects, the Planning and Project Committee would not be able to take the initiative in having them all properly handled after they have been approved by the Executive Committee of the Union.

Three projects have already been approved by the Planning and Project Committee. Two of these projects were approved by resolutions at the annual meeting of the Union on April 29, 1938, namely, Resolution on magnetic survey of the land and water areas of the United States, and Resolution on geophysical survey of Central Atlantic States, while action by the Union on the third, dealing with a suction silt-sampler, has been deferred pending decision on certain minor details. As soon as these details have been cleared up, the project will be presented to the Executive Committee for action.

Summary of suggestions received

A summary of suggestions which have been made to the Planning and Project Committee by various members of the Union follows. These suggestions have been classified according to sections and cross-references given when a suggestion overlaps two or more sections. Some of the suggestions were made in very complete form with definite proposals as to methods of execution; others involve only suggestive remarks on future work to be encouraged by the Union. No order has been followed in arrangement of the projects. Failure of the Planning and Project Committee to act on any of these suggestions does not imply an adverse reaction. The Committee believes that action of the Union on a project should be deferred until a comprehensive plan for its execution has been made. While the Committee feels that primary responsibility and privilege of execution rest with the individual or group proposing a project, suggestions regarding any of these projects will be welcomed by the Committee. Such suggestions should be sent to A. G. McNish, Secretary for the Planning and Project Committee, 5241 Broad Branch Road, N.W., Washington, D. C.

(A) General

- (1) J. H. Huddle, H. W. Straley III, and W. R. Johnson, University of North Carolina--(a) Completion of "Physics of the Earth" series; (b) education of people through colleges, secondary schools, State geologists, and newspapers to appreciate the importance of geophysical work; (c) resurvey of regions already explored using other methods.
- (2) F. Goldstone, Shell Petroleum Corporation--(a) Preparation of geophysical bibliography; (b) investigation of physical properties of rocks.
- (3) T. C. Poulter, Armour Institute of Technology--Investigation of properties of materials at very high pressure.
- (4) C. F. Merriam, Pennsylvania Water and Power Company--Interchange of ideas between sections relative to handling long series of data.
- (5) C. E. Van Orstrand, Geological Survey--(a) Design and construction of mathematical tables to meet the needs of geophysicists; (b) regular measurements of heat-flow through the Earth's surface; (c) construction of a deep bore-hole in undisturbed regions for geophysical measurements.
- (6) M. P. O'Brien, University of California--(a) Scientific investigation of beach-erosion; (b) application of hydraulic laboratories to study of problems in meteorology and oceanography.

(B) Geodesy

- (1) See item E-2.
- (2) See item C-1.
- (3) P. G. Nutting, United States Geological Survey--Measurement of Earth-tides.
- (4) B. Gutenberg, Seismological Laboratory--See item B-3.
- (5) See item G-2.
- (6) B. Hubbard, Standard Oil Company of New Jersey--Assembly and publication of gravimetric data from all the world.

(C) Seismology

- (1) J. H. Huddle, H. W. Straley III, and W. R. Johnson, University of North Carolina--Seismic and gravitational survey of Atlantic Coastal Plain and the Appalachians where magnetic work has been done and in region around Petersburg, Virginia.
- (2) See item B-3.
- (3) See item A-4.
- (4) R. R. Bodle, United States Coast and Geodetic Survey--Study of regional variations in travel-time of seismic waves.
- (5) L. C. Glenn, Vanderbilt University--Further study of earthquake-movements at various distances from center with view to effect on engineering structures.
- (6) F. Neumann, United States Coast and Geodetic Survey--(a) More intensive study of seismograms; (b) more intensive study of seismic-wave propagation; (c) improvement of existing equipment; (d) increase in number of seismic stations; (e) improvement of method of studying seismograms; (f) study of shaking-table data.
- (7) A. Blake, United States Coast and Geodetic Survey--Investigation of statistical reliability of seismic periodicities.

(D) Meteorology

- (1) C. G. Abbot, Smithsonian Institution--Study of long-range weather forecasting as related to solar variations.
- (2) I. I. Schell, United States Bureau of Agricultural Economics--Investigation of structure and behavior of atmosphere over mountain regions.
- (3) See item A-6(b).

(E) Terrestrial Magnetism and Atmospheric Electricity

- (1) E. W. Eickelberg, United States Coast and Geodetic Survey--More complete magnetic survey of the United States and contiguous waters.
- (2) N. H. Heck and C. L. Garner, United States Coast and Geodetic Survey--Detailed magnetic survey in vicinity of Washington, D. C., coordinated with gravimetric survey.

(E) Terrestrial Magnetism and Atmospheric Electricity--Concluded

(3) J. H. Huddle, H. W. Straley III, and W. R. Johnson, University of North Carolina--(a) Publication of United States Coast and Geodetic Survey charts on enlarged scale; (b) assembly of data obtained by geophysical prospecting companies.

(4) F. Goldstone, Shell Petroleum Corporation--Extensive investigation of state of magnetization of Earth's crust.

(5) Charles E. Bunnell, University of Alaska--Establishment of an observatory for auroral research and related geophysical research at the University of Alaska.

(F) Oceanography

(1) P. Bartsch, Smithsonian Institution--Study of ocean-biology by physical oceanographers.

(2) See items A-6(a) and A-6(b).

(G) Volcanology

(1) H. L. Alling, University of Rochester--Study of volcanic gases and gases in magmas.

(2) E. T. Allen--(a) Gravity- and level-determinations involcanic regions; (b) see item G-1.

(H) Hydrology

(1) L. Aldrich, Los Angeles City Engineer--Assembly of data obtained at various stations bearing on rainfall, storm-water runoff, etc.

(2) D. W. Cardwell, United States Soil Conservation Service, Virginia--See item F-1.

(3) R. C. Farrow, Department of Lands, British Columbia--Establishment of strategic stations for precipitation-studies in relation to runoff.

(4) J. A. Folse, Museum of Science and Industry--Continuation of evaporation and stream-flow studies conducted by the Carnegie Institution of Washington (see C.I.W. Pub. 317 and 400).

(5) W. Gardner, Utah State Agricultural College--Study of movement of water through ground.

(6) E. W. Lane, Iowa Institute of Hydraulic Research--Study of performance of suction silt-sampler.

(7) G. W. Musgrave, United States Soil Conservation Service--(a) Study of flow of water in thin sheets over land; (b) investigation of physical properties of soil affecting intake of surface-water.

(Signed) William Bowie, Chairman,
Planning and Project Committee

APPENDIX G-a--MINUTES OF THE ANNUAL MEETING APRIL 27, 1938, SECTION OF GEODESY, AMERICAN GEOPHYSICAL UNION

The meeting of the Section of Geodesy was held in the Auditorium of the United States National Museum, Constitution Avenue at 10th Street, N.W., Washington, D. C., April 27, 1938. For the first time it was necessary to hold the meeting in two sessions. The first session was called to order by the Chairman, C. L. Garner, at 9:13 a.m. He stated that the minutes of the eighteenth annual meeting held April 30, 1937, had been mimeographed and sent out to members and that no corrections had been received; those minutes accordingly were declared approved as mimeographed and circulated. The registered attendances were: Morning, 81; afternoon, 50.

The report of the Nominating Committee [H. G. Avers (Chairman), J. E. Willis, and E. B. Collins] was presented by Mr. Avers. C. H. Swick was nominated by the Committee for President for three years from July 1, 1938, and R. M. Wilson for Vice-President. No nominations were made from the floor, and Messrs. Swick and Wilson were unanimously elected.

The report of the Committee on Resolutions [C. H. Swick (Chairman), C. B. Watts, and R. M. Wilson] was presented by Mr. Swick, who reported that after careful consideration no resolutions were found to be necessary.

The Section then proceeded to hear the papers on the scientific program as published in these Transactions [pp. 45-96],, and adjourned at 5 p.m.

(Signed) Walter D. Sutcliffe, Secretary,
Section of Geodesy

APPENDIX G-b--MINUTES OF THE ANNUAL MEETING APRIL 27, 1938, SECTION OF SEISMOLOGY, AMERICAN GEOPHYSICAL UNION

Due to the temporary absence of the Chairman and Vice-Chairman, the Secretary. F. Neumann, called the meeting of the Section of Seismology to order at 9:08 a.m. on April 27 in Room 43 of the United States National Museum, Washington, D. C. Chairman F. W. Lee took over the chair during the reading of the first paper and presided until adjournment at 12:58 p.m. The registered attendance was 59.

The first half of the session was devoted to the reading and discussion of scientific papers. At 11:29 a 15-minute recess was called after which a business meeting was held. The minutes of the 1937 meeting were approved as mimeographed and distributed to members. There were no reports of officers.

The Chairman appointed a Committee on Resolutions consisting of M. Ewing (Chairman), T. A. Jaggar, J. B. Macelwane, and H. E. McComb. After due consideration the Committee reported that it had no resolutions to present.

An election was held to fill the offices of President and Vice-President. The Nominating Committee, as published in the preliminary announcement of the annual meeting, consisted of J. P. Delaney (Chairman), M. Ewing, and E. W. Eickelberg, with alternates named for each. As the Chairman-elect was unable to be present and requested to be relieved of duty on the Committee, the chair appointed V. C. Stechschulte to fill the vacancy and act as Chairman. The Committee then named J. B. Macelwane for President and a candidate for Vice-President who was subsequently found to be ineligible. Upon reconsideration the Committee selected H. E. McComb for this office. There were no other nominations and the candidates named were duly elected for three years from July 1, 1938.

At the close of the session a vote of thanks was tendered the retiring Chairman for his efforts on behalf of the Section during his term of office.

Because of the length of the program, the symposium scheduled at the end of the session was limited to brief remarks by the Chairman and the Secretary. The papers presented are printed in these Transactions [pp. 97-132].

(Signed) Frank Neumann, Secretary,
Section of Seismology

APPENDIX G-c--MINUTES OF THE ANNUAL MEETING APRIL 28, 1938 SECTION OF METEOROLOGY, AMERICAN GEOPHYSICAL UNION

The nineteenth annual meeting of the Section of Meteorology, Chairman Brooks presiding, was held from 3:30 to 5:15 p.m., Thursday, April 28, 1938, in the Lecture Room of the National Academy - Research Council Building, 2101 Constitution Avenue, Washington, D. C. The registered attendance of members and guests was 67. The reading of the minutes of the previous meeting was dispensed with, as they had been already circulated and no corrections had been received.

The Committee on Resolutions [C. G. Rossby (Chairman), D. M. Little, and C. W. Thornthwaite] presented a resolution commending the Steering Committee for Meteorological Research Projects under the Bankhead-Jones Act for its judicious encouragement of meteorological research. [This is Resolution (4) given in the minutes of the General Assembly of the Union.]

Chairman Brooks reported problems of the Planning and Project Committee which had come to the Section of Meteorology for consideration as follows:

(1) Scientific location of precipitation-stations to give best indication of total precipitation of a watershed (R. C. Farrow, Columbia River; C. F. Merriam, Susquehanna River).

(2) Atmospheric structure over mountains, especially turbulence and low clouds (I. I. Schell).

(3) Ultra-violet measurements of solar variability at high levels in the stratosphere (C. G. Abbot).

(4) Use of hydrologic laboratories for studies in oceanography and meteorology (M. P. O'Brien).

(5) Bringing into practical use the results of Hayford's studies of the levels of the Great Lakes, especially evaporation and stream-flow (J. A. Folse).

(6) Establishment of a continuously-manned, high-altitude reporting station to be located on the peak by the lookout-station at Deer Park, Washington (P. E. Church for Seattle (Washington) Branch, American Meteorological Society, Forest Service of the Olympic National Forest, and United States Air Lines).

(7) Annotated bibliography of geophysical literature (F. Goldstone).

The Nominating Committee [W. J. Humphreys (Chairman), C. F. Marvin, and H. H. Kimball] nominated for officers for the three-year period beginning July 1, 1938, R. H. Weightman for President, W. C. Lowdermilk for Vice-President, and H. D. Harradon for Secretary. Other nominations made from the floor were H. C. Willett for President, C. W. Thornthwaite for Vice-President, and E. W. Woolard for Secretary, the last declining the nomination. As a result of the ballot, the following nominees were elected: R. H. Weightman, President; C. W. Thornthwaite, Vice-President; H. D. Harradon, Secretary.

On a motion by C. G. Rossby, a vote of thanks was extended to C. F. Brooks for his part in arranging the successful joint symposium of the Sections of Meteorology and Oceanography on atmospheric and oceanic circulation.

There being no further business, the scientific program was presented including the papers printed in these Transactions [pp. 133-156].

(Signed) H. D. Harradon, Secretary,
Section of Meteorology

APPENDIX G-c'--MINUTES OF JOINT MEETING APRIL 28, 1938, SECTIONS OF METEOROLOGY AND OCEANOGRAPHY, AMERICAN GEOPHYSICAL UNION

A joint meeting of the Sections of Meteorology and Oceanography was held in the Lecture Room of the National Academy - Research Council Building, Washington, D. C., April 28, 1938, Chairmen Brooks and Thompson presiding. The morning session was from 9:00 a.m. to 12:30 p.m. and the afternoon session from 2:00 to 3:20 p.m. Registered attendance during the two sessions was 128.

The meetings were devoted to a Symposium on Atmospheric and Oceanic Circulation. Eight papers were presented at the morning session and five at the afternoon session; they are printed in these Transactions [pp. 157-183]. There was not sufficient time for a general discussion.

(Signed) H. D. Harradon, Secretary,
Section of Meteorology

APPENDIX G-d--MINUTES OF THE ANNUAL MEETING APRIL 27, 1938, SECTION OF TERRESTRIAL MAGNETISM AND ELECTRICITY, AMERICAN GEOPHYSICAL UNION

The meeting was called to order by Chairman O. H. Gish at 2:00 p.m., April 27, 1938, in Room 43 of the United States National Museum, Washington, D. C., and was concluded at 5:02 p.m. Eighty persons were in attendance, of which 71 registered their attendance. The report of the Secretary was read and approved [see Appendix G-d(1)].

The Committee on Resolutions [W. J. Rooney (Chairman), H. F. Johnston, and J. W. Joyce] reported through Chairman Rooney a resolution on magnetic survey of the land and water areas of the United States, which was accepted by the Section and referred to the Committee on Resolutions of the Union. [This is Resolution (1) given in the minutes of the General Assembly of the Union.]

The report of the Nominating Committee [A. K. Ludy (Chairman), T. R. Gilliland, and K. L. Sherman] was given by Chairman Ludy recommending E. O. Hulburt for President, E. A. Eckhardt for Vice-President, and E. W. Eickelberg for Secretary, each to serve three years from July 1, 1938. There being no other nominations, these nominees were duly elected.

There being no further business, the papers on the scientific program were presented as printed in these Transactions [pp. 185-222].

(Signed) E. O. Hulburt, Secretary,
Section of Terrestrial Magnetism
and Electricity

APPENDIX G-d(1)--REPORT OF THE SECRETARY TO THE NINETEENTH ANNUAL MEETING OF THE SECTION OF TERRESTRIAL MAGNETISM AND ELECTRICITY

There were no special meetings of the Section during the year. The minutes of the eighteenth annual meeting were sent to all members. No corrections or additions were received.

The dissemination through Science Service of the American world-wide magnetic character-figure has continued without interruption during the year. Through the efforts of the United States Coast and Geodetic Survey and the Department of Terrestrial Magnetism of the Carnegie Institution of Washington, the figure is compiled from the data of seven American-operated observatories well distributed over the Earth, in Maryland, Arizona, Puerto Rico, Honolulu, Peru, Western Australia, and Alaska. The figures for each Greenwich half-day of the week ending Friday are sent out on the following Wednesday.

The regular schedule has been continued of broadcasts of cosmic data by the World-Wide Broadcasting Foundation short-wave, 20-kw station WIXAL, Boston, Massachusetts. The broadcasts are daily from 4:55 to 5:00 p.m. Eastern Standard Time on 11.79 megacycles (25.4 meters) with weekly summaries on Monday evenings 8:30 to 8:45 p.m. Eastern Standard Time on 6.40 megacycles (49.6 meters).

The MacGregor Arctic Expedition, 1937-38, under the command of C. J. MacGregor, Meteorologist of the United States Weather Bureau, is established at its base in northwest Greenland. Included in its program is the operation of a complete magnetic station and extensive auroral observations.

A National Research Council publication dealing with terrestrial magnetic and electric phenomena, edited by J. A. Fleming, Chairman of the Subsidiary Committee, goes to press in June. The volume is the latest of the "Physics of the Earth" series.

Preliminary notices of the nineteenth annual meeting were sent to all members from the office of the General Secretary on February 26, 1938.

In compliance with the objects of the Union as stated in the Statutes and By-Laws, namely, to promote and coordinate the study of various branches of geophysics in America and outlying territories, brief summaries of work carried on by the various organizations and individual investigators during the year have been collected for incorporation in the Transactions of the nineteenth annual meeting. These may be listed briefly as follows:

Dominion Observatory, Canada--Magnetic survey of Canada and territories.
National Astronomical Observatory, Mexico--Magnetic survey.
Mount Wilson Observatory, Carnegie Institution of Washington--Terrestrial magnetic activity, solar activity, solar spectroheliograms in hydrogen light.
University of North Carolina--Magnetic survey of the Coastal Plain.
University of Rochester--Solar ultra-violet light, ozone.
National Bureau of Standards--Ionospheric data, ionospheric storms, radio-wave propagation; dissemination of ionospheric data for cosmic-data broadcast.
Department of Terrestrial Magnetism, Carnegie Institution of Washington--Operation of two magnetic observatories, reduction of magnetic data, ionospheric research, atmospheric electricity, nuclear high-voltage investigation, cosmic rays.
United States Coast and Geodetic Survey--Operation of five magnetic observatories, magnetic survey, instrumental development.

The total membership of the Section is 213, an increase of 14 over last year.

(Signed) E. O. Hulburt, Secretary,
Section of Terrestrial Magnetism
and Electricity

APPENDIX G-e--MINUTES OF THE ANNUAL MEETING APRIL 29, 1938, SECTION OF OCEANOGRAPHY, AMERICAN GEOPHYSICAL UNION

The annual meeting of the Section of Oceanography was held at Washington, D. C., Friday, April 29, 1938, in the Library of the National Academy - Research Council Building. The meeting was called to order by Chairman Thomas G. Thompson at 9:00 a.m. and was concluded at 11:50 a.m. Some 83 members and guests were present. The minutes of the previous meeting, having been mimeographed and distributed to the members and there being no corrections or additions, were approved.

The Chairman called for any unfinished business from the members present. There being none he called for any new business. Under new business the Chairman recognized Lieutenant Smith, Chairman of the Nominating Committee, who gave his report for officers of the Section of Oceanography for three years commencing July 1, 1938, as follows: President, Columbus Iselin; Vice-President, George F. McEwen; Secretary, Paul C. Whitney. The Chairman of the Section then called on the floor for any other nominations to the offices; hearing none, a motion was made, seconded, and unanimously carried to accept the report of the Nominating Committee. The Chairman then declared the election of the officers as named by the Nominating Committee.

There being no further new business, papers were presented as printed in these Transactions [pp. 223-260].

At the conclusion of the papers the Chair recognized Lieutenant Smith, Chairman of the Resolutions Committee of the Section, who read a resolution, passed by the Section of Terrestrial Magnetism and Electricity, on magnetic survey of the land and water areas of the United States. [This is Resolution (1) given in the minutes of the General Assembly of the Union.] After a brief discussion, the Resolution was unanimously adopted.

(Signed) Paul C. Whitney, Secretary,
Section of Oceanography

APPENDIX G-f--MINUTES OF THE ANNUAL MEETING APRIL 28, 1938, SECTION OF VOLCANOLOGY, AMERICAN GEOPHYSICAL UNION

The two sessions of the Section were held in the Library of the National Academy - Research Council Building on April 28, 1938. Chairman E. T. Allen presided until the midpart of the afternoon session, when Vice-Chairman W. S. Burbank presided. The morning session was called to order at 9:10 a.m. and the afternoon session at 2:03 p.m. Eighty-nine members and guests were present. The reading of the minutes of the previous annual meeting was dispensed with as the minutes had been previously circulated in mimeographed form and there were no corrections reported.

There being no unfinished business, the report of the Nominating Committee [D. F. Hewett (Chairman), T. B. Nolan, and J. F. Schairer] was read by Mr. Nolan. E. G. Zies was proposed for President, Adolph Knopf for Vice-President, and James Gilluly for Secretary for three years from July 1, 1938. There being no further nominations, the nominations were closed. It was moved and seconded that the Secretary be instructed to cast a unanimous ballot for the candidates. The motion carried, and the ballot was so cast.

The papers as printed in these Transactions [pp. 261-277] were then presented.

(Signed) Eugene Callaghan, Secretary,
Section of Volcanology

APPENDIX G-g--MINUTES OF THE ANNUAL MEETING APRIL 28-30, 1938, SECTION OF HYDROLOGY, AMERICAN GEOPHYSICAL UNION

The Section of Hydrology held three sessions in the National Academy - Research Council Building on April 28 and 29, 1938, and a round-table discussion in the Interior Department Building on April 30, 1938.

The first session was from 9:10 a.m. to 1:03 p.m., April 28, in the Auditorium with Chairman L. K. Sherman presiding. Ninety-four persons were present.

The Chairman appointed the following Committee on Resolutions: W. D. Collins, H. R. Leach.

Reports of committees were presented as follows: (1) Committee on Underground Waters, D. G. Thompson, Chairman (presented by O. E. Meinzer); (2) Committee on Absorption and Transpiration, C. H. Lee, Chairman (by title); (3) Committee on Runoff, by W. G. Hoyt, Chairman; (4) Advisory Committee on Soil-Conservation Research, by R. E. Horton, Chairman [see these Transactions, p. 342 and following].

Papers were then presented by W. D. Potter and M. V. Baker (read by W. D. Potter), by A. J. Lazenby, by M. L. Brashears, Jr., and by C. E. Jacob [see these Transactions, p. 393 and following].

After a brief recess, and with W. G. Hoyt presiding, papers were presented by L. K. Sherman, W. B. Langbein, F. F. Snyder, W. E. Smith, and B. F. Kimball [see p. 430 and following of these Transactions).

The second session was called to order at 2:15 p.m., April 28, in the Auditorium with Chairman L. K. Sherman presiding and was concluded at 5:17 p.m., with 64 persons present. The minutes of the 1937 annual meeting were read and approved. The Chairman reported informally on the work of the Section for the preceding year. The report of the Secretary [see Appendix G-g(1)] was read and approved. The Committee on Resolutions announced that it would report at the next session. The following amendments to the statutes of the Section, proposed by L. K. Sherman, O. E. Meinzer, R. E. Horton, W. C. Lowdermilk, and K. H. Beij, were presented by the Secretary:

Amend Article III (relating to "Subsection for the Territory of Hawaii") by striking out the entire text and substituting therefor a new article (relating to "Regional Committee").

Amend Article IV by designating the text as Article V.

Insert new Article IV (relating to "United States members of the International Executive Committee").

These amendments were passed by unanimous vote of the 41 members present. [See Appendix E of the minutes of the annual meeting of 1933 for complete text of original statutes. See Appendix G-g(2) of these minutes for complete text of the amended statutes.]

Dr. Meinzer reported briefly on the progress of the volume on "Hydrology."

Following adjournment of the business meeting at 2:30 p.m., reports were then presented, with C. F. Merriam presiding, for the committees on Snow, J. E. Church, Chairman (abstract read by the Secretary); Glaciers, F. E. Matthes, Chairman (by title); Evaporation, including Evaporation from water-surfaces, program of investigations, S. T. Harding, Chairman (by title); Physics of Soil-Moisture, including The effect of organic matter on the rate of entry of water into soils, F. J. Veihmeyer, Chairman (read by N. E. Edlefsen; Chemistry of Natural Waters, C. S. Howard, Chairman (read by W. D. Collins), and Progress-report of the National Research Council Interdivisional Committee on Density-Currents, by H. N. Eaton, Chairman. A paper "Evaporation-studies at Baton Rouge, Louisiana" by G. N. Cox was read as a supplement to Report of Committee on Evaporation. T. Saville spoke on "Contributions of the Water Resources Committee to hydrological investigations. After a brief recess the session was resumed with C. F. Merriam presiding, and papers were presented by R. G. Stone, M. Demorest, and R. T. Knapp. [All of these reports and papers are printed in these Transactions, p. 281 and following.]

The third session was called to order at 9:10 a.m., April 29, in the Auditorium with R. S. Goodridge presiding. This meeting was concluded at 12:24 p.m.; attendance was 101. The Resolutions Committee presented a resolution on permanence of location of meteorological stations [Resolution (2) in minutes of General Assembly of Union] which was adopted without any dissenting vote. Reports were then presented for committees as follows: Dynamics of Streams, by L. G. Straub, Chairman, including "Rain wave-trains" by R. E. Horton, "The Seddon and Forchheimer equations for crest-velocity of river flood-waves" by R. E. Horton, and "A study in the theory of flood-waves" by E. E. Moots. Papers were read by W. C. Lowdermilk and C. H. Pierce. After a brief recess the session was resumed with C. S. Jarvis presiding. Following the report of Committee on Rainfall, by M. Bernard, Chairman, papers were presented by E. Soucek and J. W. Howe (presented by E. Soucek), K. Clarke-Hafstad, C. F. Merriam, J. B. Kincer, C. F. Brooks, H. S. Riesbol, and A. T. Lenz. [All of these are printed in these Transactions, p. 349 and following.]

The round-table discussion of the "Preliminary outline for a comprehensive research on run-off phenomena" by R. E. Horton was called to order, with an attendance of 47, at 9:30 a.m. in Room 5243, Interior Department Building, by W. G. Hoyt, Chairman of the Committee on Runoff. The discussion was concluded at 1:15 p.m.

Chairman Hoyt gave a brief résumé of the purpose for holding the meeting. Mr. Sherman made a few introductory remarks, followed by Mr. Horton who explained the preparation of the report which was the subject for discussion.

Research-activities of the various organizations represented at the round-table were presented as follows: Soil Conservation Service, C. E. Ramser; Forest Service, E. N. Munns; Corps of Engineers, U.S.A., B. A. Hathaway; Forest Service (San Dimas project), E. J. Kotok; Geological Survey, R. W. Davenport; Geological Survey (ground-water), O. E. Meinzer; Bureau of Agricultrual Engineering, L. A. Jones; Soil Conservation Service (Coshocton project), W. D. Ellison;

National Hydraulic Laboratory, K. H. Beij; Weather Bureau, M. Bernard; and National Resources Committee, I. Bloch.

The remainder of the meeting was devoted to discussion of Mr. Horton's report by L. K. Sherman, W. W. Horner, Franklin Snyder, Merrill Bernard, D. B. Krimgold, C. R. Hursh, L. K. Hill, Waldo E. Smith, W. B. Langbein, and W. G. Hoyt.

The total registration of members and guests at the regular sessions and at the round-table was 151.

(Signed) K. Hilding Beij, Secretary,
Section of Hydrology

APPENDIX G-g(1)--REPORT OF THE SECRETARY, SECTION OF HYDROLOGY, APRIL 28, 1938

The past year has been marked by a significant increase in the scope and extent of the activities of the Section. In particular, our relations with the International Association are becoming increasingly strengthened, mainly because of the forthcoming Assembly at Washington in 1939.

Meetings--In addition to the regular annual meeting in April 1937, there have been two regional meetings of the Section. The first, at Denver, June 21 to 26, 1937, was arranged by the South Continental Divide Regional Committee, Royce J. Tipton, Chairman. At this meeting joint sessions were held with the Society of American Foresters and the South Continental Divide Snow-Survey Conference. The second regional meeting, at Davis, California, January 6 and 8, 1938, was arranged by the South Pacific Coast Regional Committee, Morrough P. O'Brien, Chairman, and F. J. Veihmeyer, Chairman of the Research Committee on Physics of Soil-Moisture; joint sessions were held with the Western Interstate Snow-Survey Conference.

Publications--In part 2 of the Transactions of the Union for 1937 are published the reports and papers of the 1937 annual meeting (242 pages), of the Denver regional meeting (92 pages), and of the South Continental Divide Snow-Survey Conference at Denver (63 pages). Papers at the joint meeting with the Society of American Foresters at Denver are published in the November 1937 issue of the Journal of Forestry.

The first American contribution to the International Bibliography of Hydrology, covering the years 1935 and 1936, was published in 1937. Publication was made possible by the cooperation of the Union and of the National Resources Committee. The Bibliography consisted of about 400 entries making a booklet of 78 pages. Copies were sent to all members of the Section.

Regional Committees--The Committee on Regional Organization under the chairmanship of J. E. Church has been very active. Four regional committees have been formed. The membership of these committees and notes on their activities are given on pages 512 and 513 of the Transactions for 1937.

Special Committees--Those formed during the past year are: (1) Advisory Committee on Soil-Conservation Research, Robert E. Horton, Chairman; (2) Committee on Flood-Waves and Allied Phenomena, Lorenz G. Straub, Chairman. Their reports are included in the Transactions for 1938 [pp. 351-386].

International statutes--A translation of the statutes of the International Association has been prepared and arrangements are being made to have it officially accepted by the International Executive Committee.

Membership--The membership has increased by 49 during the year, making a total of 507 for the Section at this date.

(Signed) K. Hilding Beij, Secretary,
Section of Hydrology

APPENDIX G-g(2)--STATUTES OF THE SECTION OF HYDROLOGY, AMERICAN GEOPHYSICAL UNION
(As amended April 28, 1938)

[Note: The objects, membership, officers, and meetings of the Section are determined by the Statutes of the Union (see Transactions, June, 1930, pp. 9-12, and later amendments).]

ARTICLE I

There shall be an Executive Committee which shall have charge of all administrative matters of the Section. The members of this Committee shall consist of the President, Vice-President, and Secretary of the Section, and of such additional members as the President shall appoint. The officers of the Section shall also be the officers of the Committee.

ARTICLE II

The scientific work of the Section shall in part be conducted by Permanent Research Committees, each of which shall devote itself to a designated field of the science of hydrology, and shall make an annual report to the Section covering that field, especially with reference to the research-work that is in progress, the results of research-work recently completed, and matters in relation to which early research is desirable. The number of Permanent Research Committees and their respective fields shall be determined by the Executive Committee. The Chairman and members of each Committee shall be appointed triennially by the President of the Section, with the approval of the Executive Committee.

ARTICLE III

In addition to the annual meeting, other meetings of the Section of Hydrology may be held within certain regions designated by the Executive Committee. A Committee for each designated region shall be appointed. It shall be the duty of this Regional Committee to determine the time and place of meetings and to supervise the programs thereof. Papers presented at Regional meetings shall be received subject to the publication requirements of the Section. Chairmen and members of Regional Committees shall be appointed in the manner specified in Article II relating to Research Committees.

ARTICLE IV

The United States members of the Executive Committee of the International Association of Scientific Hydrology shall consist of the Secretary and one member appointed by the Executive Committee of the Section of Hydrology.

ARTICLE V

Amendments to these statutes shall be made in the manner prescribed for amending the By-Laws of the Union.

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NATIONAL RESEARCH COUNCIL

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TRANSACTIONS OF THE AMERICAN GEOPHYSICAL UNION
NINETEENTH ANNUAL MEETING
APRIL 27 TO 30, 1938
WASHINGTON, D. C.

REGIONAL MEETINGS
DECEMBER 28 TO 29, 1937, SPOKANE, WASHINGTON
JANUARY 7 TO 8, 1938, DAVIS, CALIFORNIA
JUNE 21, 1937, DENVER, COLORADO (ADDENDUM)

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REPORTS AND PAPERS
SECTION OF HYDROLOGY
AND
WESTERN INTERSTATE SNOW-SURVEY CONFERENCE

PUBLISHED BY THE NATIONAL RESEARCH COUNCIL
OF
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NEED FOR RESEARCH IN THE FIELD OF HYDROLOGY

O. W. Monson

This paper is published in Northwest Science, v. 12, No. 2, p. 28, May 1938.

Montana State College,
Agricultural Experiment Station,
Bozeman, Montana

ICE-CONDITIONS ON THE MISSISSIPPI RIVER AT DAVENPORT, IOWA

T. G. Shipman

(Read at American Meteorological Society, Minneapolis, June 27, 1935, but revised and published as part of the program at Spokane, Washington, December 28-29, 1937)

These records are compiled from various sources. Records from 1841 to about 1850 are from those kept by Dr. Donaldson and his father of Pleasant Valley, a few miles above Davenport. Records from 1851 to 1871 are taken mainly from two sources, namely, C. H. Stoddard of Rock Island, Illinois, and Mr. Robins, Superintendent of the Government bridge at Davenport, Iowa. Some records seem to have been kept at Rock Island Arsenal and Dr. Donaldson also continued his during this time. The records since 1871 are those of the Signal Corps of the United States Army and the Weather Bureau. The records have been compared where they parallel each other and preference given to those in agreement. When they show not more than one day's difference, they were considered in agreement since one observer may have observed the phenomena in the afternoon and the other may not have recorded it until next morning when making his river-observation. Unusually long seasons of closed navigation and those of open navigation were checked with existing records of temperature and other reliable data. Thus the season of 1842-43 with 155 days of closed season was found to be very cold with a late cold spring.

Opening and closing of river-navigation were watched from the earliest settlements. These records were given the same importance and attention as river-stage, precipitation, temperature, and frost-data are now given. Complete records are available at Davenport since 1841. The early records were made by public-spirited men voluntarily and those later by Governmental institutions.

River-information was valuable at these early dates to lumbering interests and river-navigation. The lumbering interests watched the closing date to start cutting and placing the log-booms on the ice in the winter. They watched the opening date in spring to loosen the booms and rafts and float the product to market. River-boats and transportation companies watched the opening date to start boats as early as possible and the closing date to continue them as late as possible. In the spring there was a race to reach port first since the winning vessel received free port-service for the season. Streams were used by the early settlers as avenues of communication and in the winter ice was used as a bridge for all kinds of traffic.

The average date of navigation-closing at Davenport is December 24, as determined from 94 years of record. The earliest date was November 7 in 1842; in all there are seven instances of closing in November in the records. There are also seven seasons in which the river did not close.

The normal temperature for Davenport on the closing date of December 24 is 24°.6 F. Taking the average temperature two days before closing, one day before closing, and on the closing date, and averaging them since 1874--when maximum and minimum thermometers were used in Davenport the first time--we obtain a three-day average of 13°.1. We would expect temperatures below freezing all day and minima of 4° to 9° with this average, while the river was closing. Actual tabulation of temperatures bears this out to a large extent.

The average daily stage since 1874, one day before closing, is 2.6 feet. The normal stage for December, the month of average closing date, is 2.7 feet. A three-day tabulation of averages shows an increase of 0.3 foot in stage-height during the closing process. This might be explained by the fact that water increases one-tenth in volume in freezing.

The average date of opening navigation in spring at Davenport is February 28, being a little later in the early part of the 94-year record and a little earlier in the latter part. The latest date of opening was April 7, 1842, and there were seven seasons without opening date since the river did not close. Averages of mean temperatures two days before opening, one day before opening, and on the opening date, since 1874, show a three-day average of 39°.7. This is near 39°.0, when water is densest. This average would indicate that the temperatures would remain above freezing during most of the time during the days on which the ice was breaking up. However a study of the records shows there is more variation in temperatures at opening time than at closing time. The normal temperature for February 28, the average opening date, is 29°.0. The average stage is 4.8 feet three days before opening, 6.0 feet on the opening date, and 6.7 feet two days after opening. There seems to be almost a two-foot swell in the river during opening in the spring. This may be due to frost-release, melting snow, melting ice, ice-gorges, or an accumulation of all these.

The air-temperature at navigation opening shows a three-day average of 39°.7, which is very close to 39°.0, the temperature at which water is densest. From this it seems that as fast as water is warmed to 39°.0 it sinks and is replaced by colder but lighter water at or near 32°.0 from melting ice. This process accelerates the melting and disintegrating process by keeping the colder water and ice in contact with the overlying air which is several degrees warmer. The ice melts and does not sink, popular opinion to the contrary sometimes not withstanding. It melts very rapidly and does not move down-stream very far. Several days of floating ice indicate that the ice is coming from nearby tributaries rather than from far up-stream. Other factors than average temperature which seem to influence the length of the closed season are fluctuating river, high river, and rapidly fluctuating temperatures.

A curve (Fig. 1) has been prepared plotting the ten-year average length of closed season against the year. Thus on the scale for the year 1850 is placed a dot indicating the average length of the closed season 1841-50. Thus we have a curve showing successive ten-year averages of the length of the closed season. This shows in general that the closed seasons were longer

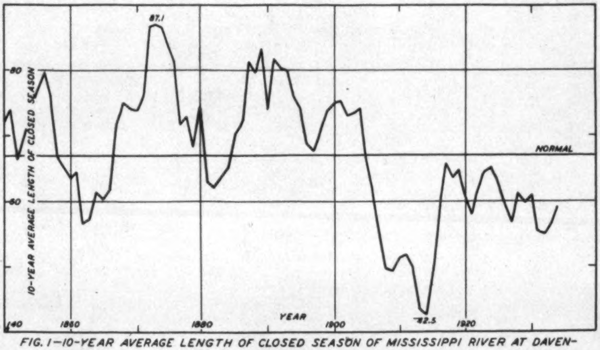

FIG. 1—10-YEAR AVERAGE LENGTH OF CLOSED SEASON OF MISSISSIPPI RIVER AT DAVENPORT, 1841-1935

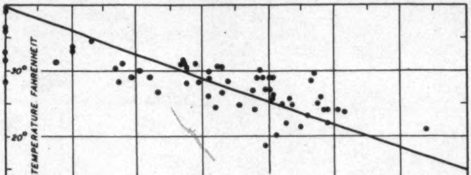

FIG. 2—AVERAGE TEMPERATURE DECEMBER TO MARCH AND LENGTH OF CLOSED SEASON, DAVENPORT, IOWA

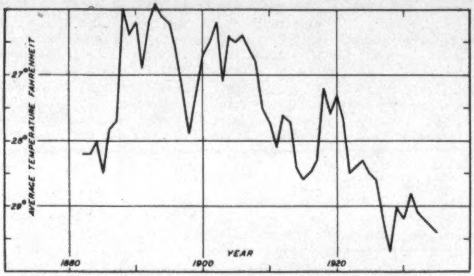

FIG. 3—10-YEAR AVERAGE TEMPERATURES DECEMBER TO MARCH, DAVENPORT, IOWA (NOTE: THE FALLING CURVE REPRESENTS RISING TEMPERATURE)

about 1865 to 1900 than now. River improvement may play a small rôle in the shorter closed seasons, but the curve seems to bear out the idea that our fathers had colder seasons than we have now.

We have prepared another curve (Fig. 2) plotting the average temperature, December to March, against the length of the closed season. We have placed a heavy line on the chart starting at 39°7, the temperature at which the river opens on the average, then running through 27°5--the normal temperature for the four months--and extending down towards 13°1--the average temperature at which the river closes. The dots cluster rather closely to this line showing that temperature is the main factor in the length of the closed season. Other small factors may have small influence as river-stage and fluctuations.

Another prepared curve (Fig. 3) shows the ten-year average temperature for the season, December to March, plotted against the individual years. Thus the dot on the curve for 1882 represents the ten-year average for the period 1873-82. Comparing this with the curve for ten-year average length of closed season we find considerable agreement in them. It also bears out the idea that winters have been growing warmer since 1890.

No records for ice-thickness in actual measurements are available for the early years. About 1853 a note was made regarding to taking of a railroad locomotive across the river on the ice before completion of a bridge. For a great many years or until about 1892, no actual ice-measurements were made. The average thickness of ice by weeks as actually measured Monday afternoon by the Weather Bureau during the winter beginning in 1892 is as follows. There was very little ice before December and after March. Beginning with the first week in December and ending with the last in March we have the following averages in inches: December--0.4, 0.7, 2.3, 3.5; January--5.5, 7.3, 7.4, 8.3; February--9.5, 9.7, 8.4, 8.1; March--5.4, 2.4, 0.9, and 0.2, respectively. The greatest thickness on the average was 26.5 inches in February 1904. Greater thickness is expected in sloughs and pools, as actual measurements show. The above averages are for channel-conditions generally.

A tabulation of the ice-gorges shows that most gorges of importance occurred before 1890. The greatest gorge in the history of Davenport was that of March 10, 1868, when the highest river-stage of record, 20.8 feet occurred there. At least three of the eleven instances on which flood-stage, or higher. was recorded at Davenport since 1860, occurred in connection with ice-gorges. It seems that ice-gorges were more frequent in the early part of our record due to colder weather, less river-improvement, and heavier ice-forming in sloughs and pools.

Table 1--Closing and opening of navigation at Davenport, Iowa

Av. temp. on closing date	Closing date	Season	Opening date	Av. temp. on opening date	Length of closed season
°F				°F	days
	December 27	1841-1842	March 1		65
	November 7	1842-1843	April 10		155
	January 27	1843-1844	February 27		32
	February 4	1844-1845	February 19		16
	November 30	1845-1846	January 24		56
	January 8	1846-1847	March 15		67
	January 18	1847-1848	February 17		31
	November 17	1848-1849	March 17		121
	December 1	1849-1850	March 1		91
	December 2	1850-1851	February 26		87
	December 16	1851-1852	March 4		81
	December 17	1852-1853	March 4		79
	December 23	1853-1854	March 2		70
	January 21	1854-1855	March 8		47
	December 24	1855-1856	March 29		97
	December 6	1856-1857	February 27		84
	Open	1857-1858	Open		0
	January 22	1858-1859	February 21		31
	December 20	1859-1860	March 3		75
	December 22	1860-1861	March 2		71
	December 28	1861-1862	March 28		91
	Open	1862-1863	Open		0
	December 18	1863-1864	March 2		76
	December 7	1864-1865	March 6		90
	December 12	1865-1866	March 6		85
	December 27	1866-1867	April 5		100
	December 8	1867-1868	March 12		96
	December 9	1868-1869	February 13		67
	January 18	1869-1870	March 24		66
	December 21	1870-1871	February 25		67
	December 5	1871-1872	March 28		115
	November 29	1872-1873	March 14		106
	December 22	1873-1874	March 10	32	79
-3	January 4	1874-1875	March 30	62	86
22	December 19	1875-1876	February 10	50	54
4	December 8	1876-1877	February 21	44	76
	Open	1877-1878	Open		0
7	December 19	1878-1879	March 9	59	81
21	December 19	1879-1880	January 7	42	20
18	November 25	1880-1881	April 1	22	128
	Open	1881-1882	Open		0
28	December 11	1882-1883	March 17	40	97
6	December 19	1883-1884	March 23	43	96
-4	December 18	1884-1885	March 30	42	103
18	December 8	1885-1886	March 18	48	101
0	December 2	1886-1887	March 9	44	98
7	December 21	1887-1888	March 19	46	90
15	January 10	1888-1889	March 12	44	62
3	January 16	1889-1890	March 14	20	58
6	February 4	1890-1891	March 10	32	35
4	November 29	1891-1892	February 14	26	78
6	December 22	1892-1893	March 13	44	82
10	December 2	1893-1894	March 4	56	93
8	December 28	1894-1895	March 1	30	64
24	December 7	1895-1896	February 25	36	81
-3	January 27	1896-1897	March 11	38	44
12	December 21	1897-1898	March 10	40	80
8	December 8	1898-1899	March 13	27	96
14	December 27	1899-1900	March 22	48	86

Table 1--Closing and opening of navigation at Davenport, Iowa--Concluded

Av. temp. on closing date	Closing date	Season	Opening date	Av. temp. on opening date	Length of closed season
°F				°F	days
6	January 30	1900-1901	March 16	36	46
-4	December 15	1901-1902	March 5	36	81
14	January 8	1902-1903	March 7	45	59
18	December 16	1903-1904	March 22	47	98
16	January 7	1904-1905	March 18	57	71
	Open	1905-1906	Open		0
	Open	1906-1907	Open		0
16	January 30	1907-1908	February 13	36	15
4	January 11	1908-1909	February 20	36	41
8	December 20	1909-1910	March 12	42	83
10	December 12	1910-1911	February 15	38	66
0	January 4	1911-1912	March 28	36	85
2	February 1	1912-1913	March 11	39	39
10	February 10	1913-1914	March 15	52	34
-2	December 16	1914-1915	February 15	29	62
22	December 29	1915-1916	March 12	50	75
16	December 17	1916-1917	March 13	35	87
-4	December 9	1917-1918	March 2	42	84
-4	January 2	1918-1919	January 21	44	20
-2	December 10	1919-1920	March 13	31	95
5	December 27	1920-1921	January 21	50	26
14	January 1	1921-1922	February 24	20	55
10	December 15	1922-1923	March 3	47	79
4	January 3	1923-1924	March 4	42	62
0	December 20	1924-1925	February 25	29	68
22	December 24	1925-1926	February 16	32	55
16	December 16	1926-1927	February 6	38	53
10	December 17	1927-1928	February 8	34	54
-4	January 7	1928-1929	March 13	58	66
25	December 4	1929-1930	February 21	58	80
17	January 14	1930-1931	February 18	37	36
	Open	1931-1932	Open		0
3	December 12	1932-1933	February 25	39	76
14	December 27	1933-1934	March 13	44	77
18	December 8	1934-1935	March 11	34	94

Average closing date December 24; average opening date February 28; average length of closed season 67; earliest closing date November 7, 1842; latest opening date April 10, 1843; longest closed season 155 days, 1842-1843.

U. S. Weather Bureau,
Davenport, Iowa

SOUTH PACIFIC REGIONAL MEETING, DAVIS, CALIFORNIA, JANUARY 7 AND 8, 1938

Morning Session, January 7, 1938

The meeting was opened by Knowles A. Ryerson, Assistant Dean, College of Agriculture. Dean Ryerson welcomed the meeting to Davis and introduced Edward Hyatt, State Engineer of California, who conducted the morning meeting. Mr. Hyatt called on Professor M. P. O'Brien, Chairman of the Regional Committee of the Section of Hydrology of the American Geophysical Union.

INTRODUCTORY REMARKS

M. P. O'Brien

This is the third meeting of the Section of Hydrology in California. Perhaps a brief explanation would be in order for those who have not attended previous meetings. It is organized under the National Research Council and is made up of committees or sections, including the sections dealing with various branches of geophysics. The Section of Hydrology has an annual meeting in Washington sometime each April in connection with the regular annual meetings of the American Geophysical Union and has recently instituted a program of regional meetings for the benefit of hydrologists. Previous meetings were held at Berkeley and Pasadena. These meetings have been much more successful than was expected both for the speakers and the attendance. They have attracted considerable interest in California where we have a relatively great concentration of people interested in the subject of hydrology. In arranging the meetings we have tried to give expression to the various interests in hydrology and have solicited contributions from all branches of hydrology. In arranging the program we have tried to get away from the purely technical type of paper and to insert subjects in the program that were somewhat controversial. The somewhat large attendance at this meeting is proof of the interest in the subject-matter for this conference. We do thank the local group for this meeting on behalf of the Regional Committee.

I have a letter here from Chairman L. K. Sherman expressing his regret that he cannot be present. He expresses his deep interest in the program and sends his best wishes for a successful meeting.

University of California,
Berkeley, California

A NEW INTERPRETATION OF THE HYDROLOGIC CYCLE

C. W. Thornthwaite and Benjamin Holzman

(Abstract)

Records of runoff from lands to oceans and of precipitation on lands are considered to be sufficiently reliable to indicate their relative proportions. It is estimated that for the United States less than 30 per cent of the moisture which falls as precipitation runs off the land and reaches the ocean. It may be argued that the continental moisture is transferred to the ocean only as runoff, and that since the ocean-levels are constant, less than 30 per cent of the precipitation has a marine origin and the remainder represents reprecipitation from continental evaporation. It may be argued equally well that all of the precipitation is of marine origin, and that, since less than 30 per cent runs off in streams and as underground flow, more than 70 per cent must return to the ocean as water-vapor. Thus, the disparity between the amount of water which falls upon a continent as rain and that which runs off from it to the ocean is in no sense an indication of the source of the precipitated moisture.

The consideration of the hydrologic cycle [see 1, 2, and 3 of "References" at end of paper] from the standpoint of air-masses reveals an important omission from all existing diagrams illustrating its operation. These diagrams invariably indicate that moisture evaporates from the oceans and is transported by the wind to the continents where it is precipitated. From there the course of the water is traced. Some is re-evaporated into the air; some returns to the oceans by way of rivers and underground water-flow; but nowhere is there recognition of the fact that any portion of the moisture evaporated from the land is transported by the wind back to the oceans.

Moisture evaporated from the land may be reprecipitated over land; but since this source does not compare with the oceanic source, it is comparatively unimportant. Likewise, the maritime air-masses do absorb some of the moisture evaporated from the land, but the fact that the moisture-content of maritime air-masses ultimately diminishes as they proceed across the land and pass off the continent indicates that they are not receiving any net gain of moisture from the land.

The consideration of the bearing of air-masses on the hydrologic cycle suggests the need of further fundamental research, but the preliminary findings indicate that the principal source of moisture for precipitation on the land is the ocean, and that continental evaporation as a source of local precipitation is relatively insignificant. Although approximately 75 per cent of the total precipitation over continents is returned to the atmosphere by evaporation in accordance with the hydrologic cycle, only 25 per cent is effective for reprecipitation, the remaining portion being robbed by dry air-bodies that pass off land-areas with a great increase in moisture.

The play of air-masses over continental areas are, in effect, great invisible rivers transporting water from the oceans to the continents and from the land to the sea at rates vastly greater than those of surface-drainage. Through this cycle of air-masses a novel method of flood-control is emphasized. It would seem that any method of soil-conservation or of forestry which would retard the runoff in the headwaters-area of a drainage-basin would increase the evaporation opportunity of the water. In this way more of the water would be diverted from the surface-streams into these great invisible atmospheric streams. Any technique which would stimulate additional evaporation of excessive rainfall from the land into these atmospheric streams, there to be conducted harmlessly back to the seas, would serve as an important and supplemental flood-protection measure.

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[1] C. W. Thornthwaite, The hydrologic cycle re-examined, Soil Conser., v. 3, No. 4, pp. 85-91, 1937.

[2] Benjamin Holzman, Sources of moisture for precipitation in the United States, U. S. Dept. Agric. Tech. Bull. 286, 41 pp., 1937.

[3] Benjamin Holzman, Use of aerological soundings in determining the sources of moisture for precipitation, Trans. Amer. Geophys. Union, 18th annual meeting, pp. 488-489, 1937.

U. S. Department of Agriculture,
Soil Conservation Service,
Washington, D. C.

DISCUSSION

R. E. HORTON (Consulting Hydraulic Engineer, Voorheesville, New York)--Dr. Thornthwaite is not present and I do not wish to take advantage of his absence. However, I discussed the subject-matter of this paper with him in Washington recently and there are some things which I should like to say. As I understand it, the paper infers that there can be no very large contribution of moisture to the air from the land; that most of the rain is derived from moisture from the ocean; and if the converse were true, droughts would perpetuate themselves.

A paper by, I believe, Dr. Turner, published in the Journal of the Royal Meteorological Society a few years ago, showed by a statistical analysis of droughts in England that droughts do tend strongly to perpetuate themselves. For example, if at a given location there has been a two weeks' drought, then the probability of two weeks' additional drought is greater than was the probability of two weeks' drought in the first instance. At the end of four weeks' drought there is a greater probability of two weeks' more drought than there was either at the beginning of the period or at the end of two weeks. This condition does not run on indefinitely, however, because of the marked seasonal variation of rainfall.

As to interchange of vapor between the continents and oceans, I think it has long been recognized that vapor-flow takes place in both directions; but what is commonly thought of is the net resultant transfer of vapor from the ocean to the land, and this, it seems to me, is necessarily equal to the runoff from the lands to the oceans, as long as the sea-level remains unchanged. It seems to me that the rainfall at a given location or at a given distance inland is the sum of the rainfall derived from the net transfer of vapor from the oceans to the land and the rain falling at that particular location which is produced by vapor picked up by air-currents en route from the ocean to the given location. This is more or less complicated, of course, by local storms and storms which come from various directions. Following out this idea, I plotted a rain-profile along the line of the Hudson and Champlain valleys, which comprise a rift-valley extending from New York Harbor to the St. Lawrence River and which, in accordance with Wegener's theory, indicates that New England is being gradually separated from the rest of America. Hudson River is at tide-water, a distance of 140 miles to Albany, then rises gradually and there is a divide at about 120 feet elevation between the Hudson and Champlain valleys. Lake Champlain is 90 feet above tide and its outlet, Richelieu River, descends gradually from the International Boundary-line at the foot of Lake Champlain to its outlet at tide-water level a short distance downstream from Montreal. The rain-profile along this line may be represented quite accurately by an expression of the form $P = a + be^{-kx}$, where x is distance, in miles, from New York, e is the base of the Naperian logarithms, and a and b are constants. There is a little hump in this profile at the divide between the Hudson and Champlain valleys. This equation indicates that the constant a represents the part of the rain which is derived from local precipitation. The second term of the equation is an exhaustion-function and represents the part of the rain which is derived from oceanic vapor. I have found that a similar equation will quite accurately represent the rainfall proceeding from the mountain marginal rim toward the desert interior of Australia, and, to a somewhat less degree of accuracy, the rain-profile proceeding up the Mississippi River from New Orleans. The constants a and b are not the same in different profiles. I do not recall the precise figures but I should say offhand that one-third or one-half of the rain in the Hudson Valley is derived from local evaporation. This method of analysis can only be applied to rain-profiles which are not too greatly affected locally by topographic conditions but where it can be applied, it affords a ready means of separating the rainfall into the components derived, respectively, from the ocean and from antecedent evaporation en route over the land.

The paper brings out a good point in calling attention to the fact that evaporation occurs mostly during the eastward passage of a high-pressure area or air-masses, which are relatively dry; whereas the rainfall is derived mostly from moist air-masses from tropical regions or from over the oceans. I do not think the correct implications have been drawn from these facts. If through an increase of evaporation more vapor was carried out to sea in the dry air-masses, this excess of vapor would necessarily either be precipitated over the ocean or added to the general vapor-supply; and hence some of it would be brought back and precipitated over the land. One of two things must follow: Either the oceanic levels would rise, and equilibrium no longer continue, or else additional vapor would be brought from the oceans over the land and precipitated, making the net resultant of transfer in both directions from ocean to land precisely equal to the runoff from the land to the ocean, as before. Probably both factors would enter in to some extent, for a period of time at least, until a new equilibrium was established.

J. E. CHURCH (University of Nevada, Reno, Nevada)--The failure of the potential water-supply to produce precipitation in its lee is convincingly shown by the variation in precipitation along the shore of the Pacific Ocean belonging to the Western States. During the period from 1874 to 1934, the variation in summer precipitation at Los Angeles, San Francisco, and Portland has varied for the entire period of 60 years from 10 to 480 per cent of normal for the entire group of stations, and the maximum variation between the three stations in the same season has been 440 per cent of normal.

This study was undertaken to determine the probability of a normal snow-cover creating a normal summer precipitation in its lee, and is the extension of investigations begun by Professor O. W. Monson, of the Montana State Agricultural College. It would seem that a boundless water-supply, such as provided by the Pacific Ocean, would provide greater uniformity in precipitation along the Pacific Coast, if the water-supply were the major factor in precipitation.

Regarding the control of floods, suggested by Mr. Holzman, through the control of evaporation, the great disparity between the maximum rate of evaporation under normal conditions and of torrential or continuous rainfall would apparently prevent the very control desired, even though the evaporation-control should be continued over a long period in anticipation of the possible flood. Certainly the major downpours, because of their intensity, would swamp the accumulated effort associated with evaporation-measures.

B. HOLZMAN (U. S. Soil Conservation Service, Washington, D. C.)--In regard to the idea of the perpetuation of droughts I should like to make clear that droughts as far as we know do perpetuate themselves, but this has nothing to do with the available moisture on land. You may not all know it, but the moisture-content in the atmosphere of Death Valley greatly exceeds that over areas having ample rainfall. The problem comes down to the method of release of the annual rainfall. That is the thing that is lacking in regions of low rainfall. Meteorological forces precipitate moisture. It can be shown that during some of our droughts, as in the Great Plains drought, the total amount of moisture in the atmosphere during the drought actually exceeded that of times when abundant moisture was precipitated there. Continental invasions did not take place. You can see that in summer the total amount of moisture is greatly increased over that of winter. Yet on the Pacific Coast we get much rainfall in winter and little in summer. In the atmosphere we have ample amounts of moisture for precipitation, and the thing which fails is a suitable meteorological condition to precipitate the moisture.

J. E. CHURCH--We made investigations of the precipitation at Los Angeles, San Francisco, and Portland to see what effect abundance of moisture windward of these places may have. We found the usual disagreement, sometimes amounting to 100 per cent between precipitation and atmospheric humidity. Therefore there must be something else in the atmosphere. In regard to controlling floods by some system of watersheds, it seems to me that evaporation is so slow and excessively slow during a rainfall-period that it would overwhelm any possible evaporation you might get. That is in the realm of theory rather than in the realm of fact. It is interesting to note that evaporation is pretty slow as compared with precipitation.

J. E. CHRISTIANSEN (University of California, Davis, California)--Where is the source of moisture for the western portion of the United States?

B. HOLZMAN--I am sorry I did not bring that up. The moisture for California and the entire Great Basin Area and the Rocky Mountains almost invariably comes from the polar Pacific air-masses, evaporated from the Pacific Ocean, traveling south and entering the continent from the north and northwest, occasionally from the southwest.

E. HYATT (State Engineer, Sacramento, California)--Your statement is that evaporation from land, forests, and other vegetative cover is a negligible factor in producing rainfall and that there is an abundance of moisture for precipitation available in the upper air if only the meteorological conditions were favorable.

CHARLES F. BROOKS (Blue Hill Meteorological Observatory, Milton, Massachusetts, comments in a letter to J. E. Church)--I fully agree with Holzman in the main that the major source of precipitation is direct vapor from the oceans. I think he is going too far, however, in excluding so drastically the secondary source for interior locations. In the Rocky Mountain area these secondary re-evaporations after a primary big wetting from an ocean may keep an enclosed valley-area showery for some time afterwards. Every day the up-valley winds will produce showers, then the water will flow down the valley and wet surfaces will supply vapor necessary for the next day's up-drafts.

HYDROMETEOROLOGY--A COORDINATION OF METEOROLOGY AND HYDROLOGY

Merrill Bernard

The respective rôles of the meteorologist and the hydrologist have, until now, been fairly free of confusing overlaps. The meteorologist has dealt with the atmosphere, including water in the form of vapor which he has handed to the hydrologist as it condensed and fell as rain. The hydrologist, in turn, has undertaken to follow the now widely publicized and graphically ornate "hydrologic cycle" through the return of runoff-waters to the sea and of evaporated moisture to the atmosphere.

The hydrologist has not been interested particularly in the causes of precipitation. Rather has it been important to him to know (1) its variations in terms of rate, duration, and areal extent, (2) its variation with changes in elevation and locality, (3) seasonal differences to be expected, (4) the periodicity with which like phenomena could be expected to occur, and finally (5) the relation of precipitation to the resultant phenomena of infiltration, transpiration, evaporation, and runoff.

Hydrology, the science embracing the "Waters of the Earth", is relatively new. In this country its beginnings are associated with the work of Mead, Horton, Meyers, Shermān, and others, who organized it and gave it an identity sufficiently concrete to permit of a formal systematic presentiation. As an embryonic science it has passed through the usual generalized and empirical phases and now holds promise of emerging some day as a fairly exact science. Perhaps there is no better index to its rate of growth than the activities of the Section of Hydrology of the American Geophysical Union. Organized in May, 1930 by five or six of its pioneers, the work of this Section has during the intervening years gained a prominent place in the broader field of geophysical research.

The meteorologist, on the other hand, has a background beginning with the ancient interest in weather-signs. The so-called "instrumental" period, culminating in recent important developments, began with the early years of the nineteenth century when twelve meteorological stations in Europe and five on this Continent began records of rainfall and temperature which are today considered dependable.

From weather-signs, through empirical rules and dependence on surface-observations alone, meteorology is now entering what may be considered a three-dimensional stage of development. For a long time it has been known that the answers to many problems relating to weather lie in atmospheric strata considerably higher than any directly observable from the ground. The unexplored regions above the clouds have intrigued Man always. A young American doctor visiting in Europe in 1784 purchased a seat in a balloon, and occupied himself throughout the ascent by taking air-samples and making meteorological observations; but real progress of upper-air exploration languished until after the World War.

The steps in the development of upper-air sounding are of more than passing interest, for they reflect the major part of the progress made in meteorological science during recent years. Because of the hazard and expense of manned balloon-ascents, small "sounding"-balloons were utilized at first to send aloft instruments which were precariously returned to the ground by parachutes. Later, huge kites provided much valuable information, but their use also was attended by serious limitations. The World War, however, demonstrated the practicability of the airplane in taking observations at high altitudes, so that now there are nearly 30 aerographic sounding stations throughout the United States at which daily observations are made to a height of 16,000 feet. Again, hazard to life and the difficulty, often impossibility, of flight during stormy weather stimulated development so that we now have the radiometeorograph, a small instrument weighing only a few ounces, which, upon being sent aloft by a small hydrogen balloon, transmits to the Earth by radio a record of barometric pressure, temperature, and humidity in the upper air.

Sciences so closely allied as meteorology and hydrology can not long remain sharply separated. The continual search for supporting facts will carry one investigator into the field of the other. The objectives of both are provisional and constitute an unrelenting search for physical and statistical relationships, and closer correlations of the factors which are of importance in improving the means of forecasting meteorologic and hydrologic events such as storms and floods.

The hydrologist, therefore, has every reason to be interested in meteorology, and he should project his knowledge beyond that phase of the cycle embracing precipitation. Also, in the light of a woefully inadequate record of hydrologic data, both as to record length and geographical extent, he could improve materially his statistical analyses by including in his correlations a consideration of the principal causal factors that are now the everyday tools of the air-mass analyst.

Furthermore, we find the meteorologist suggesting that the prevailing orthodox conception of the hydrologic cycle is illogical and unsupported by fact. You will recognize the conventional picture of the hydrologic cycle shown in Figure 1, together with a diagrammatic attempt to approximate quantitatively the relationships. It is reasoned that because about one-fourth of the continental precipitation is returned to the sea as runoff, and insofar as there is no perceptible change in ocean-levels, therefore only one-fourth of the atmospheric moisture appearing over land is evaporated from the oceans, the remaining three-fourths being of necessity comprised of moisture evaporated from land-surface and inland waters.

Recent articles by Dr. C. W. Thornthwaite and Benjamin Holzman [see 1 of "References" at end of paper] have injected into the hydrologic cycle a more pleasing style of art, and have presented a conception of the cycle which the authors support with modern meteorological analysis and a reasonable array of factual data. Their conception of the hydrologic cycle is shown in Figure 2 to which I have added a graphic explanation. Mr. Holzman has summarized a portion of his study in the Transactions of the American Geophysical Union [2] as follows:

"Polar air-masses advancing equatorward absorb moisture by evaporation, become heated, and are eventually transformed into warm, moist, tropical air-masses. Tropical air-masses on the other hand, traveling poleward, precipitate most of their moisture, become cooled, and are ultimately transformed into cold, dry, polar air-masses. This air-mass cycle which is manifestly an expression of the general circulation of the atmosphere is closely associated with the balance

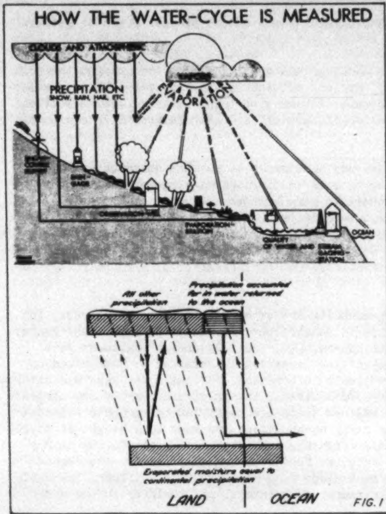
HOW THE WATER-CYCLE IS MEASURED
CLOUDS AND ATMOSPHERIC
VAPOR
PRECIPITATION
SNOW, RAIN, HAIL, ETC.
EVAPORATION
EVAPORATION-STATION
OCEAN
QUALITY OF WATER AND STREAM-GAGING-STATION
All other precipitation
Precipitation accounted for in water returned to the ocean
Evaporated moisture equal to continental precipitation
LAND
OCEAN

FIG. 1

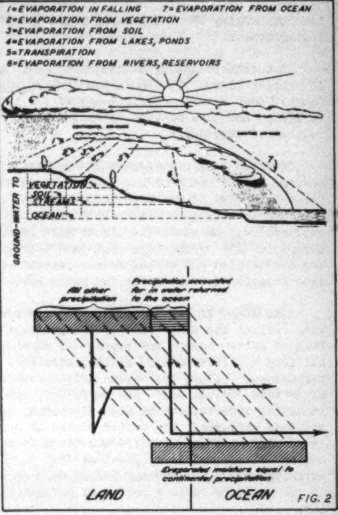
1=EVAPORATION IN FALLING
2=EVAPORATION FROM VEGETATION
3=EVAPORATION FROM SOIL
4=EVAPORATION FROM LAKES, PONDS
5=TRANSPIRATION
6=EVAPORATION FROM RIVERS, RESERVOIRS
7=EVAPORATION FROM OCEAN
GROUND-WATER TO
VEGETATION
SOIL
STREAMS
OCEAN
All other precipitation
Precipitation accounted for in water returned to the ocean
Evaporated moisture equal to continental precipitation
LAND
OCEAN

FIG. 2

of water over continental areas or, as it is better known, the hydrologic cycle. In the cycle of air-mass transformation it is apparent that moist, tropical maritime air-bodies must rid themselves of their high water-vapor content before they can ultimately be modified into polar air-masses, and similarly polar air-masses must gain in moisture before they can properly be transformed into tropical maritime air-masses. Thus, in the alternating inundations of extensive bodies of air of different origin can be distinguished two principal classes, those that are losing their moisture on the Earth and those that are absorbing moisture and constitute the greatest robbers of moisture from continental areas."

The point is that vast quantities of water evaporate into polar continental air-masses, which are never given the opportunity to precipitate their accumulated moisture in passing across the Continent. They therefore carry out to sea the greater portion of moisture evaporated from the land.

Had Mr. Holzman continued he would have explained, no doubt, that he has pictured conditions prevailing mainly during the winter, but that his general conception of the hydrologic cycle applies as well to the summer conditions in this country and to all tropical regions.

During the summer, polar continental air-masses, because of the absence of a sufficient driving force, seldom penetrate beyond the Northern States. The picture, therefore, changes somewhat, as shown in Figure 3. Solar radiation raises temperatures at the ground, thus increasing the vapor-capacity and favoring shower-type precipitation. Such rainfall is usually quite localized. At the same time, factors conducive to high rates of evaporation are active.

The opportunity for the occurrence of summer precipitation, of either great areal extent or long duration, is much less than that for evaporation, which is continuous and over much larger areas including every source of moisture to be found on land. It is believed that there is in summer as great a tendency for evaporation into the tropical maritime air-masses after they have reached land-areas as there is in the polar continental air-masses. While tropical air-masses are initially highly charged with water-vapor, the evaporation and transportation of additional vapor is made possible by sharp rises in temperature after the air has left ocean-areas. In other words, as a vehicle for the transportation of vapor we have replaced the cold, dry, air-masses of polar continental origin, with tropical air-masses, which likewise, because of greater

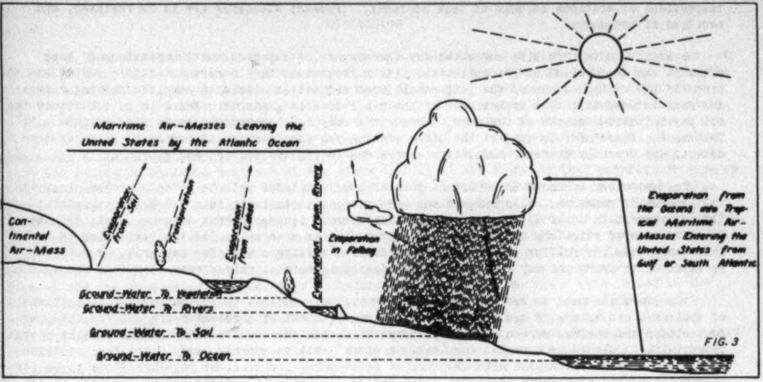
Maritime Air-Masses Leaving the United States by the Atlantic Ocean
Continental Air-Mass
Evaporation from Soil
Transpiration
Evaporation from Lakes
Evaporation from Rivers
Evaporation in Falling
Evaporation from the Oceans into Tropical Maritime Air-Masses Entering the United States from Gulf or South Atlantic
Ground-Water To Vegetation
Ground-Water To Rivers
Ground-Water To Soil
Ground-Water To Ocean

FIG. 3

thermal energy, receive moisture from the land in quantities far beyond the amount returned immediately as summer rainfall; the excess moisture in the form of invisible vapor, moves out to sea, just as does that in the polar continental masses during the winter.

The need for meteorological support in problems of applied hydrology has become obvious in planning the expansion of the river-and-flood forecasting service of the Weather Bureau. The ultimate measure of success of this service, particularly in headwater-areas and on small basins, lies in extending the time-period covered by the forecasts. Dependence on river hydraulics alone delays the initiation of an estimate until the flood-wave has developed in the channel-system, and then only a few hours are available for the evacuation of areas doomed to inundation. Fortunately, newly developed hydrologic methods are found effective in extending the forecasting interval to the time of beginning of flood-producing rainfall, and it is believed that, by making use of the more practical phases of air-mass analysis, the forecasting period on the smaller river-basins may be still further increased and made at least twice that resulting from an analysis of stream-stages only.

We have discussed the contributions that meteorology might make to hydrology. The younger branch of the science, hydrology, makes its principal contribution in sponsoring an expansion of our hydrologic data-base comprised of Nationwide networks of precipitation-stations and stream-flow recording stations. Plans are being prepared under which the present systems will be augmented by approximately 2400 automatic rainfall-stations, 1000 snowfall-stations, and 1100 stream-flow stations.

Much of the structure of the weather-sciences has been built on the foundation of data provided by climatology and hydrology. We are particularly indebted to the climatological service of the Weather Bureau for our Nationwide record of precipitation, which has been accumulated over the past 40 years through the cooperation of nearly 7000 unpaid observers--an accomplishment few understand or appreciate.

More recently developed methods of estimating stream-flow from rainfall have attracted attention to the necessity for continuous record of precipitation throughout rainfall-durations shorter than 24 hours. Climatological investigations of storm-morphology, likewise, require a more direct relation between duration and rate of rainfall than is obtainable from the published records of daily precipitation. Accordingly, research-projects established recently in Pennsylvania and Ohio have set up networks of closely spaced recording rain-gages having density of from 16 to 400 square miles per gage.

In the program of hydrologic research now being organized and reported upon by a special committee of the National Resources Board, recognition is given first to the need for a study of rain-gage location and exposure. One criticism of the present system of gathering precipitation-data through unpaid observers lies in the lack of authoritative supervision of the central agency over observational work, proper location and exposure of gages, and wholly inadequate inspections of stations because of lack of funds. Present standards are to be reviewed, and modified if necessary.

Special studies are also suggested for the purpose of improved instrumentation and technique in the measurement of precipitation. It is recognized that a more scientific justification for the continued use of the long-established eight-inch circle in sampling falling rain and snow is essential to a broader use of the basic data so gathered. There is no substitute for the precipitation-records of the past, despite the very real criticisms to be made of them. It is planned, therefore, to subject the older station-records to critical review in an attempt to detect, and possibly correct, cumulative errors due to faulty location and exposure.

The hydrology of snow, an important form of precipitation, will be given a proper place in the program of research. This subject has been divided tentatively into the following parts: (1) Improvement in snowfall-measurement; (2) improvement in measurement of snow-depth; (3) the determination of relations between the factors causing snow to melt; (4) the evaluation of surface-conditions controlling the losses to runoff from melting snow. The research, as outlined, is expected to point the way to methods of estimating quantitatively stream-flow from snowfall.

The immediate need to resort to hydrometeorological analysis in estimating the capacities of spillways and waterways has resulted in the establishment of a research-project for this purpose within the Weather Bureau. It is proposed to divide the country into regions of like storm-potentiality. Within each region the "maximum storm" will be superimposed in critical position upon the basins of the region with the view of determining the greatest possible flood to be accommodated by the particular structure under design. Hydrologic regions can be expected to embrace a considerable area in the Great Plains, where physiographic influences are minimum and where storm-magnitudes and durations are fixed by distance from, and position relative to, moist air-mass sources. The areas embraced by the regions of like storm-magnitude will diminish rapidly as the mountainous sections of the West and of the East are approached, being confined in many instances to single basins or to one slope of a mountain range.

In attacking this problem, the modern meteorological method of approach provided by air-mass analysis, which considers the physical properties and mutual interactions of the major air-currents as they are related to storms, is to be utilized. This is being accomplished by a detailed investigation of the fronts and air-masses in the great storms that have occurred in the past throughout the country.

Previous reference to the hydrologic cycle did not adequately emphasize the importance of evaporation. Dr. Thornthwaite and Mr. Holzman have suggested that, by prolonging the evaporation opportunity through the storage and retardation of surface-runoff, a part of excessive rainfall might be returned to the atmosphere by evaporation and routed to the sea through upper air-mass channels.

While the idea may now lie largely in the realm of fancy it does carry the conviction that we have an almost unexplored field before us--hydrometeorology.

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[1] Benjamin Holzman, Sources of moisture for precipitation in the United States, Tech. Bull. No. 589, Dept. Agric; also
C. W. Thornthwaite, The hydrologic cycle re-examined, Soil Conser., October 1937.
[2] Benjamin Holzman, Use of aerological soundings in determining the source of moisture for precipitation, Trans. Amer. Geophys. Union, 18th annual meeting, pp. 488-490, 1937.

U. S. Weather Bureau,
Washington, D. C.

DISCUSSION

E. H. FLETCHER (U. S. Weather Bureau, Sacramento, California)--With reference to the subject of hydrometeorology, or meteorology in connection with hydrology, which Mr. Bernard has just discussed, a comment in regard to the California storm of December 1937 may be of interest in connection with a type of flood-situation in which flood-producing rains must be anticipated considerably in advance of their occurrence. On December 9, a large area of low pressure of great magnitude and very great intensity was located about 700 miles off the California Coast. From that position, with very warm moist air being carried over northern California at gale-velocity, and with the indicated eastward movement of the storm itself, it was apparent that rainfall of unusual intensity would undoubtedly occur within 24 to 36 hours, with the consequence of extremely high runoff from the steep slopes and flashy streams of the mountain area. On December 10 the storm moved east-northeastward, having by this time developed almost to the point of hurricane-intensity. The pressure at its center was 28.10 inches and wind-velocities over California were increasing rapidly. Rain had already fallen in heavy amounts over the northern portion of the State, especially in the Sacramento Valley, and was beginning to fall in very heavy amounts over the Feather and the Yuba areas. On December 9, before the rain began, warnings were issued to the effect that a flood of probably serious proportions would develop. Being so near the sources of moisture, and having the problem of almost immediate runoff to deal with, we have a situation here requiring predictions of the very type mentioned by Mr. Bernard--that is, warnings in advance of the occurrence of flood-producing rains. At critical times a constant watch must be kept on developing conditions and our forecasts revised as may be necessary to meet them.

C. J. KRAEBEL (U.S. Forest Service, Berkeley, California)--I want to call attention to an intensive study of the variations in the recording of rainfall in the San Dimas Experimental Forest. There we found some peculiar inversions of quantity of rainfall with change in altitude. Sometimes topography was more important than altitude in affecting catch. We are interested in that because we must have confidence in our basic data in order to have confidence in runoff-figures. Until we have an accurate record of rain-catch, we cannot express any relationships that mean anything. We want ratios between runoff and rainfall. We shall certainly be happy if the Weather Bureau enters this field of research in methods of registering rainfall.

C. L. KAUPKE (Kings River Water Association, Fresno, California)--One phase has not been touched upon. I know from our experience on the Kings River that the temperature of rainfall and consequently the elevation at which rainfall occurs is quite as important as the intensity itself. The unprecedented runoff of Kings River occurred very largely because the rainfall fell at an elevation of nearly 10,000 feet, whereas usually it occurs to an elevation of 3500 feet. This is what made the flood. I would like to have Mr. Fletcher discuss that. Is there any way of forecasting at what elevation we may expect precipitation to occur in the form of rain?

E. H. FLETCHER--I think this question can be answered best from a consideration of the position, intensity, and development of the December storm. As mentioned a few minutes ago, this storm was of such magnitude and intensity that the air-masses, moving in from the sea over northern California, were from farther south than is usual. In other words, these Pacific air-masses, moving from the southwest, bore many of the characteristics of tropical air-masses, principally in the matter of high moisture-content and high temperature; and their movement over the high California mountain ranges resulted in precipitation which probably broke all existing records for intensity. Within 48 hours, in fact, the amount of rain was as great as that produced by previous serious storms of as much as seven or eight days' duration. The combination of the effective factors of unusual initial and increasing storm-energy and the resultant great elevation of very warm and heavily moisture-laden air-masses, which in turn resulted in record-breaking rains where snow usually falls at this time of year, constitute a situation of such rapidly-changing elements that no quantitative predictions could be made very long in advance concerning any of them.

Some comparisons may be of interest. Over large areas of the Feather-Yuba Drainage-Basin, from 12 to 20 inches of rain fell in two days. In another storm (that of 1928) the period for a smaller amount of rainfall was about eight days; while in 1907 and 1909, when the fall was somewhat greater, the period covered between two and three weeks. In December 1937, on the other hand, falls of approximately 12 inches occurred within 24 hours in the altitudinal belt between 3000 and 5000 feet. The small mountain streams were of course unable to discharge such a great and sudden downpour, and overflow was inevitable. Vast streams of water rushed down mountain canyons, carrying everything before them; bridges were washed out; and swollen and overflowing local creeks menaced areas never before known to have been flooded.

From answers to questionnaires concerning the damage caused, it now appears probable that the losses will exceed $15,000,000, making this the costliest flood in the history of this Valley. The figures available show that the floods of 1907, 1909, and also 1904 reached only an aggregate of about $10,000,000.

E. HYATT--Mr. Fletcher implies that the intensity of these storms, the peaks they reach, is the important factor in designing structure. I may say that in the State of California the spillways on more than 300 dams have been enlarged within the last three years. During the recent flood fortunately only three small dams went out.

PRECIPITATION AND VEGETATION

Ralf R. Woolley and J. Cecil Alter

As time marches on, historians are usually quite faithful in recording the activities of man, and it will usually be found that Mother Nature is even more meticulous in reflecting and preserving her experiences, more especially with regard to climate and vegetation. Just how much the activities of man have done to modify the natural conditions of a virgin country may in some respects always remain a question. There is however, no longer the slightest doubt about the influence civilization may have had on the climate--which is none at all! And it is the purpose of the present paper to introduce a few trustworthy witnesses in support of the hypothesis that the activities of man have also been ineffective with respect to noteworthy changes in the general aspect of the native vegetation, outside the ranch-fences of the West.

The early explorers and pioneers in the West have left us written records of the general facts of climate and vegetation in scores of authentic books, as well as many useful statistical records. For example, precipitation-measurements were begun as early as 1849 or 1850 at a few places, including Salt Lake City, Sante Fe, and San Francisco,and a great many more in the 1850's and 1860's, most of them continuing to the present time.

There are also fairly precise observations, recorded at the time, of the water-level stages of Great Salt Lake, beginning as early as 1824, to indicate early day variations in water-supplies. All of these records and observations are supplemented, and mostly substantiated, by tree-ring growth studies.

As any two different but competent observers might make different reports on a crop of vegetation today, so might an observer's examination 50 years ago have differed in some respects from an examination made today; but they would not differ in the major facts of luxuriance or scantiness, or preponderance of varieties of vegetation, with which we are here concerned.

When the Cardenas party visited the vicinity of the Rainbow Natural Bridge, southeastern Utah, in the year 1540, after crossing an uninhabitable desert to the south, and found the country "Covered with low and stunted pines, exposed to the north," they were going through, and looking upon, precisely the same scenes a party would nowadays witness at that time of year, after the lapse of 400 years! Those exposed mesas are still so cold at night, the summer tourist is glad to have a blanket, just as was the Cardenas party four centuries ago!

When Father Escalante made his memorable trip through Utah in the summer of 1776, he wrote: "In Camp Asuncion, on the Dolores River (Colorado). . .the soil was so burned and dry that it appeared to have received no rain all summer." That describes a typical scene in an average summer today, 160 years later.

He appropriately described the land through the Uinta Basin, Utah, as "good, but somewhat arid". If he had come a little earlier in the season, he would doubtless have said it was very dry, just as he would have done in most of the 160 summers since, because those are normally the driest months in the Basin.

The Utah Valley, around Provo, Utah, Escalante says, September 25, 1776, was: "All clean land. . .good for planting. . .with the exception of the marshy places along the shores of(Utah)

Lake. . .The climate here is good, and having suffered so much from cold since leaving the (Green) River (passing over the Wasatch Mountains) we found this valley very comfortable both day and night."

Fifty thousand prosperous people in that fertile valley today, can heartily agree with Father Escalante! They can also bear witness that the climate has not permanently changed in the 88 seasons the valley has been occupied, and that the water-supply and the native vegetation (where undisturbed by cultivation) have varied through the years only with, and as a direct result of, the passing cycles of precipitation. Father Escalante's reference to the marshy shorelands around Utah Lake indicates a familiar low, or median stage of the lake-level in 1776, for the marshes are much less in evidence at the high-water stages.

When Father Escalante passed through Sevier Lake Desert, central Utah, he said: "In most parts, the grazing is very poor, and although two rivers empty into the (Sevier) Lake, we saw no good location for a settlement." This is quite as true today, as it was 160 years ago; and were it not for irrigation, by the diversion of the waters of the rivers mentioned, the towns of Delta, Hinckley, Deseret, and Oasis would not exist today.

Jedediah S. Smith crossed the same region 50 years later, and on June 24, 1827, wrote: "I started very early in hopes of soon finding water. But ascending a high point of a hill, I could disclose nothing but sandy plains, or dry, rocky hills." That is a good description of the country between Holden and Delta today, part of which has been reclaimed by irrigation and dry-farming methods, but none of which has actually been altered, by man or by nature, in its water-supply, its vegetation, or its general aspects, in these 110 intervening years, nor in the half century of actual use.

Colonel John C. Fremont looked into Bear River Valley, northern Utah, on August 24, 1843, noting in his journal: "The soil appears generally good, although with the grasses, many of the plants are dried up, probably on account of the great heat and want of rain." July and August are much the warmest months of the year; and June, July, and August are normally the driest, July being much the driest of the three. Hence, it would have been extraordinary indeed, if Fremont had reported otherwise on the vegetation!

Orson Pratt entering the Salt Lake Valley with the Mormon Pioneers July 22, 1847, wrote: "A great variety of green grass, and very luxuriant, covered the bottoms for miles, where the soil was sufficiently damp; but in other places, although the soil was good, yet the grass had nearly dried up for want of moisture." July is now, and undoubtedly always has been, the warmest and driest month of the year in the Salt Lake Valley. Consequently, only the low, poorly drained land remains moist enough to produce the grass Pratt mentioned--a fact as true today, as 90 years ago!

Captain Howard Stansbury came to survey the Great Salt Lake in 1849-50, and to locate the best route through the mountains for a road into the valley. Crossing the Wasatch Mountain Divide between the Bear and Weber rivers, northwest of Evanston, Wyoming, in August, 1849, he noted: "The timber is small and consists of oak, black-jack, aspen, wild cherry, service-berry, and box-elder of large size. In many places it is quite abundant." While that region has been utilized intensively for summer livestock grazing during the past 85 years, recent examinations indicate that the region still supports about the same varieties and quantities of trees and bushes.

Coming into "Ogden Hole" where the town of Huntsville is situated, Stansbury says: "The scene was cheering in the highest degree. The valley, rich and level, was covered with grass; springs broke out from the mountains in every direction, and the facilities for irrigation appeared to be very great." Even in dry years, a part of that valley yields perennial springs, due to the geology of the basin. Ogden City's celebrated, never-failing artesian wells gush forth in the lower edge of the valley.

But the existence of springs on the upper sides of the valley in August, 1849, indicates a previous season or two of heavy snowfall in the mountains--a condition that has often occurred since Stansbury passed over the last Great Divide! At the time of Stansbury's visit, Great Salt Lake was five feet deeper than it is today, thus indicating that the Great Salt Lake Drainage-Basin was in the rising phase of a precipitation-cycle.

Another notable field of heavy grass is reported by Stansbury in Tooele Valley, Utah, November 6, 1849, when camping: "In the prairie near a noble spring of fresh, cold water, with . . .an extensive grove of large willows as fuel. . .This valley forms an excellent pasturage for numerous herds of cattle wintered here. . .The grass is very abundant, and numerous springs are found on both sides (of the valley)."

It might be easy to believe that these reports of "abundant" grass were indicative of a condition that permanently existed before these valleys were settled by the Mormon pioneers. Also possibly, that the settlers' unwise utilization of these lands afterward, had depleted, or destroyed, the grass and other vegetation--and thus exposed the top soils to erosion as they had never been exposed before. However, Stansbury makes it clear that the grass was abundant where (and when) springs and streams were plentiful.

Captain Stansbury did not revisit these valleys again, but as an example of several such records, a Weather Bureau observer making his report for the Tooele and Skull valleys region in the wet year of 1923 says: "The range- and brush-feed are excellent around here, and livestock are in good shape for the winter." A report from the same source in 1935, in the midst of a dry cycle says: "There isn't any green feed in this locality, and the springs are low." Also, an observer in Cache Valley that year reports: "There is no third hay crop, due to the drought."

Without making further quotations, it appears obvious that the same condition of precipitation and vegetation-balance prevails in these latter days, as was found to be prevailing when the pioneers arrived in the west. Captain Stansbury's statements of 1849 concerning the luxuriant stand of vegetation therefore do not necessarily corroborate nor form a part of the theory that the depletion of vegetation during the recent severe dry cycle is solely the result of overuse of the forage.

The western mountain country generally was notably arid, and Stansbury says it supported very little forage when the Indians and wild game dominated it. "The studding of this beautiful (Salt Lake) City with noble trees will render it by contrast with the surrounding regions, a second 'Diamond of the Desert.' One of the most unpleasant characteristics of the whole country (traversed between Kansas and Utah), is the entire absence of trees," continues Stansbury. "The shady grove, the babbling brook, the dense and solemn forest are things unknown here; and should the traveler by chance light upon some solitary cottonwood, or pitch his tent amid some stunted willows, the opportunity is hailed with joy". Fremont had said a similar thing some years before.

When Clarence King made his "Geological explorations of the fortieth parallel", considerable field-study of the vegetation was made by trained botanists in 1867-69. The report of this work, published in 1871, makes the following comment concerning that part of the Great Basin included within the scope of the work: "No portion of this whole district, however desert in repute and in fact, is destitute of some amount of vegetation, even in the driest seasons, excepting only the alkali flats, which are usually of quite limited extent. . .This vegetation, covering alike the valley plains, the graded incline of the mesas, the rounded foothills, and the mountain slopes, possesses a monotonous sameness of aspect, and is characterized mainly by the absence of trees; by the want of a grassy, greensward, the wide distribution of a few low shrubs or half-shrubby plants, to the apparent exclusion of nearly all other growth."

At the time of King's visit to the Great Basin, the precipitation-cycle was at the highest stage of known record, being much higher than it has ever been since. This is shown by the precipitation-records at Salt Lake City, as well as by the Great Salt Lake water-stage, which was 17 feet higher than today. Nevertheless, with all that excess of moisture, King found evidences of summer dryness in the vegetation and of the inadequacy of precipitation far in excess of normal, to sustain a luxuriant growth of vegetation.

The reports of Fremont, Stansbury, and King indicate that the conditions observed by them were prevalent over extensive areas covering at least the Great Salt Lake Drainage-Basin. Similarly, the depletion of the range-forage during periods of drought would naturally have been equally extensive. As to the size of the area, sometimes referred to as "unlimited" and commonly classified as "overgrazed", we may say that 62 per cent of the State of Utah receives 12 inches, or less, of precipitation annually. This vast area originates no streams at any time of the year, and its vegetation normally consists of the fragile, sparsely-set desert types of plants.

During the drought-cycle culminating in 1934, the 12-inch zone of precipitation had expanded to include 74 per cent of the State's surface. Obviously, if enough cattle and sheep are run on the land to consume the forage during the wet cycles, there will not be enough forage for them during the dry cycles!

It seems evident from the citations herein given, not to mention many more that could have been given, that no extreme phase of the precipitation-cycle, whether upward and comparatively wet, or downward and relatively dry--nor the practices of man in substituting domestic livestock for wild game and diverting irrigation-waters onto two per cent of the land--have ever been sufficient to produce a material change of a permanent character in the general aspect of the native vegetation.

U. S. Geological Survey (R. R. W.),
U. S. Weather Bureau (J. C. A.),
Salt Lake City, Utah

DISCUSSION

E. I. KOTOK (U. S. Forest Service, Berkeley, California)--Is the inference to be made that in the West, as a whole, man's occupancy of land and use of the land does not modify native vegetation?

J. C. ALTER (U. S. Weather Bureau, Salt Lake City, Utah)--The native vegetation, yes, I think it is.

E. I. KOTOK--We do not have to go very far along a straight line from the redwoods down to Santa Cruz County and see where harvesting of redwood-forests has materially changed the aspect of the country. This is attested to by historians and commentators. We have only some remnants left of a vast expanse of forest. There are 400 logging operations of redwoods. In fact, Palo Alto was named after a redwood tree. We had fuel and lumber furnished from that territory. I think we can take you along the foothills of California where gold was first discovered. Marshall went there not to find gold but to start a sawmill, and in those days they did not have any skilled ways of transporting material excepting oxen and man power. The mill was located in the little town of Coloma, because there was lumber right near there at the American River, and now you would have great difficulty in finding a tree. That happened about 70 years ago. Extending north and south, there is historical evidence that the timber-line has been shoved back by man's use, by cutting, burning, or whatever he did, in the historical period of the lifetime of men still alive in California.

C. S. SCOFIELD (U. S. Bureau of Plant Industry, Washington, D.C.)--There is another aspect of this change which takes place as the result of man's occupancy to be seen in Texas, which is, of course, a long way from California. There the effect of controlling fires has been to increase very substantially the arboreal cover of south and southwestern Texas, so that conditions within historical times with respect to the vegetative cover of the Southwestern plains have changed materially in the direction of increasing the arboreal cover, with the resulting problem of preventing fires.

J. C. ALTER--In reply to the question whether the salinity of Great Salt Lake has increased: The salt content of the Lake is practically constant, the percentage of saturation consequently decreasing when the Lake rises and increasing when the water surface falls. He also said that big trees and vegetation will come back again in due time. It is all the same, whether the Indians burn it or the white man harvests it. The crop-cycle includes harvesting and planting--requiring a longer perspective for Sequoias than for Sweet Clover. The replanting of forests, natural and artificial, is already more extensive than harvesting or destruction have been.

THE PRACTICAL APPLICATION OF THE RIGOROUS EVAPORATION-EQUATIONS

N. W. Cummings

(Abstract)

At San Bernardino, California, a comparison is being made between the transpiration-loss from a plant, and the total radiant energy received by a horizontal projection of the foliage. It appears likely that the transpiration-loss can be computed with fair accuracy by the simple procedure of dividing the total radiant energy by the latent heat of water.

On the basis of data taken at Lifton, Idaho, by the Utah Power and Light Company during the summer of 1937, a comparison is being made between the evaporation from Bear Lake, as computed by the energy-equations, and the numbers obtained by applying a coefficient to the evaporation from a Weather Bureau pan, in accordance with the work done by Rohwer at Fort Collins. The present indications are that the agreement will be decidedly unsatisfactory.

A complete paper under the title "A recording evaporimeter" by Jarrett Oliver and N. W.

Cummings will be submitted shortly for publication in the Transactions of the American Society of Civil Engineers and will give the details of Cummings' communication.

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DISCUSSION

R. E. HORTON (Consulting Hydraulic Engineer, Voorheesville, New York)--I have a suggestion to make to Mr. Cummings regarding some of his statements. Other statements, I highly endorse, especially, that engineers have followed altogether too empirical a line of approach. Several who have attacked the problem have a physical viewpoint and have published papers in the Proceedings of the Royal Society, etc. I also refer to studies by John B. Leighly here in California. I do not think you will find your statement quite justified. I cannot take time to outline what has been done or what I myself have done excepting to say there have been a few results published on evaporation from lakes by the heat-value method; if you will give me the data of water-surface temperature and wind-velocity, I can compute the evaporation quite as readily as you have computed it by your method and get the same result as you do by another line of approach.

M. BERNARD (U. S. Weather Bureau, Washington, D. C.)--I have the responsibility of the evaporation-records of the Weather Bureau, and we have in the Bureau a mass of unorganized and unpublished evaporation-data. We are hoping to organize and publish it as basic data. But I do feel that some means should be provided to make the resulting public records of practical usefulness to those who must some way make an estimate of evaporation-losses from open water-surfaces. I am wholly in accord with the view that the empirical approach is wrong. On the other hand, it seems to me to be wrong to withhold this mass of evaporation-data from general usefulness and so I should like to ask Professor Cummings if from his studies he could suggest a practical way of applying what would otherwise be an impossible application of theory to published record of the Weather Bureau pan results.

N. W. CUMMINGS--I should like to make some comments on both these remarks. In the first place, if Mr. Horton has a new, rational procedure for obtaining evaporation, I hope he will inform me where I can obtain material on this so that I can be familiar with it.

R. E. HORTON--I will be glad to supply you with my notes. Some of this material was published in the Engineering News-Record, but they did not see fit to provide the complicated differential equations.

N. W. CUMMINGS--As to the question of wind affecting R, that question has been thoroughly discussed by Bowen if Mr. Horton studies his paper thoroughly. He shows that the coefficient cannot under any conditions be less than 0.46--cannot possibly fall below 0.44 or rise above 0.56. As to the other comment, I suppose those data can be used, but, unless Mr. Horton's literature throws some new light, I do not see how they can.

R. E. HORTON--I labored with the Weather Bureau a number of years ago to have them keep water-surface temperatures, but I was unsuccessful. The Bureau of Plant Industry maintains a number of stations in the West at which they did keep water-surface temperature-records. The records furnished by the Bureau of Plant Industry are ones I have used. I wish the Weather Bureau would now start in maintaining water-surface temperature-records; it is not too late at all class-A stations. Sooner or later this question arises of determining the water-surface temperatures of large lakes. We will apply and interpret; but in order to do that, we must have two sets of data which we do not get from the Weather Bureau, namely,(1) actual water-surface temperatures, and (2) character of wind-movement, whether laminar or turbulent.

E. A. COLEMAN--I still feel enough of a student to appreciate your didactic approach. But one thing still bothers me. As I understand it, your main objection to the shallow pan is the heat-absorption. In that case would not a floating pan in a reservoir give you a good gage of the evaporation from the entire reservoir?

N. W. CUMMINGS--You would be quite helpless in attempting to keep track of the heat-components.

E. A. COLEMAN--I intended to speak merely of the evaporation-records from that pan as being indicative of the evaporation from the entire lake-surface.

A RECORDING EVAPORIMETER

Jarrett Oliver and N. W. Cummings

The instrument herein described was originally designed and built to record the evaporation-loss from a standard Weather Bureau pan for use in a study of the variation of flow in Santa Ana River. Valuable suggestions were made by various members of the Water Resources Branch of the Geological Survey in Southern California, and financial assistance for construction was given by F. C. Ebert and H. C. Troxell of the same organization. The typing of the paper and some of the drafting were done by Works Progress Administration help.

The original instrument was damaged by flood-waters in 1934, while in operation at Baldwin Park, California. It was then redesigned and constructed of stainless materials and installed on the campus of the San Bernardino Valley Junior College. The Weather Bureau pan was replaced by a thermally insulated pan. The damping unit was added at this time by the present writers. The evaporimeter was placed in regular operation on October 30, 1937, in a study of the relative magnitudes of the various energy-components associated with solar and sky radiation and with evaporation from water-surfaces, from damp soil, and from plants.

Any instrument designed to measure evaporation from a given mass of water must depend either on change of volume or change of weight. The latter has at least two important advantages, namely, (1) it eliminates the necessity of accounting for thermal expansion of the water-mass and the container, and (2) it eliminates errors due to the formation of bubbles in the water-mass. For these reasons the weighing principle was adopted in the present case, and the instrument as a whole, was made to consist of three separate units.

The first unit supports the pan and transmits to the second unit a force which is proportional to the combined weight of the pan and water. The second is the recording, and the third the damping unit.

Weighing unit--The evaporation-pan is supported on a platform-scale of conventional design but of special construction. The knife-edges are so placed that the unit is approximately in neutral equilibrium when the evaporation-pan is removed. All bearings are of agate and the knife-edges are of tool steel. The end of the long beam of the platform-unit is connected with the recording unit by a steel tape.

Recording unit--The part of the apparatus which does the recording is an adaptation of the pendulum-type of computing scale in general use. Two circular arcs and a bearing for a knife-edge carrying a pendulum-bob are rigidly fixed to a main knife-edge. This assembly constitutes the pendulum. The line joining the two knife-edges will, for convenience, be called the pendulum-arm. The bob carries the recording pen, which is free to slide along the vertical axis of the bob. The pen is counter-balanced so as to minimize friction between the pen and the paper. The entire unit is made of stainless steel and chromium-plated material. The graph is traced on the same kind of paper as is regularly used by the Geological Survey for stream-flow charts. The driving mechanism for the Stevens water-level recorder is used.

Over one arc is fastened a steel tape which is connected to the long beam of the platform unit. Over the other arc is fastened a similar tape which supports a damping disc and a counterweight which balances the dead weight of the platform-unit. The relative positions of the various parts of the pendulum are shown in Figure 1.

The meaning of the symbols used in Figure 1 and the accompanying equations is as follows: r = radius of arc a--r = three inches; R = radius of arc A--R = 21 inches; W = variable part of force on tape t; W_1 = constant part of force on tape t; C_w = tension in tape T--$C_w = C_1 + C_2$; M = mass of pendulum with B and C_w excluded; L = distance between main knife-edge and bob knife-edge; B = mass of pendulum-bob; α = angle between vertical and pendulum-arm; r_1 = distance between center of gravity of pendulum and main knife-edge; β = angle between pendulum-arm and a line from main knife-edge to center of gravity of pendulum.

Taking moments about the main knife-edge:

$$(W + W_1)r - BL \sin \alpha - Mr_1 \sin(\alpha + \beta) - RC_w = 0 \qquad (1)$$

B, L, M, and C_w are constant.

If the two arcs are circular and centered at the main knife-edge, r and R are constant, and

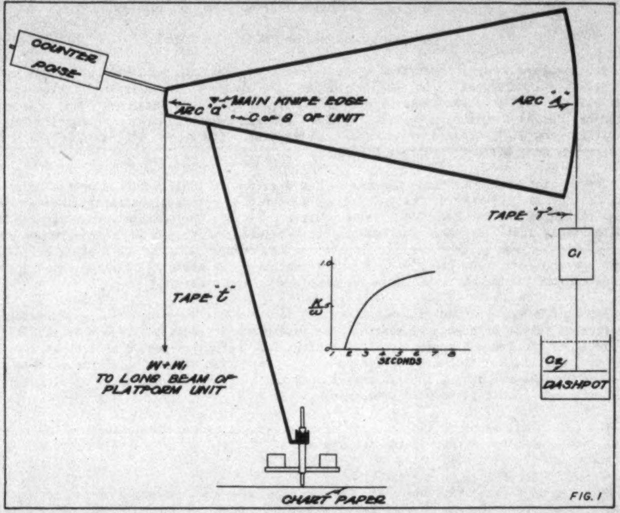

FIG. 1

if C_W is properly chosen $W_1r = C_WR$. Also if the center of gravity of the pendulum is placed in the line joining the main knife-edge and the bob knife-edge $\beta = 0$. Under these conditions differentiation of (1) gives

$$r\delta W = (BL + Mr_1)\delta \sin \alpha \tag{2}$$

It is possible, therefore, to use rectangular coordinates for the evaporation-graph and by properly choosing B the scale can be varied to meet any particular need.

If $\beta \neq 0$, (1) can be written

$$(W + W_1)r - BL \sin \alpha - Mr_1(\sin\alpha\cos\beta + \cos\alpha\sin\beta) - RC_W = 0 \tag{3}$$

By considering the numerical magnitude of the second and third terms of (3) it can be shown that if $r_1 = 0.27$ inch and $\beta = 10$ feet the maximum resulting error in the recorded evaporation will be one per cent. It is easily possible to make both r_1 and β smaller than these specified values.

Several other sources of error have been considered but need not be detailed here. The errors are small and because it is not necessary, practically, that any of the above conditions be met precisely, they can be neglected in design. Since, in the end, the only condition required is that $\delta W = k\delta \sin \alpha$ shall hold throughout the range of W, it is customary to add increments of W, and then dress the face of the arc A, as necessary, to keep k constant. This method was used to give a constant scale throughout the range of evaporation-loss.

As set up for this experiment the scale is one inch equals 2000 cc of water. This scale was chosen because the area of the evaporation-pan is 5800 square cm, so that 1000 cc corresponds to 100 calories per square cm. Thus one inch on the chart represents an evaporation-loss of 200 calories per square cm. In depth of evaporation the instrument responds to less than 0.03 mm and the record is accurate to less than 0.1 mm.

From necessity the evaporation-pan must be exposed to the air and therefore to sudden gusts of wind. It was at first found that sudden changes in wind-pressure frequently caused a movement of the pen equal to the movement representing an entire day's evaporation. This effect seems to be caused by eddies around the members of the platform-unit more than by direct pressure on the surface of the water in the pan. The amplitude of the vibrations of the pen can be materially reduced by inclosing the entire instrument in a box, the top of which is at approximately the same elevation as the top of the pan.

Damping unit--To reduce further the undesirable vibrations of the pen, a damping unit was added to the instrument. This unit consists of a thin metal disc suspended from the recording unit by a small cable and free to move in a cylinder of mineral oil. By means of a simple procedure it was found possible to determine the optimum damping conditions.

For the purpose of making clear the meanings of certain constants, the well-known differential equation for damped vibratory motion of rotation will be written thus

$$I(d^2x/dt^2) + b(dx/dt) + cx = 0 \qquad (4)$$

where I is the moment of inertia of the vibratory system, b is the damping constant, and c is the elasticity-constant, which in this case becomes the gravitational restoring torque per radian of angular displacement. It is convenient to divide through by I and set $2k = b/I$ and $\omega^2 = c/I$ obtaining

$$(d^2x/dt^2) + 2k(dx/dt) + \omega^2 x = 0 \qquad (5)$$

Without difficulty it can be shown that a solution of (5) is given by

$$x = Ae^{\alpha t} + Be^{\beta t} \qquad (6)$$

Substitution of (6) in (5) will demonstrate the advisability of choosing α and β in the following manner

$$\alpha = -k + \lambda \qquad \beta = -k - \lambda \qquad (7)$$

when $\lambda = \sqrt{k^2 - \omega^2}$.

The initial displacement may be found from (6) by setting $t = 0$, thus

$$x_0 = A + B \qquad (8)$$

For the initial velocity we differentiate (6) and set $t = o$, thus

$$v_0 = \alpha A + \beta B \qquad (9)$$

These equations give

$$A = [x_0(k + \lambda) + v_0]/2\lambda \quad \text{and} \quad B = [x_0(-k + \lambda) - v_0]/2\lambda \qquad (10)$$

From equations (10) we readily obtain

$$A - B = (x_0k + v_0)/\lambda \qquad (11)$$

It will be observed that λ appears in the denominators of the expressions for A and B. Consequently if these expressions are substituted in (6) and λ diminished indefinitely, it might at first seem that the value of x will approach infinity. Such is not the case, however, as will presently be shown for imaginary values of λ, by substituting the trigonometric equivalents of the exponentials. It turns out that, if $v_0 = 0$

$$x = x_0(1 + kt)e^{-kt} \qquad (12)$$

When $\lambda = 0$, the system is said to be critically damped, and vibrations just barely do not occur. This is undesirable in the evaporimeter because in the vicinity of the rest-point the restoring torque would be vanishingly small, so that any minute friction, such as exists between the pen and paper, would interfere with the motion of the pen. Matters would become worse if $k^2 > \omega^2$. We must therefore make $k^2 < \omega^2$ which causes λ to be imaginary. Thus if $\lambda = i\ell$

$$x = e^{-kt}(Ae^{i\ell t} + Be^{-i\ell t}) \qquad (13)$$

Eliminating the imaginary exponentials by means of the well-known equation $e^{i\ell t} = \cos(\ell t) + i \sin(\ell t)$, collecting terms, and making use of equations (8) and (11), gives

$$x = e^{-kt} \left\{x_0 \cos \ell t + [i(x_0 k + v_0)/i\ell] \sin \ell t\right\} \tag{14}$$

Obviously, the right-hand member of (14) approaches that of (12) as ℓ decreases.

In order to secure the most effective damping, it is necessary for λ to be almost zero. Its magnitude should be just barely great enough to overcome such friction as cannot be eliminated.

In order to insure that λ^2 should be as small as possible, but never positive, the following procedure was adopted: It was arranged experimentally so that v_0 in equation (14) was zero and the value of t was found that made $x = 0$. This was easily done by displacing the pendulum and releasing it with zero-velocity. The time required for returning to the rest-point with damping, was then determined by the aid of a stop watch. Under these conditions, equation (14) gives us

$$-\ell/k = \tan(\ell t) \tag{15}$$

Before this measurement was made, however, the undamped frequency, represented by ω was found. Values of k giving convenient magnitudes of the ratio k/ω were then chosen and the corresponding ℓ's were computed by the aid of the last of equations (7). These values of k and ℓ were then substituted in (15) and the corresponding t's were found. It was then possible to plot a graph between t and k/ω. This is shown in the central part of Figure 1. This curve indicates clearly that very little is gained by choosing k/ω greater than about 0.8, corresponding to about five seconds. The viscosity of the damping fluid was accordingly adjusted so as to permit the pendulum to pass through the rest-point about five seconds after having been released from rest. This was regarded as the optimum damping obtainable by controlling viscous resistance alone. It is, of course, possible that further improvement might have been achieved by modifying the design of other parts of the instrument and thus changing I or c. This was, however, not regarded as necessary.

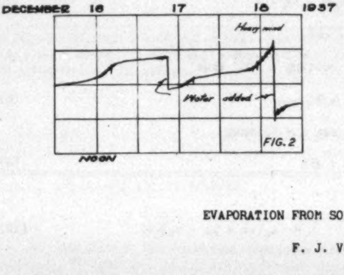

FIG. 2

To illustrate the effectiveness of the damping system, a tracing of three days' record during a period of heavy wind is reproduced in Figure 2.

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EVAPORATION FROM SOILS AND TRANSPIRATION

F. J. Veihmeyer

Early in the course of our studies on water-relations of soils and plants, it was noted that the amount of water transpired by growing plants was tremendously greater than that evaporated directly from the surface of the soil, and that, under the conditions prevalent in California, evaporation-losses are too small to warrant concern. These results are based not only on comparison of the losses of water through trees or plants growing in tanks with losses of water from uncropped tanks but also on the results of sampling the soil for moisture-determinations for fallow and cropped areas in the field.

The conclusion that losses of moisture are occasioned by the upward movement of moisture from the lower moist layers of soil and dissipated into the air is based upon the familiar teaching that moisture is capable of moving through the soil in all directions through capillary forces. It is reasoned that the moisture exists in the form of films around the soil-particles and in wedge-shapped masses of water between the soil-particles at the points of contact with each other, being held partly by the attraction of the non-water particles for the water and partly by the molecular attraction of the liquid itself. Adjoining particles with different thicknesses of moisture-films surrounding them hold different amounts of water in the adjacent capillary spaces.

The amount of this capillary water in the soil is determined by a number of conditions. The most important of these is the size of the particles, the soil-texture. The arrangement of the particle or the structure of soil, and the depth of soil to the level of standing water may be important factors in determining the amount of water held in the soil. Other conditions which exert much less influence are the temperature and the kind and quantity of the materials dissolved in the water.

It has been pointed out by numerous investigators that the moisture-surfaces around the particle containing less amounts of water than adjacent particles will have greater curvature and consequently a greater difference in pressure across the interface. Moisture will move through the connecting films from the greater to the lesser mass until the pressure becomes the same, a process which takes place when the capillary spaces contain equal amounts of water. This movement can extend to any number of capillary spaces through any number of films. A decrease in the amount of water in the capillary spaces in the upper layers of soil would then be expected to result in water being drawn to that point.

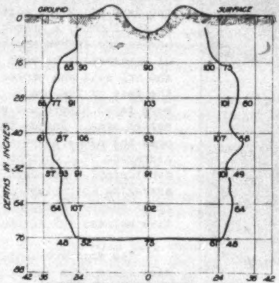

FIG. 1—WET AREA AND MOISTURE-CONTENTS ON OCTOBER 22, 1925 (NUMBERS ARE RATIOS OF MOISTURE-CONTENTS TO MOISTURE-EQUIVALENTS)

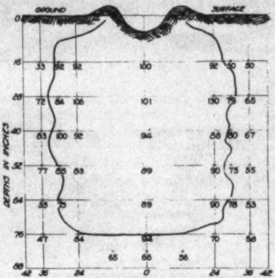

FIG. 2—WET AREA AND MOISTURE-CONTENTS ON DECEMBER 16, 1925 (NUMBERS ARE RATIOS OF MOISTURE-CONTENTS TO MOISTURE-EQUIVALENTS)

Stated another way, moisture will move from a region of high level or high potential to a region of low potential, the motion being proportional to the pressure gradient. It has been shown [see 6 of "References" at end of paper] that the potential is a continuous and probably a single-valued function of the moisture-content--at least through the range of moisture met with in the field.

Movement of moisture in soil contained in glass or metal cylinders and columns of soil in tanks or metal-lined flumes when the column-ends are in contact with free water has received much study. The movement of moisture from moist soils to soils containing lesser amounts of moisture--the field-conditions more frequently met with--has received much study. The results obtained, however, indicate that the movement of moisture especially in an upward direction from moist soil to drier soil is slight in amount and extent when the source of the moisture-supply is not saturated soil in contact with a free water-surface.

The fact that the motion of water through soil under capillary forces is slight in extent is illustrated in Figures 1 and 2 which have been published previously [7]. The first one shows the distribu-

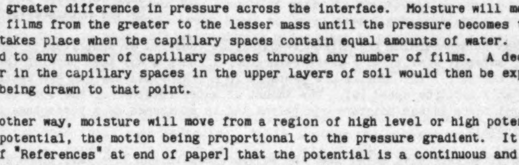

FIG. 3—DISTRIBUTION OF MOISTURE IN CLAY-LOAM FOUR DAYS AFTER A DEPTH OF FOUR INCHES OF WATER WAS APPLIED IN A BASIN (NUMBERS ARE RATIOS OF MOISTURE-CONTENT TO MOISTURE-EQUIVALENTS)

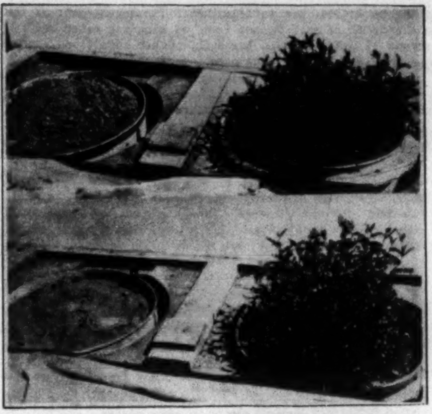

FIG. 4—VETCH-PLANTS GROWING ON SOIL WHICH HAD BEEN EXPOSED TO EVAPORATION FOR A PERIOD OF 80 DAYS AFTER IRRIGATION AND BEFORE SEEDS WERE PLANTED (SOIL IN TANK AT LOWER RIGHT WAS CULTIVATED LIKE THE SURFACE OF THE SOIL IN TANK AT UPPER LEFT AND SOIL IN TANK AT UPPER RIGHT REMAINED UNCULTIVATED LIKE THAT IN TANK AT LOWER LEFT)

tion of water in soil under an irrigation-furrow. The extent of the distribution of water is indicated by the irregular line under the furrow. Before the water was applied, the soil had been dried out by means of growing a crop of barley. On October 22, which was two days after the water was run in the furrow, a trench was cut across the furrow and the line of demarcation between the wet and dry soil was marked out on the face of the tract. Samples were taken within and without the wetted area. Inside the wetted area the numbers are about 100, averaging 97. Outside they are considerably less than this, averaging approximately 50. These numbers represent the relative wetness [2] of the soil.

The moisture-content of samples taken from the face of the trench was determined by drying them in an oven. Since no soil is uniform and since the water-holding capacities of soils vary with the variations of texture, it is desirable to divide the moisture-content by the moisture-equivalent.

The method of determining the moisture-equivalent consists of centrifuging samples of soil in a machine, somewhat similar to a cream-separator, in such a way that some of the water is thrown out of the soil. The samples are prepared in a prescribed way and are centrifuged for a certain time at a definite speed [9]. The amount of water retained by the soil so prepared against the centrifugal force is the moisture-equivalent and is expressed as a percentage of the oven-dry weight of the soil.

A number of workers have shown that the moisture-equivalent is a fair measure of soil-texture. By dividing the moisture-content by the moisture-equivalent of the sample we are able, therefore, to reduce the moisture-percentages to the same base, to a relative wetness base. The result of sampling on December 16, or 57 days after the water was applied, is shown on Figure 2 using the same method as in the previous test on October 22. The trench was covered during the interval between samplings to prevent drying and the wetting by rain. The line of demarcation between the wet and dry soil has not changed very much and the values or numbers inside the wetted area are about 100, again averaging 97, while outside they are approximately one-half of those inside. The downward movement of water was very slight and there was practically no lateral movement of water even within the relatively long period of 57 days.

The distribution of water in the soil when the water is applied in a basin is shown in Figure 3. It may be mentioned that results similar to those with the furrows were obtained. Studies showed that movement of moisture from moist soil to drier soil packed in columns [5] and remaining in contact with each other for 4-1/2 months was slight in amount and extent in both an upward and downward direction.

It is clear there is a rather rapid distribution of moisture throughout the soil wetted shortly after water is applied to the dry soil. The soil-moisture condition within the wetted portion of the soil is called the field-capacity. After the soil is drained to its field-capacity, the downward movement of moisture becomes extremely slow. Conditions are different when enough water is applied to soil to wet it to a depth of about 30 feet or more or to the water-table, in which case it may take many days to drain the soil to its field-capacity. The field-capacity represents the approximate upper limit of moisture-content that can be stored in the soil for use by plants under field-conditions. In most soils, especially those of fine texture, there is a close relation between the moisture-equivalent and the field-capacity. An approximately correct statement is that the difference between the amount of water required to saturate the soil and the field-capacity is that which will drain away and contribute to the underground water.

Since the motion of water at moisture-contents lower than field-capacity is slow, it would seem that the rate of movement to the surface and hence the rate of evaporation-loss would be small. Our experiments show that the loss of moisture by direct evaporation from the soil-surface is largely confined to a shallow surface layer of about eight inches. Furthermore, the evaporation-loss has been shown to be very small compared to the amount of water taken from the soil as transpired by plants. Figure 4 shows vetch plants grown in tanks on soils which were irrigated early in the summer and exposed to evaporation. The seeds were planted in November after the top five inches of soil which had dried was mixed with the moist soil below. No water was applied to these tanks. The photographs were taken in May of the next year.

Even after much longer exposure to evaporation, in some of the tests as long as 167 days before planting the crop, the presence of relatively large amounts of water in the soil in these tanks was strikingly demonstrated by the growth made by the vetch plants. After irrigation the soil in some of the tanks was left undisturbed like the surface of the soil in the tank in the lower left-hand corner of Figure 4. Others were cultivated at frequent intervals like the one in the upper left-hand corner. That cultivation did not lessen the loss of water by evaporation is illustrated by the equally good growth made by the plants in all of the tanks whether or not the soil had been cultivated during the summer.

Some of the tanks with bare uncultivated soil were kept under observation for long periods but were covered during rains so that no water was applied to them after the beginning of the tests. One of the tanks lost only 57 pounds of water during four years of exposure to evaporation and still there was moist soil below the top foot. Thus, 57 pounds loss, which is 18.9 pounds to the square foot of surface exposed to evaporation, is equivalent to a depth of about 3-3/8 inches of water for a period of four years.

The morning-glory plants grown in tanks used 704 pounds of water in 144 days. A four-year old prune-tree in a similar tank used 1250 pounds of water by transpiration during one growing season. One acre of mature deciduous fruit trees at Davis used eight acre-inches of water, or about 900 tons per acre, in about six weeks during mid-summer or approximately one pound of water per day for each square foot of soil. Where water is the main factor the success of the practice of summer fallowing rests upon the fact that the soil loses very small amounts of water by evaporation and not to the frequent cultivations sometimes given.

When the soil is in contact with the free water-surface losses of water by surface-evaporation may be greater than those just indicated. Table 1 gives the average evaporation from water-logged soils at Davis, expressed in inches per day. Depth to the water-table is indicated in the first column. Depths were maintained by automatic water-level regulators. It is interesting to note that the loss of water by evaporation from soils with a water-table at 0.5 foot from the surface is about the same as the average use of water by many agricultural plants. Also, it is interesting to note that with the water-table at about four feet from the surface, the loss by surface-evaporation is approximately one-hundredth of that from soils with a water table at 0.5 foot; or in other words, the transpiration-losses of water through plants, is as much in one day as that which would occur by surface-evaporation from soils with a water-table at four feet in about 100 days.

It seems clear, therefore, that the losses of moisture by direct evaporation from soils have been over-emphasized in much of the discussions of the recent drought in this country. For example, the report [10] of the Great Plains Drought Area Committee states: "With the destruction of the natural grass cover the soil has been exposed to the drying influence of the summer winds--water has not been adequately conserved." There are many earnest advocates of extreme conservation-measures who stress the importance of the litter-covered forest-floor in reducing evaporation-losses without counting the great draft on the soil-moisture supply by the vegetation which produced the litter.

It might be well at this point to consider means by which the amount of water which can be stored in soils may be changed. Organic matter, due to its very great capacity to hold water, is generally assumed to materially increase the water-storage capacity of soils with which it is mixed and thereby increase the available water-supply. The water-holding capacities of manure treated and untreated soils are listed in Table 2. It is thought that well-rotted barnyard manure is one of the best sources of organic matter and since it is the most commonly used material for this purpose, it was selected for our tests. Quantities to give applications of the amounts indicated in tons per acre were mixed with the soil on the basis of a mixture to a depth of one foot.

Table 1--Average evaporation from water-logged soils at Davis

Depth to water	1936	May 15 to Sept. 1, 1937	Mar. 29 to Nov. 10, 1937
feet	inch/day	inch/day	inch/day
0.0	0.328	0.317	0.216
0.5	0.215	0.230	0.182
1.0	0.209	0.194	0.162
1.5	0.093	0.086	0.076
2.0	0.079	0.055	0.051
3.0	.....	.....	0.026
4.8	.....	.....	0.016

If this is considered to be too great a depth to which it would be possible to place the organic matter in the soil in the field, the amounts need only be divided by two for a six-inch depth. For example, the application of 200 tons per acre to one acre-foot of soil is equivalent to 100 tons per acre to a six-inch depth, or 50 tons per acre to a three-inch depth, etc. Of course, on the forest-floor or on soils that are not plowed the organic litter is not incorporated with the soil. Water absorbed by it is held away from the soil and evaporated and cannot be considered to have a direct effect on water-storage in the soil.

The moisture-equivalent is taken as a measure of the field-capacity of the soils and equivalent amounts of water are given as pounds per cubic foot, and as acre-inches to the acre. The increases even with such tremendous applications as 200 tons of manure per acre are surprisingly small, being only 0.2 and 0.3 of an acre-inch with the sandy loam, silt-loam, loam, and clay and 0.4 of an acre-inch in the case of the fine sand. In Table 3, the moisture-equivalents of the soils are listed together with the permanent wilting-percentages which, as explained later, for practical purposes may be regarded as the minimum moisture-content a soil in the field, except in a thin surface-layer, can reach. The difference between the moisture-equivalent and the permanent wilting-percentage is the maximum amount of readily available water which can be stored in the soil [8]. It is evident that there have been no significant differences in the amount of usable water even with applications of organic matter far beyond anything practical in the field.

Table 2--Moisture-equivalents and equivalent water-capacities of manure-treated and untreated soils

Soil	Item	Tons of manure to the acre					
		0	10	50	75	150	200
Fine sand	Moisture-equivalent	2.8	3.0	3.4	4.0	4.6	5.2
	Equivalent pounds per cubic foot	2.5	2.7	3.1	3.6	4.1	5.1
	Equivalent acre-inches	0.5	0.5	0.6	0.7	0.8	0.9
	Organic matter	2.1	2.2	2.9	3.3	4.5	5.3
Sandy loam	Moisture-equivalent	9.6	10.0	10.3	10.4	11.0	11.2
	Equivalent pounds per cubic foot	8.6	8.9	9.3	9.4	9.9	10.1
	Equivalent acre-inches	1.7	1.7	1.8	1.8	1.9	1.9
	Organic matter	1.1	1.2	1.9	2.3	3.5	4.6
Silt-loam	Moisture-equivalent	16.0	15.9	16.5	16.8	16.9	17.4
	Equivalent pounds per cubic foot	13.1	13.0	13.5	13.8	13.9	14.3
	Equivalent acre-inches	2.5	2.5	2.6	2.6	2.7	2.7
	Organic matter	1.6	1.8	2.5	2.9	4.1	5.0
Loam	Moisture-equivalent	21.1	21.5	21.6	21.9	22.4	22.7
	Equivalent pounds per cubic foot	17.2	17.6	17.7	18.0	18.4	18.6
	Equivalent acre-inches	3.3	3.4	3.4	3.5	3.5	3.6
	Organic matter	2.2	2.4	3.1	3.5	4.7	5.6
Clay	Moisture-equivalent	28.4	28.9	29.5	29.3	29.6	29.9
	Equivalent pounds per cubic foot	23.3	23.7	24.2	24.0	24.3	24.5
	Equivalent acre-inches	4.5	4.6	4.6	4.6	4.7	4.7
	Organic matter	4.3	4.5	5.2	5.6	6.9	7.7

Recent work by Feustel and Byers of the Bureau of Chemistry and Soils [3] in which peat was mixed with soils in quantities as large as equal proportions by volume or as much as 1600 cubic yards, or 268 tons of air-dried peat, per acre showed that there is little or no advantage to be gained in moisture economy resulting from the addition of the peat.

The effect of the organic matter probably is greatest just after it is mixed with the soil, the moisture-equivalent of the freshly mixed samples then being higher than when decomposition of the organic matter has occurred. The transitory nature of the organic matter and the extreme slowness with which it is built up in the soil by surface-applications has been observed by several investigators. The data in Table 4 illustrate this fact. The moisture-equivalents of the soil just after mixing and after three months, during which time plants were grown on the soil, are shown. The moisture-equivalent of the manure-treated silt loam, loam, and clay was reduced to that of the untreated soil; and in the case of the fine sand and sandy loam there was a significant reduction even though the organic matter had only three months within which to decompose.

Table 3--Moisture-equivalents and permanent wilting-percentages of manure-treated and untreated soil

Soil	Item	Tons of manure to the acre					
		0	10	50	75	150	200
Fine sand	Moisture-equivalent	3.2	3.4	3.3	3.7	4.2	4.4
	Permanent wilting-percentage	1.0	1.2	1.4	1.5	2.6	2.3
	Available water	2.2	2.2	1.9	2.2	1.6	2.1
Sandy loam	Moisture-equivalent	9.5	...	9.4	9.7	10.0	10.4
	Permanent wilting-percentage	2.9	...	2.9	3.0	3.5	3.8
	Available water	6.6	...	6.5	6.7	6.5	6.6
Silt-loam	Moisture-equivalent	16.1	16.0	15.8	15.9	15.8	15.7
	Permanent wilting-percentage	7.5	7.5	7.6	7.6	7.6	8.0
	Available water	8.6	8.5	8.2	8.3	8.2	7.7
Loam	Moisture-equivalent	21.7	21.7	21.2	20.9	20.8	...
	Permanent wilting-percentage	10.3	9.7	9.6	9.8	10.4	...
	Available water	11.4	12.0	11.6	11.1	10.4	...
Clay	Moisture-equivalent	28.4	28.9	...	29.3	29.6	29.9
	Permanent wilting-percentage	13.4	13.7	...	14.2	15.0	14.8
	Available water	15.0	16.2	...	15.1	14.6	15.1

Table 4--Moisture-equivalent of manured and untreated soils expressed as acre-inches to an acre-foot of soil

Soil	Moisture-equivalent	Tons of manure to the acre			
		0	50	150	200
Fine sand	Just after mixing	0.47	0.59	0.77	0.87
	Three months later	0.54	0.57	0.71	0.74
Sandy loam	Just after mixing	1.61	1.71	1.85	1.88
	Three months later	1.60	1.58	1.68	1.75
Silt-loam	Just after mixing	2.54	2.62	2.68	2.79
	Three months later	2.55	2.51	2.49	2.52
Loam	Just after mixing	3.34	3.42	3.55	3.66
	Three months later	3.45	3.37	3.24	3.32
Clay	Just after mixing	4.63	4.68	4.91	4.85
	Three months later	4.51	4.65	....	4.50

Other investigators have pointed out that there is a rapid loss of organic matter from soils even though relatively large applications are made annually and that relatively small amounts of organic matter are added to soil by fertilizers or cover-crops. Tests [4] made at Davis on orchard-plots which have been clean cultivated and on others which have been in cover-crops for more than ten years show no significant differences in water-storage capacity as measured by the moisture-equivalent. Alway and Neller [1] concluded from field-studies over a period of 22 years that the difference in water-capacities that may be caused by differences in manuring or cultivating operations exerts but little influence upon the productivity.

We have tested the effect on the water-holding capacity of soils and on the amount of available water by adding commercial fertilizers containing nitrogen, phosphorus, and potassium. The concentrations of the applications made were much more than those which would be used on soils in practice yet the moisture-equivalents of the treated soil were not markedly different from the untreated ones. Permanent wilting-percentages of the sandy soils treated with 100 parts per million of nitrogen were slightly higher than the checks. Those of the fine-textured soils were about the same as the checks. At higher concentrations the plants in the sandy soils showed toxic effects of the nitrogenous fertilizers. The loams and clays were not affected until concentrations above 500 parts per million were reached, in which case the plants were stunted and showed evidence of the toxicity of the salts.

It seems clear, then, that the amount of water that can be stored in soils, the moisture-equivalent or the field-capacity, is a rather constant quantity, determined chiefly by the texture of the soil and it cannot be materially changed by the addition of organic matter or of soluble material. Furthermore, the maximum amount of water available to plants likewise is not markedly changed by such treatments.

From what has been said it is apparent that one of the important soil-moisture conditions

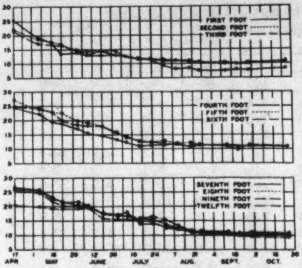

FIG. 5—SOIL-MOISTURE CONDITION IN A WALNUT-ORCHARD, DAVIS, CALIFORNIA, 1936

is that which limits the growth of plants and which determines the amount of unavailable water in the soil. This is the permanent wilting-percentage. It is a term used to mean that moisture-content at which plants wilt and do not revive when placed in a moist chamber or until water is added to the soil. It is the moisture-content below which plants cannot readily obtain water. Because of the difficulties in obtaining precise results in soil sampling and in determining exactly when a plant is wilted, the permanent wilting-percentage represents a narrow range of soil-moisture within which wilting takes place. This can usually be measured to within about one per cent of the soil-moisture. It is characteristic of the soil. The permanent wilting-percentage is independent of the kind of plant or variation of climatic conditions likely to be met with in the field. Figure 5 gives the soil-moisture record in a walnut-orchard for the year 1936, to a depth of 12 feet. The moisture was exhausted in the upper layers about the middle of June and in the lower depths about the first of August. The horizontal portions of the curves correspond to the permanent wilting-percentage and show that absorption of water practically ceases when it is reached. For all practical purposes the permanent wilting-percentage may be taken as the lower limit of moisture-content to which a soil in the field may reach, except in a thin layer at the surface which is affected by surface-evaporation.

Since attention has been directed to the effect of organic matter on soil-moisture properties and to the implications this may have on the value of plant residues on water-storage on our watersheds, it may be well to point out some pertinent facts. Of course, reference is made only to storage of water in the soil and not to other related matters, such as the influence of litter on the rapidity of runoff or to percolation into the soil. It is doubtful whether more than about two inches of rainfall can be stored in each foot of soil, this amount being the difference between the field-capacity and the permanent wilting-percentage. On the shallow soils of most of the mountainous drainage-areas of the West, it is probable that rarely more than 10 or 15 inches can be stored in the soil from which plants extract the water.

The readily available water in three markedly different types of soil is given in Figure 6. The Aiken clay-loam is a typical Western mountain soil. While it has a high field-capacity the maximum amount of readily available water is small, averaging about one acre-inch of water per acre-foot of soil. Yolo clay-loam has two acre-inches, and Madera sandy loam one and one-half acre-inches per acre-foot of soil.

Except, therefore, on very steep slopes where there is insufficient time for percolation of water or on deep soil, it is logical to assume that in a region with rainfall of more than about 15 inches, the soil will be fully wet at the end of the rainy season. The extent of the decrease of the stored water will be determined by the depths of the roots of the plant-cover and the permanent wilting-percentage of the soil. The rapidity with which water-extraction takes place is relatively unimportant since usually there are long periods during the growing season within which there are no rains. It should then be immaterial whether the cover is dense or sparse, whether it is the tall-growing species, or brush- or grass-type if the soil is permeated with roots.

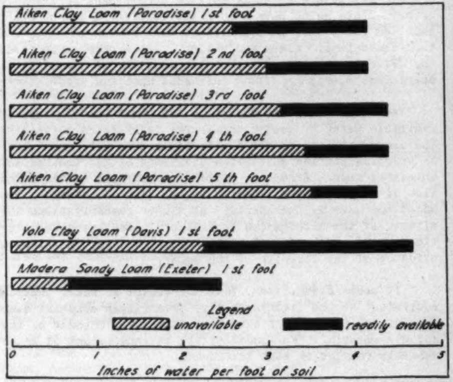

FIG. 6—AVAILABLE AND UNAVAILABLE WATER AT VARIOUS DEPTHS IN SOILS (THE AIKEN CLAY-LOAM IS CALIFORNIA MOUNTAIN SOIL)

It matters little whether the water is taken out within a short time or not until the end of the growing season. In either case the soil-reservoir will be exhausted before the rainy season begins. Stated another way, it seems probable that most of our mountainous-drained slopes will be fully wet at the beginning of the growing season and dry at the end. And from this viewpoint alone the type of plant-cover may play a minor rôle.

Briefly summarizing some of the points discussed: Water, when applied to dry soils, is rather quickly distributed until a condition known as the field-capacity is reached. After this, the motion of the water for practical purposes may be considered to have ceased. The field-capacity of soils cannot be changed markedly by the addition of organic matter or by fertilizers. The amount of usable water which can be stored in soils is also unaffected by the addition of these materials. The amount of readily available water in soils may be considered to be constant for any given soil and, like the field-capacity, seems to be fixed by the texture of the soil. Plant-transpiration is the chief means by which water is lost from the soil. Direct evaporation from the soil-surface is extremely small compared to transpiration-losses. When vegetation-cover is maintained on watersheds for protection, this is done at the expense of the water stored in the soil, and cannot be relied upon as an effective means of increasing the water-storage capacity of soils for flood-protection.

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University of California,
Davis, California

Afternoon Session, January 7, 1938

The afternoon session was under the chairmanship of Professor Frank Adams.

FOREST- AND RANGE-HYDROLOGY IN RELATION TO WATER-SUPPLY

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The discussion as to whether forests were the cause of increased rainfall, or whether increased rainfall may possibly be an aid in the production of forests, attracted the attention of the Topographic Service of the United States Government prior to the first trip of the Pony Express. Time has not changed the general subject, but the wording has been modified, and one of the variations of that same subject is the topic of this paper.

The United States Geological Survey, since its inception, has been charged with the duty of acquiring and preserving, among other things, data of stream-flow and allied phenomena. The attempt is here made, first, to assemble data from this source in an attempt to trace the variation of the conception of stream-flow and its relation to forest-cover through the records of this branch of the Federal Government, and second, to assemble data from other sources to assist in their evaluation and to present the state of our present knowledge on the subject.

The first Geological Survey paper was dated 1896, but competent observers discussed various phases of the problem many years earlier. One of the first, Grove Karl Gilbert, said of conditions in Utah that the irrigators there had found that stream-flow had increased since the settlement of the country. He pointed to the rise of water-levels in Great Salt Lake as evidence of greater inflow as the result of man's activities. At the same time, he contrasted the beneficial effect of man's occupation of the land in arid regions with the bad results brought about in humid areas. Later, Gilbert discussed the problem further, recognizing the effect of varying precipitation from year to year as a dominant factor in affecting stream-flow with man's activities affecting the resulting flows but slightly.

Water-Supply Paper No. 80, published in 1903, was the first of these publications to take up the subject of forest and rainfall. In this paper, "Do forests increase rainfall," George Rafter states that inaccuracies in rainfall-data obscure the solution of the problem and that, at that date, the data as to the effect of forests and runoff were purely tentative.

In 1907, W. W. Ashe, a forester, discussing the Potomac River in Water-Supply Paper No. 192, indicated that the turbidity of the Potomac was not a recent thing and that the deep gorges and ravines indicate the earlier natural rapid erosion of friable soils. He concluded that forests improve the potability of water by (1) prevention of erosion and (2) increase in volume, either as the result of storage, transient in form, tending to prolong or distribute flood-crest or deep seated, tending to increase the amount of the dry-season flow.

In 1909 (Water-Supply Paper No. 234) as parts of a report by the National Conservation Commission, two papers were prepared--one relating to floods, by M. O. Leighton, and one relating to denudation, by R. B. Dole and Herman Stabler. The paper on denudation presented data on the rate at which the Earth's crust is being moved as solid particles carried in suspension by streams and as matter carried in solution. No attempt was made to correlate the findings in the different river-basins with physical or vegetal characteristics, but rough geologic and vegetational correlations may be deduced from the facts shown that the Southern Atlantic, Eastern Gulf of New Mexico, Mississippi River, and Colorado River drainage-basins are losing material faster than the average and that loss in water-suspension is greater than loss in solution in the basins named, and also in the territory tributary to the Western Gulf of Mexico. On an average, it was disclosed that in the United States, movement is taking place at a rate of one inch in 760 years and that about a third of material carried by streams is in solution and two-thirds in suspension.

Leighton made a study of scattered records of flood-frequencies previous to 1890, and of more adequate data from 1890 to 1907, concluding from his studies, that the reduction of forest-cover by natural or man-made processes has influenced largely the unmistakable tendency for flood-frequencies to increase. In contrast with these findings, in Water-Supply Paper No. 771, published in 1935, R. E. Horton found the reverse to be true in the Tennessee River Area.

In 1911, L. C. Glenn, a geologist, after a personal study of the Southern Appalachian Region, decided that deforested regions that become grass-covered are effectively protected from erosion, but that the grass does not absorb the precipitation as well as the forest-cover so the runoff is more rapid, the crest of the flood is higher, and the low water comes sooner from the sod-cover than from the forest [U. S. Geol. Sur. Prof. Paper No. 72].

The Geological Survey was required, under the provisions of the Weeks Act, to conduct experiments on the effect of forests on stream-flow during the years 1911-12. No final report was forthcoming, but a mimeographed paper covering preliminary findings and signed by the Director, George Otis Smith, concluded that "the removal of forest-growth must be expected to decrease natural steadiness of dependent streams during spring at least" for the White Mountain Area studied.

It is unnecessary to more than point out the divergence of the conclusions based upon actual measurements and those based upon general observation and deductive reasoning, nor is it necessary to point out that the above expressed conclusion was the basis (following a favorable report thereon by geologists who used the same conclusion) for making immense expenditures in acquiring national forests in the Eastern states, and later in areas west of the Mississippi.

For more than twelve years, there appears no record that the general topic was further considered by the Geological Survey. The Forest Service and Weather Bureau were, however, conducting an experiment in Colorado at Wagonwheel Gap (1910-26) on a larger scale than had been attempted before.

The Geological Survey continued with its stream-flow measurements. In August, 1924, several drainage-basins in Southern California in which gaging stations were maintained were denuded by forest-fire, the adjacent Santa Anita Watershed was untouched by fire; it, too, had a gaging station with a good record.

H. C. Troxell, Hydraulic Engineer, United States Geological Survey, made an exhaustive study of the changes in runoff as the result of the destruction of vegetal cover. Meanwhile, W. G. Hoyt, Hydraulic Engineer, United States Geological Survey, prepared a careful study of the data collected by the Forest Service and the Weather Bureau at Wagonwheel Gap between 1910 and 1926.

The permission of the Director of the United States Geological Survey being obtained, the conclusions of these two investigations were published in the Proceedings of the American Society of Civil Engineers, and after discussion by 19 engineers, the whole were incorporated into the Transactions of the American Society of Civil Engineers [v. 99, pp. 1-111, 1934]. Thus, in one place appears the only discussions of results of experiments and studies developed from data obtained from sufficiently large areas to be termed basin-wide upon this subject which have been published. These data show the magnitude of the effect of deforestation upon stream-flow; in one case, only the trees were removed (underbrush remained), in the other, burning effected complete denudation.

The conclusions reported follow:

(1) Total runoff--Forests did not "conserve the water-supply," because after their removal there was an increase in average annual yield amounting to 15 per cent in a mountain area in Colorado and 29 per cent in a Southern California coastal mountain area.

(2) Distribution of increase in runoff--Contrary to the widely quoted opinion, the increase in runoff is not confined wholly to flood-periods. In both the Wagonwheel Gap Area and in the Southern California Area, 52 per cent of the increase occurred during the non-flood period. The flow during the non-flood period is derived from sub-surface storage. The increase during non-flood periods results from either (a) increased sub-surface flow and storage, or (b) decreased transpiration, or (c) a combination of both. More of the precipitation may enter storage after removal of vegetative covering for the following reasons: (1) Less interception by trees, undergrowth, tree-litter, and humus; (2) faster melting of snow with corresponding decrease in evaporation. The gradual lessening of the increase during the non-flood period in both areas after the second or third year may be a reflection of gradual increase in plant-growth and corresponding increase in transpiration.

(3) Maximum daily discharge--In the Wagonwheel Gap Area, there was an average increase of 46 per cent in maximum daily discharge after deforestation. This gain is due to increased sub-surface flow, as practically all the snow had melted several days prior to the date of maximum peak.

During flood-periods, following the snow-melting, the discharge in Area B uniformly reached a peak three days or more after that in Area A, but after the storm of October 5, 1911, the peak in both areas occurred at the same time. This was the only so-called flood that resulted from rainfall and indicates that surface-runoff from both areas reached the gaging station at practically the same time. In the Southern California Area, the four storms occurring during the first year after the fire resulted in an increase of 1700 per cent in the maximum daily discharge. The peak-discharge, which was ordinarily 2.5 times the maximum daily discharge prior to the fire, increased to 16.2 times on April 4, 1925--the maximum peak for the period of record. The floods in the Southern California Area usually result from rainfall and represent direct surface-runoff.

The removal of vegetative covering clearly increases normal flood-heights. Beginning in the second year after the fire on Fish Creek, the flood peak-discharges were practically the same as those which occurred before the fire, indicating that the new growth, small as it was, exercised practically the same effect as the original cover in reducing flood-crests.

The gradual increase in vegetative covering on the Wagonwheel Gap Area B after deforestation had little effect on the increase in flood-runoff. This was to be expected in that most of the flood-runoff in this Area reaches the stream through underground channels.

The earlier melting of the snow in the Wagonwheel Gap Area resulted in an advance of three days in the flood-peak. Where rain passes directly into streams without entering the ground, the flood-peak for any small element of drainage-area occurs so soon after the storm that removal of vegetative cover has little effect on the time-element. In the Southern California Area, the peak was advanced only a few hours.

(4) Summer runoff--It is almost universally believed that forests or vegetative covering will increase summer runoff and shorten the low-water period through the exercise of storage-functions. This belief is an outstanding fallacy in so far as these two widely different areas are concerned. The summer flow (July to October) in the Wagonwheel Gap Area showed an average annual increase of 12 per cent during the seven years after deforestation, and the Southern California Area, an average annual increase of 475 per cent during the six years after the fire. The absolute average increase of runoff, in inches, for both areas was practically the same. The high percentage of increase shown for Fish Creek results from the extremely small summer runoff of that stream under normal conditions. This increase is probably a result of increase in subsurface storage and decrease in transpiration.

(5) Minimum daily discharge and date of occurrence--Coincident with the increase in summer runoff there was an increase in the average summer minimum and the period of low-water runoff was considerably shortened. In the Wagonwheel Gap Area, the average minimum was increased about 12 per cent and the time of occurrence delayed about five days. In the Southern California Area, the average minimum was increased more than 400 per cent and the time of occurrence was delayed about 30 days.

(6) Winter minimum--Deforestation made no appreciable change in the low flows which occurred during the winter in the Wagonwheel Gap Area.

(7) Erosion--Erosion results from surface-flow. In the Wagonwheel Gap Area, there was practically no evidence of erosion after deforestation, and this was to be expected because there was little direct surface-runoff either before or after deforestation. In the Southern California Area, complete denudation increased erosion as a direct result of the increased surface-runoff. So far as the surface-features of the Fish Creek Basin were concerned, the erosion did not destroy any present use. However, deposition of eroded material and ash carried by the streams the first year after the fire was materially injurious to agricultural lands and transportation rights-of-way below the canyon. As the growth of new vegetative covering by the second year after the fire was nearly as effective in reducing normal flood-runoff as the original cover, it may well be argued that if the Fish Creek Area had been deforested by cutting of the vegetative cover, as in the Wagonwheel Gap Area, without the destruction of the roots and tree-litter, the erosion would have been practically the same as with the original vegetative covering.

Herman Stabler, Chief, Conservation Branch, United States Geological Survey, provided one of the discussions to the Hoyt-Troxell paper. In it, he pointed out the following from material gathered in reporting [Tech. Bull. No. 220, U. S. Dept. Agric.] on the grass- and weed-covered slopes of the Wasatch Mountains in Utah:

(1) On the steep grass- and weed-covered mountain sides of the Wasatch Plateau, Utah, wide changes in vegetative cover and in grazing practices (a) produce little change in the rate of runoff from summer storms--slightly more runoff with more grazing and with less vegetation being indicated--and (b) affect notably the rate of sediment removal from summer storms--more erosion with more grazing and less vegetation being indicated.

(2) Under the conditions of soil, relief, and climate affecting the experimental areas, measures that will restore and maintain the maximum plant-cover for grazing purposes will also insure adequate watershed-protection.

(3) A cover of grass and weeds, grazed under careful regulation, may afford protection to soil as satisfactorily and more profitably and deplete water-supply less than a cover of useless brush. Relative effects of grass and weeds, brush, and trees on water-supply and soil-protection should be studied to afford a reliable guide in administration and use of lands.

(4) If wide variations in vegetative cover and grazing practices on steep mountain sides, where the annual precipitation is about 30 inches, produce changes in runoff and erosion no greater than indicated by the Wasatch Plateau experiment, neither variations artificially producible in the annual forage-crop of about an ounce to the square yard on the relatively flat-lying and arid unreserved public domain (170,000,000 acres), nor the presence or absence of a stock population sufficient to harvest it, can affect appreciably problems of water-supply or sedimentation of the arable lands of the West.

(5) The study, in general, tends to corroborate the conclusions reached by Hoyt and Troxell and to extend their application.

In Water-Supply Paper No. 796C, H. C. Troxell and J. Q. Peterson report results of studies conducted in the hills above Glendale, California, after the destructive New Year's flood of 1934. They concluded that the recent burn-over had accentuated the volumes of water and debris from those areas, but that the intensity of the storm was largely to blame for the magnitude of the runoff. They say: "The main source of debris carried in Pickens Canyon during the New Year's storm was probably the material deposited in the stream-channel by earlier storms...It appears that the presence of a vegetative cover, while it may tend to reduce landslides and debris-movement, is in nowise a guaranty against soil-movement if the rainfall is heavy and continuous..., that heavy rainfall will produce flood-runoff, whether the area is forested or nonforested, although forest-cover may tend to reduce flood-discharges."

In 1934-35, the United States Geological Survey in cooperation with a Committee appointed by the Section of Hydrology of the American Geophysical Union and by a second committee from the American Society of Civil Engineers, obtained material for two Water-Supply papers. The first, No. 771, covering a detailed study of flood-magnitudes and frequencies, was completed with the assistance of a group from the American Society of Civil Engineers. The second, No. 772, with the assistance of the American Geophysical Union Committee, considered the relationship between rainfall and runoff. R. E. Horton, a member of the American Society of Civil Engineers Committee, submitted a separate report covering his findings on the records of the Tennessee River for two periods, 1874-1903 and 1904-33. He found that floods occurred less frequently and were of lower magnitude during the first four spring months of the latter period than in the former and justifies this condition as follows: "Before the timber was cut, snow might tend to remain unmelted in the woods, accumulating until removed by heavy spring rains, then producing a flood of high intensity. After the timber was cut, the snow might generally not accumulate, but melt or be carried away by rain between successive snowstorms, producing more floods but these of smaller magnitude. An assumption that deforestation is partly the cause of a decrease in flood magnitudes in the Tennessee Drainage Basin is therefore consistent with the findings resulting from the statistical analysis."

In Water-Supply Paper No. 772, findings are reported which are at variance with the opinion frequently held and raise the question, "whether cultivation has not accomplished conservation of moisture for crop-production in amount sufficient to overweigh any increased surface-runoff that might have been occasioned as a result of agricultural or other activities of man."

The Advisory Committee felt, in concluding, that enough data were not available to reach definite conclusions as to man's effect on meteorology or stream-flow conditions in the basins studied.

In 1935, W. P. Cross, a Junior Hydraulic Engineer of the United States Geological Survey, reviewed 200 books and articles on the subject under discussion, and abstracted 69 of them as part of a thesis written under the direction of Professor C. E. Sherman of Ohio State University. He concluded as follows: "Volumes have been written, excellent arguments and marvelous inductive reasoning have been presented, but there is very little actual evidence for either side and much of this is circumstantial in character. Several very elaborate experiments have been completed recently, but even these have been criticized as inadequate, and it may safely be said that at the present time there is insufficient evidence upon which to base a conclusion as to the influence of forests upon stream-flow. It is surprising that so little has been accomplished with so much effort, but the problem of stream-flow is a very complex one and requires study rather than argument."

Studies made by the Geological Survey of the relation between the rainfall and runoff during the flood of March 1936 embraced the entire northeastern part of the United States. Part I, New England Rivers, has appeared as Water-Supply Paper No. 798. The results of the studies in the other districts will soon be published as Water-Supply papers 799 and 800 in the other districts. The studies indicate: "The direct runoff was a function of the total amount of water in the area in the form of rain and melted snow and that differences in cover, topography, channel-conditions, and other features apparently had little influence on the total direct runoff. These factors, however, may have influenced the concentration of the runoff with respect to time, as measured by the ratio between the direct runoff during the maximum day of each flood and the total direct runoff during each flood. Preliminary analyses of the hydrographs for all the gaging stations in New England indicate that at about ten per cent of the stations the direct runoff during the maximum day was 40 per cent or more of the total runoff. These high ratios were obtained for gaging stations situated in headwater-areas. At about 20 per cent of the gaging stations the runoff during the maximum day was only 20 per cent or less of the total runoff. The gaging stations where these low ratios were obtained were, in general, in the lower parts of the basins. At the remaining 70 per cent of the gaging stations, the ratios of the runoff during the maximum day to the total runoff were from 20 to 40 per cent.

"It is also evident that with only slightly different timing or combination of events and conditions, the flood-runoff in many areas might have been much greater."

Referring to the Merrimac River, the studies indicate: "The measured direct runoff during the total storm-period in these headwater-areas (Merrimac River) approximates 20 inches, thus indicating practically 100 per cent runoff for the storm-period...There is strong reason to believe that the measured runoff of 20 inches during the storm-period is reasonably correct, and this figure gives some idea of the amount of runoff that may be produced in a mountainous, timbered, headwater-area from a combination of early spring rains and accumulations of snow. Nearly all minor basins in the Merrimac River Basin showed runoffs averaging from 14 to 16 inches during the storm-period."

In the next publication of the United States Geological Survey, Water-Supply Paper No. 880, dealing with the influence of vegetal cover upon stream-flow, the reference is somewhat indirect, being (in part) a discussion upon the inter-relationship of trees and climate on the shelter-belt in the region of the Plains and Prairie States: "This theory seeks its support on the assumption that, inasmuch as trees and vegetation, in general, transpire large quantities of water, they serve well to increase the atmospheric moisture available for precipitation. It should now be realized that in a region in which the runoff represents an inch or less in depth on the basin, evapo-transpiration losses are about equal to the rainfall. The prevention of the return to the sea of the small runoff is valuable as a conservation-measure but at most it can have slight if any effect on local precipitation.

"Atmospheric moisture is essential to rainfall; it is also necessary that meteorological events be such that condensation may occur. It is possible, and moreover it frequently happens, that the atmosphere contains moisture sufficient to produce substantial precipitation and yet no rain falls. J. B. Kincer, meteorologist of the United States Weather Bureau, in an address before the American Meteorological Society at Pittsburgh, Pennsylvania, December, 1934, has outlined the factors requisite to condensation as follows: 'The only effective method of producing rain is through the cooling of the air in volume and degree sufficient to extract from it a goodly portion of its valuable water-treasures hidden in vapor-form. Nature effects this cooling in a number of ways and usually on an immense scale, far greater than man could ever hope to emulate. These masses are of two major sources, polar and tropical; those of polar origin are dense, heavy, and relatively cold, and those of tropical inception are warmer and lighter. A mass of tropical origin moving northward may come in contact with a polar mass, and, being lighter, it naturally flows up over the opposing dense air just as it would flow up the side of a mountain that by chance may be disposed in its path. In its ascent, through expansion, the cooling necessary to produce rain is effected. This illustrates Nature's method of producing rain in appreciable amounts; no other is effective. Under different circumstances the process varies, of course, often being decidedly local.

" During the past droughty summer, there was a persistent absence of the dense, cool, polar air-masses from the north until about the middle of August when a more normal movement began, definitely breaking the heat-wave, and in conjunction with more favorable cyclonic air-circulation farther south, producing drought-relieving showers over the interior states. Just how these drought-conditions definitely establish themselves and persist so long, as in 1934, has not yet been determined, but it is evident that any effort of man to effectively change the situation would be fruitless.'"

In order to prevent misinterpretation of the purport of the above reference, the next quotation is inserted. In its report "Possibilities of shelter-belt planting in the Plains Region," 1935, the United States Forest Service makes the following statements: "The general effect of shelter-belts is not the creation of more rainfall over the area covered by tree-growth, but the more economic use and conservation of the available rainfall. Shelter-belt planting is not a cure-all of unfavorable climatic conditions in the Plains Region.

"Romantic expectations such as raising the water-table or increasing the rainfall should not be entertained.

"It is apparent, therefore, that most of the benefits that may be expected from a shelter-belt relate to the amelioration of the effects of the wind and to the esthetic aspects."

Excerpts heretofore quoted have dealt with the measurement of flowing waters as a basis of measurement of the effects of precipitation. Here follows a part of the conclusions of the Great Lakes Study, by R. E. Horton, in collaboration with C. E. Grunsky: "The entire north region was never covered with pine. The average condition was probably about midway between that for an open forest and the present conditions. Still it seems certain that as a result of cutting off of the northern forests the water-losses have been reduced and the yield of streams or inflow to the Great Lakes from this region has been materially increased. That this should be so is indicated by the fact that cut-over lands, with only brush and shrubs remaining, form an ideal catchment-area, especially where, as in the northern region, the soil is sandy and permeable, so that most of the rainfall goes immediately into the ground below the reach of capillary uplift and soil-surface evaporation. Brush-cover reduces interception- and transpiration-losses to a minimum and at the same time shades the soil-surface, reducing evaporation. As the northern region is gradually brought under cultivation, there is a tendency to increase the water-losses, although they will probably never equal those from the original pine-forest.

"The net effect on runoff, which equals rainfall minus water-losses, has apparently been about five inches for the Lakes Michigan-Huron Region as a whole. So far as effect of reduced water-losses is concerned, cutting off the timber from the drainage-basin of Lakes Michigan-Huron has tended to prevent these lakes from falling as low as they otherwise would have fallen."

In 1908, Col. H. M. Chittenden, United States Army [Proc. Amer. Soc. Civ. Eng., v. 62] in "Forests and reservoirs in their relation to stream-flow," directed attention to the two neighboring streams in California, Putah Creek and the American River, their respective flood-discharges and the rainfall which had produced these discharges during the highwater period of 1907.

Pointedly directing attention to the differences in vegetal covering of the watersheds, Col. Chittenden said: "The tributaries on the east side come down from the densely wooded slope of the Sierra; those on the west side from the bare and sparsely wooded slopes of the Coast Range. If the forest-theory be true, these smooth western slopes should send down a greater flow for the same precipitation than the eastern. Exactly the reverse seems to have been the case. For the period March 17-26, the precipitation on the Putah Creek Watershed on the west side (805 square miles) averaged 22.7 inches. The maximum resulting runoff per square mile was 39 cfs. Directly across the Valley, on the Sierra Slope, the precipitation on the American River Watershed (2,000 square miles) averaged 14.6 inches for the same period, and the maximum daily discharge was 48.7 cfs per square mile. Considering the fact that the unit-runoff for the same conditions is always less, the greater the watershed, this result is quite remarkable. It is undoubtedly due to the action of the Sierra forests on snow melting and again illustrates the inability of the forests to exert any restraining influence upon great flood."

Then came Gifford Pinchot, Chief Forester, who said: "Colonel Chittenden has made an unfortunate choice when he selected Putah Creek Watershed." His data for the American River are all right, but his rainfall data on the Putah Creek watershed are incorrect. If we take into consideration some additional rainfall-stations we have the Putah Creek rainfall as between 9.5 and 10.9 inches, not 22 inches as the Colonel states. So the rainfall on the American exceeded the Putah by 53 per cent while the peak-runoff exceeded the Putah by only 23 per cent.

In reply, Col. Chittenden shows that of the eight rainfall-stations used by Pinchot, six were outside of the watershed. Chittenden suggested that Pinchot take in the stations Guinda and Lakeport, leave out Vacaville, Dunnigan, and Woodland, and the runoff would still have been produced by an average rainfall of 18 inches.

Now the anticlimax of the violent and acrimonious discussion is the fact that the place where the large error was made was not in the Putah Creek Watershed, but on the American, where, at the time, both contestants agreed. It was not until 1928 that there was an opportunity to determine the flow of the American at a flood stage approximating the discharge in 1907. Then it was discovered that the discharge at the stage recorded in 1907 was badly in error, and that the peak-discharge in excess of 182,000 cfs for 1921 square miles of watershed gave a figure 94.8 cfs per square mile. Thus for a rainfall of 18 to 20 inches on Putah Creek, the peak-discharge was 39.1 cfs per square mile, while for a rainfall of 14.6 inches on the American River, the peak-discharge had a maximum of 94.8 cfs per square mile or a 24-hour mean of 72.4 cfs. In any event, the restraining effect of the forest was of small avail against rainfall-intensities and snow-conditions experienced.

In 1937, Mr. Horton discussed "Determination of infiltration-capacity for large drainage-areas," and in this paper, infiltration-capacities from stream-rises described in Water-Supply Paper No. 772 [Trans. Amer. Geophys. Union, 18th annual meeting, p. 371, 1937]: "Soil-surface acts as a sieve separating rainfall from surficial runoff and infiltration."

Infiltration as it may be affected by forests or otherwise is a factor in flood-runoff or in water-retardation. In describing rates of infiltration he observes: "The highest is for October

5, 1927, at the end of a long dry period. The lowest occurred when the soil had been moderately wet by recent rains."

Horton develops a curve of season infiltration-capacity extending from April to November, rising to a peak in August, and with reference to the 1927 New England flood states: "The precipitous, rocky, mountainous and forest-covered areas, such as the upper Connecticut Drainage-Basin, show generally lower infiltration-capacities than the flatter agricultural slopes."

He concludes: "One important use of infiltration-capacity is in the estimation of maximum flood-intensities to be expected on a given area. For this purpose the minimum infiltration-capacity should, in general, be assumed."

S. T. Harding [Trans. Amer. Geophys. Union, 18th annual meeting, p. 370, 1937], in discussing "Direct accretion to ground-water from rainfall" defines it thus: "It is the part of the rainfall which is absorbed by the soil and not used as plant-transpiration or soil-moisture evaporation. . . Deep penetration of rainfall depends upon the moisture-content of the soil at the time the rain occurs.

"Much larger amounts of rainfall (in Southern California) were required to cause penetration in deep-rooted brush than in areas of grass or weeds [Blaney, Calif. Div. Water Res. Bull. Nos. 33 and 46].

"The prevalence of sod instead of brush or trees (in the Great Plains) is an indication of lack of rainfall-penetration.

"In the Pacific Coast areas rainfall-penetration is larger than would be expected from the same general average rainfall in other areas owing to the concentration of the rainfall in the winter months of semi-dormancy of plants."

Mr. Harding's Pacific Coast data referred principally to the south coastal basin, which is the location of probably the largest area of the best soils for infiltration to be found. For several years, past, stream-flow has been spread for percolating purposes. The ineffectiveness of infiltration as a means of reducing flood-flows was demonstrated during the recent heavy precipitation (1938) when the streambed was miles wide in places and the resultant damage ran into millions.

Other authorities endorsed by L. K. Sherman indicate an advantage of 15 per cent in favor of the forests from measurements of the variation of infiltration-capacity of otherwise similar soils, located in forests and elsewhere [J. T. Auten, J. Agric. Res., v. 46, p. 11, 1933]. In Illinois, Indiana, and Ohio, he states: "Forests greatly reduce, and in many cases eliminate surface-runoff from the soils which they occupy in the St. Lawrence Drainage-Basin."

These data arrange themselves in two definite groups, which point out that there are two definite periods in which, for his convenience, man desires to affect stream-flow: (a) When the flow is large, to diminish it; and (b) when the flow is small, to increase it.

In the lack of fulfillment of his desires, man universally attempts to justify or excuse his disappointment by accusing some other member of the human family with being the destructive agent. The age of laying responsibility for catastrophe upon evil spirits is no longer in vogue. The primary source of all water is precipitation, and this, after it touches the Earth, registers its presence upon the mind of man as stream-flow. Stream-flow may result from surface-runoff or from ground-water draining back into the channel. For a large part of the world, the effect of surface-runoff is of major importance, because of flood-hazard for a considerable portion of the Earth; ground-water is a vital requirement. This is especially true in semi-arid regions.

Ground-water as its name indicates is precipitation which has percolated into the soil and passed downward beyond the root-zone. Infiltration is the process by which this water enters the soil; it varies greatly with the amount of water already in the soil, with the rate of rainfall, the length of the storm, with soil-qualities, and with the same soil under different conditions, with size and grading of soil-units--whether it is frozen, frosted--and with the degree of cultivation.

No runoff occurs unless the rate of rainfall exceeds the infiltration. For relatively low rates of rainfall, very definite modification in rate of surface-runoff may be produced either by infiltration or vegetal cover, acting separately or together. Major flood-flows cannot be prevented. Because of the paucity of our knowledge as to the true magnitude of major flood-flow, financial expenditures for flood-control cannot safely be reduced by placing dependence for safety upon either infiltration or vegetal cover. If adequate knowledge of flood-flow existed, these two factors would have to be neglected as safety provisions if knowledge of the experiences of 1936 in New England and of 1938 in California are to be recognized.

It follows, therefore, that as a flood-control expedient, both infiltration and vegetal cover must be evaluated at zero for a flow anything like a major flood such as occurs in the semi-arid West or for a non-summer flood on the Eastern seaboard. These facts being true, further expenditure to increase infiltration in forests or elsewhere under the claim of flood-control is unjustified. Any other conclusion is untenable.

It is interesting to reflect upon the effectiveness of forests and native vegetal cover in affecting flood-flow, prior to the inauguration of the wasteful processes of civilization. The first settlement in the Sacramento Valley was made by General John A. Sutter in August, 1839. He selected for his camp the knoll on which the fort still stands. In the mountains there were the densest forests; in the forests, there lay the deepest beds of humus. With the rains, there were the most perfect reservoir-effects (as long as they were in action)--yet the rivers rose out of their banks, the knoll a mile from the river-bank was not topped, but the cattle in the corral stood belly deep in water. For six weeks the pinnace, which was the only means of communication with Yerba Buena, could not safely venture forth, and when finally the boat did get away, the round trip took six weeks compared with the usual time of transit of ten days to two weeks. Primeval forests, humus, snows, and surface-retention, separately or combined, were of no avail in preventing flood-runoff in the locality.

Floods continued just as they had come in 1825-26 when Jedediah Smith was marooned on the Marysville Buttes. There were other floods in 1847, in 1849-50, in 1852-53, 1861-62, and so on; floods before the discovery of gold and afterward. It is interesting to observe that Sutter's surveyor, Voiget, in 1841, in preparing his survey for Sutter's Grant, specifically exempted the areas "usually overflowed in winter." The floods of 1861-62 were undoubtedly greater than any which have occurred since that date. However, W. W. Waggoner, Member, American Society of Civil Engineers, former State Debris Commissioner, quotes from the press of February 4, 1862: "There are marks now which show beyond a doubt that the Sacramento Valley has had water in it five or seven feet deeper than it was in the great flood of January." This is corroborated by Thomas Rowlandson, F. G. S. L., writing in 1862: "Marks exist on trees growing in the San Joaquin Valley showing that a former flood had been fully six feet higher than 1861-62." So much for the effectiveness of natural cover, etc., in preventing major floods.

Now let us look at the record of low flow of the same streams, when the regulating effects of infiltration should normally have equalized summer flow for navigation, and should have maintained an ample and potable supply for other purposes; erosion should have been negligible, because of undisturbed vegetal cover. The United States Exploring Expedition (Wilkes, 1838-42) dispatched Lieut. Ringgold to San Francisco, where he arrived August 14, 1841 (Sutter's first winter was 1839-40), and proceeded with the exploration of the Sacramento River. Upon leaving the ship at anchorage, the expedition proceeded with a launch and three boats, finishing the first day's run on what is unmistakably Sherman Island in the Sacramento River Delta. The cook proceeded to prepare the evening meal, which when served, could not be eaten, because the river-water in which it had been cooked was salty--so a dry camp was made. Proceeding toward Sutter's establishment, shoals were encountered just below the site of Sacramento and the launch had to be left behind. When the mouth of the American River was reached, the rowboats had to be dragged across the bar which completely closed the channel, and when the mouth of the Feather River was reached the boats had to be unloaded and carried across that bar. Truly, undisturbed forests and vegetation did not regulate sufficient summer flow for rowboat-navigation, nor was the quality of the water satisfactory, nor were the effects of natural erosion hidden.

That this condition was not unusual is evidenced by the fact that the Indians drove rows of stakes across the river to make "fish-weirs" at which points the spearing of fish was successfully practiced; these fish-weirs were a serious inconvenience to the expedition's boats. This is not the only evidence of lack of navigability of the River. The Sacramento City Directory, 1851 [P. 74], states, "From September 1850 until the rains began, the mail was carried on horseback to Benicia, where it took to water again."

Further evidence of the same phenomenon is present in the history of the founding of the town of Fremont near the mouth of the Feather. In March 1849, Jonas Spect freighted a small schooner with merchandise at San Francisco and sailed to the head of navigation on the Sacramento River for the purpose of establishing a trading post from which mining companies at the heads of that stream, the Feather and the Yuba, could obtain their supplies. On March 25, 1849, during the usual high-water season, he began business. The waters of the succeeding winter washed out the fords and the head of navigation was moved 100 miles up the Sacramento, and up the Feather to the City of Marysville. Channel-obstructions, sand-bars, and similar evidence of the products of erosion existed in March, 1849; the first year after the discovery of gold, there was not sufficient water for the navigation of small, very small boats, even in the period prior to extensive change in vegetal cover on the watersheds.

The question naturally arises, just how much if any dependence should be placed upon the effectiveness of vegetal cover or melting snows upon water-supply, if in the undisturbed state of nature, the same problems were present as are encountered today, in flood-flows and low-water conditions. Just how much relief can be expected by an attempt at replication of the original conditions? The answer is self evident.

The question then arises, why have not the scientist, the engineer, and the farmer so marshalled their facts as to lead to the same conclusion as natural laws have produced?

The whole answer to the summer-runoff problem in California seems to be involved in the consideration of "infiltration" or "soil-moisture" and the interpretation of our knowledge upon these subjects.

An approach to the subject may be made by considering (1) how much water can be impounded in the mountain soils and (2) how slowly will it drain back into the streams. Expressed differently, how deep is the soil and how steep are the side slopes? It may safely be stated, that in general, the depth of the soil-layer decreases with altitude, on the summits there is little soil; at lower elevations, say around 2500 feet, the soils average possibly ten feet in thickness. Let us assume that the whole area is covered with say ten feet of soil, of which less than 50 per cent represents the voids. If this ten feet of soil is water-logged and it can drain, the soil will retain moisture up to its field-capacity, say 17 per cent. The balance, 33 per cent, will drain off. If there is not sufficient water to raise the water-content of the soil to field-capacity, no water will drain off. If the soil has been previously dried to the wilting-point, there will still be about eight per cent moisture present, and the moisture-deficit will approximate nine per cent of the soil-volume. Upon the assumption that the average depth of soil is ten feet (120 inches), it would require 9.8 inches of rain to fulfill the soil-deficiency to give to the ten feet of soil all of the water which it can retain. There is probably no place in California at an elevation of 2500 feet or more which does not receive several times this amount of rainfall each season.

During the growing period, the depletion of soil-moisture as measured in foot-hill orchards [Edlefsen, Agric. Eng., v. 18, No. 6, p. 249] was found to be 13.3 inches in three months in addition to natural rainfall in apple orchards (about 100 trees per acre). In a forest or on brush-covered land the amount of transpiration would not be less.

It may therefore be stated with confidence that at the beginning of each growing season, the soils in mountain areas are fully provided with all the water they can hold and that at the end of the growing season, they have had all the available water removed by transpiration. Little rain falls during the growing season.

Between field-capacity and complete water-logging in a ten-foot depth of soil, there is available pore-space of approximately twice the 9.8 inches which represents the field-capacity, or say 20 inches. It would appear that if the rate of precipitation does not exceed that of infiltration, no runoff should occur during the stormy season for total precipitation not exceeding say 30 inches of rainfall. But there is the additional question of how long would it be before the surplus drains out of the soil and appears as surface-flow. This is flow through soil under saturated conditions of the general type measured by Bodman and Edlefsen [Soil Science, v. 38, No. 6, p. 441, 1934] where it was found that the rate of flow under saturated conditions (silt-loam) was 7-1/2 inches per day; that in a few days after complete saturation that at a plane of nine feet below the surface the rate of downward flow was less than one-quarter inch per day, or approximately equal to the amount required for orchard-growth and transpiration. Since it may safely be stated that the flow through coarser soils is more rapid, it then follows that in the mountain areas, similar rate of downward movement of water would be more rapid; it would appear logical that, given a place where structure or grade is sufficient for soil-drainage, an average mountain soil saturated or water-logged will drain at a rate so that field-capacity only is left at a point ten feet below the surface a few days after complete water-logging ceased. Thereafter, movement of water would be less than the expected rate of transpiration.

Wherein then can benefit to water-supply be derived from soil-storage which becomes so small within a few days after complete saturation of the soil-prism?

[Note: Owing to limited time Mr. Gross could not cover all the discussion in the above paper. However, he briefly summarized the material and discussed the paper by Hoyt and Troxell in some detail. He also showed that floods in California would not usually be increased because of the melting of snows by warm rains. He showed the enormous amount of rain required to melt snow considering that the source of heat was solely from the rain. He made assumptions as to the temperature of snow and rain which are more extreme than would probably be met with under usual conditions.

The discussion of Mr. Gross' paper was delayed until the presentation of all the papers on the afternoon program.]

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WATERSHED-MANAGEMENT FROM THE VIEWPOINT OF THE FORESTER

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American foresters have been ardent propagandists and zealots in the cause of watershed-conservation. Their claims that forests and a mantle of vegetation ameliorate or prevent floods, accelerated erosion and silting of streams and reservoirs have been, however, seriously questioned. For 25 years this controversy has continued, the foresters being charged as extremists with no facts to support their philosophy on watershed-protection. These contentious discussions chiefly between engineers, meteorologists, and foresters have produced a voluminous literature; but in spite of this, floods and drouth continue to plague us.

Floods and drouth--one a result of a superabundance of meteoric waters and the other a lack of them--have again focused public attention on phenomena even as spectacularly recorded in Genesis in the first flood and in the seven dry years of Joseph's sojourn in Egypt. Whether such catastrophes are punitive measures of Divine Providence because of man's sin, whether man in his own divine wisdom can ameliorate or even prevent such disasters are questions propounded and argued by the learned professions of engineering, geology, ecology, and forestry, rather in the fashion of scholasticism of the Middle Ages determining how many angels could dance on the point of a needle.

Before discussing watershed-management, we must clearly define what we mean by watersheds. The speaker defines a watershed as a drainage-area which feeds a stream-course. Its physiographic character is an expression of the more violent geological processes of diastrophism through the ages, the less discernible and more subtle processes of erosion and gradation between upheavals of the Earth, and the dynamics of biological processes and climate.

We may profitably distinguish between those processes cosmic in character and those readily modified by man. We might place the major and normal geologic processes and climate in the first category and the biological processes in the second. The processes which influence the condition of a watershed or give it character cannot actually be segregated. They are closely interrelated; one process may initiate or critically modify the other. A watershed may well be considered as a concrete, dynamic, living entity, composed of a cover of vegetation rooted in and living on the soil, and the soil itself as much alive as the vegetation that it supports--all superimposed on a reasonably stable mass, the native bed-rock. Under the influence of climate and the supporting mass one watershed develops characteristics different from another. The fact that a watershed is a dynamic thing postulates a constant process of change. The vegetative cover is slowly and often imperceptibly being modified in ecological stages through a successional and progressive series of types to an ultimate climax. And even when the climax is reached, natural forces may destroy it and initiate again the original progressive ecological series, like the child's house built of cards, blown down by a gust of wind and again rebuilt for another blow-down. Just as the plant-life is constantly in a state of flux and change, so likewise is the soil that supports it. And in consequence of these changes there must inevitably be corresponding changes in the regimen of water, both on the surface and in the underground channels.

Let us explore a little further the thesis that a watershed is a biologically living and changing organism. In the last three decades the science of plant-ecology has built up a rich literature on the changes in plant-associations, the progressive series towards a climax and the retrogressive processes that occur as well. These include the desert and grassland types at one end of the scale and the humid forests of fir and spruce on the other. The task of foresters has been to recognize the climax and by cultural means to control the process so as to retain the desirable species which the natural ecological process would eliminate, or to reduce those which man finds less useful. Undisturbed, the climax types of vegetation persist through long periods of time because they are an expression of the climate and soil. They can withstand the shock of periodic wet and dry cycles and are thus to a degree independent of the normal fluctuations in climate. While the field has not been entirely or sufficiently explored quantitatively, it is known that plant-associations vary considerably in their water-requirement and their ability to form soil and hold it in place. In these respects vegetation may have a marked influence on the physiographic and hydrologic character of a watershed.

In the last two decades a new science has been in the making--soil-biology, which emphasizes two interdependent phenomena. First, a biologic complex permeates the soil, and secondly, this complex is important in creating soil-structure. Much as the vegetative cover can be considered a living and ever changing organism, so likewise can the soil in the light of these new investigations.

The rôle of the vegetative cover in soil-formation has in recent years been given greater significance. As one worker in this field has stated: "The forest-litter, for example, comprising the fallen leaves, wood, fruit, and flower parts, contains organic compounds which are in part transported downward by water, but the animal-life existing because of the plant-life plays no small part in speeding up these processes. Investigations have brought out the fact that certain invertebrates of fields and meadows annually mix the soil-layers and perforate them to a greater depth than the plow."

The soil cannot be considered apart from the life that exists within it and may thus be considered a living, changing organism. Man's interest in a watershed, per se, excluding the use of the land-area for growing crops or for other occupancy, lies in two directions, water that it yields and protection that it affords against the movement of soil--or erosion.

A good many have considered a watershed static, and its water-yield in quantity, rate, and quality dependent solely on the total amount of precipitation it receives. In this approach the influence of the vegetative cover in the water-cycle equation or the part it plays in forming or holding soil is disregarded.

Another group grants that the surface-features of a watershed are not stable, that through cycles and epicycles of aggradation and degradation the Earth's surface tends to approach a level plain. This group asserts that water, ice and wind, the meteorological factors, are largely the basis of change in the character of a watershed. No distinction is made between normal speed of erosion and abnormal or accelerated rates. The character of stream-flow, flood-peaks, or low-stage flows, are under this theory largely dependent on geological and meteorological processes, and in no wise much affected by the living nature of a watershed. I have tried to adduce in contrast that a watershed must be considered as a living organism.

A watershed might also be described as a factory in which active forces and agencies, sunlight, protoplasm, chlorophyll, and water, manufacture cellulose and by-products or convert energy from one form to another. Water, the most important commodity, humanly speaking, and one of the active agencies in producing cellulose, undergoes in this process many changes. In its initial condition it may reach the ground and vegetation as rain or snow. As vapor part of it rises from the surface or is transpired by the plant-life. Part sinks into the soil as a reserve supply for later plant-use and only a fractional part is returned to the drainage-channels as flowing springs and streams. Of all the ingredients utilized in the production of cellulose, water and carbon dioxide go through a distinct and extraordinary cycle in that they leave the watershed, and return to it, through outside forces. Thus man can remove surplus water from a watershed with the assurance that other water will return. Other constituent ingredients of plant-life if removed, must be built from material contained within the watershed itself.

Here we may pause a moment to define the functions of the forester and the hydraulic engineer in watershed-management. The forester's function is to develop and protect the natural forces at work in the watershed-factory. The task of the engineer is to harness the excess free-flowing energy which appears as surplus water in the channels, capturing and converting it for the use of man. The part that vegetation plays in the watershed-factory, although it is of prime importance, has frequently been disregarded. All animal-life on the surface and beneath the mantle of vegetation within a watershed depends directly or indirectly on vegetation. This mantle markedly influences the amount of water absorbed and retained by the soil, the time and rapidity of runoff, the silt-content of streams, and the velocity and turbidity of the water itself.

While these relationships have been but partially explored and much remains unknown, a body of experimental evidence is available which indicates significant qualitative values and trends. I shall briefly summarize the findings of some recent studies which help to clarify the rôle of vegetation in the water-cycle.

(1) A forest-canopy through interception of precipitation retards water reaching the surface and much that is retarded may be directly evaporated into the air.

(2) The mantle of trees, woody shrubs, herbs, grasses, and even the litter, greatly reduces the rate of evaporation from the soil.

(3) Consumption of water by forest-vegetation incorporated in its cell-structure or transpired functionally, frequently reaches a high per cent of the total precipitation.

(4) More important is the fact that water-percolation into the soil-mass is materially promoted by the litter-and-humus cover, produced only through a mantle of vegetation. I want to refer particularly to the experiments conducted here on the Berkeley Campus on this phase of the problem. Although it was a simple experiment, it was an important contribution to our knowledge. Lowdermilk, Shaw, and others working together showed conclusively that litter functions in two important ways to promote percolation, independently of the kind and character of the vegetative mantle. First, rain-water striking a litter-covered soil is filtered by it and passes into the soil always clear; striking a bare soil it rapidly muddies. Secondly, a soil-mass can absorb unlimited quantities of water through percolation if clear water is applied and conversely muddy water limits percolation.

(5) The absorptive capacity of the litter in itself retards large quantities of immediate surface-runoff.

(6) A forest or other vegetative cover profoundly influences the amount and character of erosion originating on a watershed. This is largely brought about by the litter and humus developed and deposited by the vegetation.

Insofar as a mantle of vegetation induces percolation and reduces immediate surface-runoff, peak-flows and floods are definitely modified and materially reduced in intensities. As Munns has stated, accelerated erosion through the development of gully-streams increases the flashiness of runoff from the margin to the center of a drainage-basin. This has the effect of congesting major valleys with flood-waves of abnormal height and abrupt frontal slope. The water-carrying capacity of alluvial channels leading out of areas of accelerated erosion is decreased by accumulations of sediment, due to the overloading of the stream by abnormal soil-wastes from headwater-areas. As flood-waters increase, their sediments clog water-spreading grounds and reduce natural replenishment of underground waters, tending in time to lower the water-table. This phase is particularly important in the West where pumping from underground water-basins forms the cheapest source of local water-supply.

I have attempted to sketch briefly the major results and inferences from recent experiments which indicate the significance of plant-life in maintaining certain favorable conditions in a watershed by affecting the regimen of streams and silt-deposition. We might ask the question, how the water-cycle functions in a watershed under primeval conditions? While this may be pure speculation, the nature of the vegetation- and soil-mantle that clothed the virgin forest and range, the character of stream-flow and erosion therefrom can be safely judged by evidence found on vestiges of primitive areas that have escaped depletion and also from other evidences of geologic character. Bailey as a geologist describes this as follows: "Under primeval conditions we find a stable and porous soil-mantle built up by the age-old process of soil-building and normal erosion, which progresses with the slowness of geologic time and has throughout milleniums sculptured and molded the face of the Earth. The soil of the mountain slopes and the alluvium of the valley-floors have been produced in this way--even the rocks of which most mountains are composed have been formed of sediments which are products of older periods of erosion and deposition. But the principal method of transportation of the weathered-material from the slopes was by natural gravity creep rather than by stripping and gullying by water. The creep of the soil was rarely rapid enough to disturb plant-populations or to modify their general aspect. Surface-runoff caused a minimum of silt, destructive floods were unknown on many areas and uncommon on most others. Streams were generally clear, receiving what silt-load they carried from the bottom of channels rather than from the vegetated slopes or protected stream-banks. Ordinarily erosion progressed so slowly that soil was formed or accumulated more rapidly than it was removed. Only under unusual conditions, as in Boyce Canyon on certain shales in Colorado and Utah, on the badlands of Arizona, etc., have adverse climatic and geologic conditions prevented the fixing of the land-surfaces by soil-formation and plant-growth. In these few instances runoff has always been rapid, erosion pronounced. Elsewhere soil and vegetative cover were sustained by virtue of a delicate balance between constructive and destructive forces. Vegetation was invariably the deciding factor in the balance. The presence of a natural plant-cover enabled the constructive forces to hold sway and to preserve watershed values."

Even under primeval conditions vegetation is never stable. Nature in her processes of progressive change sometimes applies destructive forces that arrest some phase of succession, but immediately she proceeds to heal the scars and starts again her inevitable cycle of plant-succession. This has been going on for eons. The natural recuperation that takes place on a watershed under these normal processes is materially modified and disturbed when man occupies the land. He starts a greatly accelerated chain of events. He begins processes far more destructive and at a more rapid pace than nature herself pursues in her destructive moods. The axe, the plow, the torch, and more recently the giant blades of bulldozer and dredge, have changed completely and often destroyed the capacity of a watershed to function favorably as an energy-producing factory for the use of man. Under primeval conditions, abrupt and severe changes of the environment had no direct significance to man, and time was an immaterial denominator. Once man occupies the land-areas and builds a civilization thereon, every violent disruption of a watershed brings with it impelling consequences.

Let us briefly examine how man's occupancy of the West has affected forest, woodland, and range on the more important watersheds. The activities of man which affect vegetation unfavorably may be listed as follows: (1) Over use of the ranges by domestic livestock; (2) uncontrolled fires on forest, woodland, and range; (3) smelter-fumes and dredging; (4) destructive logging; (5) unregulated hill agriculture.

The range--Misuse of the 729,000 acres of the western range west of the 100th meridian has been responsible for depletion of plant-cover, a reduced stand density of plant-life, and elimination of many of the more valuable perennials, which in turn have markedly changed the conditions of the watersheds. In a Utah experiment, plots of ten acres each on a range show that where 16 per cent of the soil was covered by plants, surface-runoff was 10.33 per cent of the precipitation; whereas plots with 40 per cent of the soil covered by plants had only 5.49 per cent runoff. The thinly stocked plots showed accumulation of 133.8 cubic feet of sediment; whereas the densely stocked plots showed only 7.7 cubic feet.

Another experiment where artificial rains were applied to plots showed that areas well stocked with bunch-grass--which has the greatest forage value--yielded very little runoff or erosion. The areas with downy-chess, and needle-grass lupine, which succeeds bunch-grass on over-grazed ranges, showed on the other hand 25 and 47 per cent of the precipitation going off as immediate surface-runoff. The bunch-grass showed no erosion, the downy-chess showed 2000 pounds, and the needle-grass lupine showed 4700 pounds per acre during the course of the experiment. This experiment permits the inference that a climax type of vegetation for a region possesses the highest inherent qualities for maximum percolation of precipitation into the soil-mass as well as the power for maximum stabilization of the soil-mantle.

(2) Uncontrolled fires--Fires have swept our forests, woodland, and range-lands since time immemorial. Lightning has always been a source of fires. The kindling of the forest-fires by prehistoric man, undoubtedly brought with it forest- and range-fires whether through accident or design. Fire-scarred tissues in the Sequoias date back to the time of Columbus. Some of our important forest-types are an expression of past fires. Historical evidence and healed-over fire-scars in our trees indicate that during the past 80 years fires have increased in number and intensity in the western United States. There are many reasons why fires must now be controlled, but I shall confine my discussion to the relationship of fire to watershed-conservation.

Fire is the most destructive disturbance of the forest-cover. Even the lightest fire consumes part of the litter, although it may not damage standing trees. Intense fires may destroy all the vegetation, the litter, and the humus, as well as the faunal and bacterial life in the forest-floor. Considerable experimental evidence is available regarding the effect of burned and unburned areas on runoff and erosion.

The laboratory-experiments on this Campus, in which artificial and natural rains were applied to litter-burned and litter-intact plots, have already been reported. The burning of the litter, regardless of the soil-type, consistently gave higher runoff, inversely decreased percolation, and gave high erosion-values.

In the pine-region of the Sierras, a five-year record of runoff and erosion from repeatedly burned and comparable unburned plots snow a yearly runoff from the burned area ranging from 31 to 463 times that from the unburned, and an annual erosion from 22 to 3000 times that from the unburned. Identical-plot experiments in southern chaparral gave even wider variations between burned and virgin areas.

(3) Destructive logging--Clear cutting of forests when followed by severe slash fires undoubtedly approximates the same conditions of a watershed as described for severe fire. Except for the Wagon Wheel Gap Experiment in Colorado, no experimental data are available to show the effect of different methods of cutting on runoff and erosion. The Wagon Wheel Gap Experiment merely changed a mixed spruce-aspen type to an aspen-coppice, but even this snowed a change in the regimen of the stream and increase in silt-load on the cut-over areas until complete cover was re-established.

(4) Smelters--Fumes from smelters and other industrial plants may injure or completely destroy vegetation. Spectacular areas in the United States where smelter-fumes have destroyed vegetation are to be seen at Ducktown, Tennessee, Kennett and Bully Hill, California, Anaconda and Butte, Montana.

At Ducktown, an area of 10 to 12 square miles is practically denuded except for occasional clumps of sage-grass and wild smilax. Potato Creek, originating in this drainage-area, has built up a flood-plain 100 to 300 yards wide, burying telephone-poles up to their cross-arms. At Kennett an area of 67,000 acres was completely denuded, and 86,000 acres partially. In 1920 Munns estimated a total of more than 35 million cubic yards of detritus had been removed and emptied into the Sacramento River. This loss still continues.

(5) Agriculture--Watersheds have been severely damaged by the injudicious attempt to cultivate hill-lands. The breaking of the sod on the eastern slope of the Continental Divide is now judged as a gross mistake. Generally such forms of agriculture ultimately defeat themselves, but in their wake follow the tragedy of broken homes and men. Various estimates have been made of the silt-load that poor husbandry brings down through channels, rivers, and on valley-floors. Even the more modest estimates are appalling.

Summarizing briefly, the major cosmic forces which shape and control geologic processes and produce climate, are beyond man's power either to guide or change. These basic and integral factors which give character to a watershed, while important, generally operate at a slow pace and have no appreciable effect on man's occupancy of the Earth's surface. In any event, man can do but little to arrest or change these unless another Joshua comes among us.

The mantle of vegetation that clothes a watershed, on the other hand, and the upper soil-layers beneath it can be controlled by human endeavor, can be changed or destroyed by accident or design through man's own activities. This mantle of vegetation may well be considered as the key within man's control for promoting and maintaining favorable watershed-conditions. Violent changes in this cover may bring disastrous consequences and even in short historic periods may produce conditions which can wipe out the substance of an entire civilization. As we reach the regions of semi-aridity the delicate balance between vegetation and soil-stability can be easily disturbed. The top soil, the most precious part of the Earth's surface, which has taken centuries to build, may be washed away in a few years. In our arid West we have not only the problem of preventing floods but of maintaining maximum usable water-yields from each watershed as the basic prerequisite for sustained agriculture and industrial life. What man has done to change the mantle of vegetation in the West since Marshall picked his first gold nugget at Coloma, is a sad commentary on unregulated exploitation. The real gold of the West lies in its top soil, in its well-clothed mountains, and its clear running streams. I want to add the word of Professor Barrows of Chicago who stated recently: "It is one thing to recognize the qualitative significance of certain factors in runoff and therefore in some floods, and a very different thing to generalize on its quantitative significance." I agree with him that it would be particularly misleading if we assumed that either deforestation or soil-erosion were solely responsible for major floods in the light of existing knowledge. His plea is that we must have a great deal more knowledge in hand before we can weigh accurately the forestry-measures that must be taken, the erosion abatement that must be provided for, and the engineering structures that must come with it as well. He notes that most engineers have had a predilection for costly works for flood-control and have given little regard to the other features of watershed-protection which I have attempted briefly to sketch.

Floods and muddy rivers are but a manifestation of a diseased watershed. There are other indicators, the condition of the vegetation reflects the health of a watershed well in advance of muddy rivers and floods.

In conclusion, I want to stress that an extraordinary opportunity is offered to scientists in answering some of the more complex riddles that still confront us in fully understanding the inherent processes of watersheds and the manner in which man must handle them. Forestry, after all, is an integration of many basic sciences and many exacting disciplines. Foresters will be able to do their tasks competently only with the help of the soil-scientist, the plant-physiologist, and the geologist. The door is wide open for important contributions from these fields.

California Forest and Range Experiment Station,
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SOIL CONSERVATION

Harry E. Reddick

(Abstract)

This was an informal paper and no manuscript was submitted for publication. The seriousness of erosion and the possible effectiveness of vegetative cover in lessening erosion were indicated. A film taken by the Soil Conservation Service showing erosion in the southwestern part of the United States was shown.

Soil Conservation Service,
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FOREST- AND RANGE-HYDROLOGY AS INFLUENCED BY LIVESTOCK

George H. Hart

The plant-cover on our uncultivated lands varies greatly under different elevations, soil-types, topography, climatic conditions, and so forth. It is logical, therefore, that we should expect variations in the method of handling surface-rights on these lands depending on local conditions and that generalization in a regulatory way must be done with care, to avoid more or less serious pitfalls.

So also must all interested groups recognize that this cover will be more or less influenced from the impacts made upon it by animal-life, both vertebrate and invertebrate, wild and domestic. V. E. Shelford [Ecology, v. 12, pp. 455-467, 1931] has attacked the fundamental concepts of some plant-ecologists who apparently consider and name the vegetation apart from the animals. These investigators have desired to learn the ultimate climax of vegetation with the presence of wild animals or with equivalent domestic animals excluded by fencing rather than reduced to the early or original numbers.

The purpose of Shelford's paper was to restate the unity of the plant-animal community and suggest principles that make such a concept tenable, and expressed by him as follows: "That a much broader and more flexible view of controlling factors is tenable than the one which holds that the animals are merely an environmental factor acting upon plants. That food-relations, especially of abundant and influent animals, are usually flexible and rarely if every obligate; and that observation of apparently restricted food-relations made in one locality may not hold good under other conditions. That the climaxes of nature (bioecological climax) include that vegetation which occurs with the pristine numbers and kinds of animals present."

Regarding the bioecological climaxes, Shelford claims that if bison held some of the mixed prairie to the short-grass stage, then short grass was the bioecological climax even though the climax with bison excluded would have been quite different. The discussion of uncultivated land uses with animal-life excluded would be comparable to discussing large cities with people excluded.

The multiple-use method of administering the publicly owned lands seems to be meeting with greater and greater acceptance and under such a program grazing would be expected to assume first importance in certain areas and under certain conditions just as silviculture, recreation, water-conservation, and other uses would each be accorded first place under others. In California and Oregon different conditions exist than in much of the Rocky Mountain Area. Let us first get some idea of the enormous areas involved. The data of Table 1 was adapted by P. J. Webster from the

California Department of Public Works, Division of Water Resources [Bull. 35, Plate II, p. 18], and gives the acreage in major land-uses in the State.

Table 1--Major land-uses in California, 1930

Land-use		Approximate area acres
Timber		16,200,000
Brush and woodland		16,600,000
Grass		27,500,000
Desert and sagebrush		24,900,000
Crop-land		8,400,000
Irrigated	3,500,000	
Non-irrigated	4,900,000	
Urban area		1,200,000
Miscellaneous		4,700,000
	Total	99,500,000

In round figures the approximate acreage of Federal controlled land within the State is divided as in Table 2.

Table 2--Federal controlled land, California

Purpose	Approximate area acres
Public domain	16,600,000
National forests	19,000,000
National parks and monuments	1,200,000
Indian reservations	600,000
Game-and-bird refuges	40,000
Oil-and-shale reserves	70,000
Total	37,510,000

From Tables 1 and 2 it will be seen that only a small percentage of the land-area in the State is cropland producing all of California's many and much-publicized agricultural crops. Also that a very considerable percentage of the non-cultivated land is in Government ownership and this has important bearing on commensurable lands adjacent thereto and held under private patent. In many cases these areas held under private patent are so located and handled that they constitute winter ranges for the owners' livestock. In some locations they consist largely of meadow-lands from which hay is harvested in summer and stored for winter feeding. During the summer the livestock must go on public land for grazing in order that the private holdings will constitute an economical working unit. Without the additional summer feed the private holdings in many cases would have to be vacated.

California a deficient livestock-producing area

With the rapid increase in population California is becoming more and more deficient in livestock-production to meet demands of consumers. Even though we export 250,000 to 500,000 lambs in the spring for the early eastern market we import heavily during the remainder of the year so that we are a net importing-sheep State to the extent of over 1,000,000 head annually.

We have always been a border-land territory for production of hogs and at present are importing over 1,000,000 head annually either as live hogs for slaughter or as cured-pork products.

In some years a few fat cattle are shipped east at the end of our green-grass season but always large numbers of stocker- and feeder-cattle and fat animals for immediate slaughter are shipped in so that we are net importing in beef cattle to the extent of 750,000 head annually. This is in addition to the 30,000 head of dairy cattle yearly entering the State and mainly going to the Los Angeles milk-shed area. Imports of even poultry for meat purposes are ahead of exports despite our large exportation of eggs.

This large demand for meat is fortunate because it keeps in line with and utilizes the enormous available tonnage of by-products of industry and agriculture which are produced here or imported from the Pacific Area in ever-increasing quantities. These include beet-pulp, cotton-seed-meal, linseed-meal, fish-meal, molasses, coconut-meal, orange-pulp, and many others.

Some of these are particularly favorable in their chemical composition and physical qualities for supplementing the dry feed on the range and render this much more efficient for maintaining herds and flocks. Thus livestock are a service to industry by consuming by-products and are a very important creator of wealth. Natural vegetation on which they subsist to a considerable extent thus becomes one of the most important crops in the State. Individual acres make small return but there are so many of them that in the aggregate the return is very large.

Federal policy

Under the act of Congress approved April 10, 1930, President Hoover appointed a Committee on the Conservation and Administration of the Public Domain. This Committee made a report to the President of the United States in January, 1931, and conclusions 3 and 4 read as follows: "(3) That the remaining areas, which are valuable chiefly for the production of forage and can be effectively conserved and administered by the States containing them, should be granted to the States which will accept them. (4) That in states not accepting such a grant of the public domain responsible administration or regulation should be provided."

During the depression considerable public sentiment developed among livestock interests in the middle west to the effect that allowing the grazing of livestock on Federal forest-lands and public domain constituted a Government subsidy to the industry in the public-land States which was unfair competition to the corn-belt and should therefore be discontinued.

Such sentiments from owners of livestock were very joyfully received by certain groups of conservationists. It has been the policy of the Department of Interior in recent years to exclude livestock on National Park and Monument lands, and these now constitute in California an area of 1,200,000 acres, where the effect of this policy can be studied.

With the advent of the present administration nothing more was heard from Washington regarding turning back public domain to the States. A rather extended controversy arose between the Departments of Interior and Agriculture regarding jurisdiction, conservation, handling public lands, extending National Park Areas by encroachment on forest-lands, increasing public-land holdings, etc., which is still continuing at the present time.

Out of it has come the Taylor Grazing Act of 1934 which provides for the Interior Department regulating livestock on certain newly delineated grazing districts constituting part of the public domain where large tracts of it are contiguous.

The enormous national area known as the public domain comprises about 179,000,000 acres. Prior to the passage of the Taylor Grazing Act practically no regulation had ever been maintained over it. It was only to be expected that under a free-for-all, first-come first-served, policy, some marked abuses occurred. This was therefore very much needed and welcomed legislation even though its administration has met with some opposition by those who had long used the lands for livestock grazing without any restriction.

In California we have two areas under the Taylor Grazing Act known as Mojave District No. 1 and the Modoc Lassen District No. 2. The method of administering these grazing districts under this act is quite different from that pertaining in the National Forests. In the latter the carrying capacity is ascertained by officials and then permits are issued usually from year to year for the allotted number of cattle or sheep and the dates when they shall be taken in and out. Under the Taylor Grazing Act local boards are appointed for each grazing district consisting of livestock men with commensurable property to advise the official administrators of the act regarding carrying capacity, grazing rights, water-development, fees, and so forth.

In 1936 Senate Document No. 199 entitled "The Western Range" prepared by the United States Forest Service was published. On pages seven and eight E. H. Clapp makes the following statement: "As might be expected, both ownership, and the form of control within ownership, have had a marked influence on depletion. The Federal public domain, a no-man's land without management prior to the creation of the grazing districts, is in the worst condition, with depletion of 67 per cent. Very surprisingly, fee-simple private ownership has been so little of an incentive to the preservation of the range-resource that depletion stands at 51 per cent. Indian, State, and county holdings have fared no better than private lands. National-forest ranges make the best showing, but despite 30 years' management are still 30 per cent below virgin conditions.

"Whether range-conditions are on the up or down grade may be even more significant than the extent of present depletion. Here also the public domain has the blackest record, with nearly 95 per cent of the total area depreciating during the last 30 years and only two per cent improving. Over three-fourths of the national forest-range has improved during the same period, and only five per cent has declined. For all other ownerships, largely private lands, from 75 to 88 per cent have declined and seven to ten per cent have improved in value. Of all classes of ownership and forms of control only the national forests show any appreciable gain in range-conditions."

The publication builds up evidence to show that the National Forest Service of all agencies public or private alone knows how to properly administer range-lands and then makes a bid for extremely large additional territory to be brought under their supervision. Thus on page 446, S. B. Show under the title "The probable future use and ownership of range-lands" makes the following statement: "So, with present very approximate knowledge, and with a very large program of public range-land acquisition indicated as desirable, a minimum of 125 million acres appears to be a conservative initial program. Critical needs for public ownership of submarginal range- and crop-lands, for watershed- and wildlife-protection, and for effective Federal range-land administration will doubtless be reasonably well met by such a program."

Thus in a five-year period we have seen a complete change of Governmental policy from one of turning back public domain to the States to one of keeping all they have and acquiring millions of acres more.

Conditions in California

Now let us come down to facts in the case and here it will be necessary for me to confine my facts to California to which area my personal experience has been limited.

From Table 3 it will be seen that after 25 years of Federal supervision the National Forests of this State graze a less number of animals as well as a lower percentage of the total animals in the State than was the case in the beginning of their management in 1909.

In certain areas this is much more serious than in others. Thus in Tehama County in 1917 there were 140,000 sheep owned by 66 operators in the National Forest while in 1937 this number was reduced to 27,000 head and 22 operators.

These figures present only partial evidence of what is actually going on. It used to be that lambs came out of the mountains fat, weighing from 80 to 90 pounds and could go direct to slaughter. In recent years the lambs from the same areas have all too frequently come out thin, weighing 60 to 75 pounds, which required them to be sold as feeders at a reduced price and in some cases thereby removing all profit from the operations.

Vegetative cover

There are in general three types of vegetative cover on forest-lands in this State, namely, trees, brush, and grass. All cover is, of course, maintained at the expense of soil-moisture but the amount of moisture lost and the degree of protection from erosion varies with the different cover-types. The trees and grass-cover all admit have usefulness; while this is not admitted by all in the case of brush, despite the fact that brush has been encroaching on greater and greater areas of forest-land and contiguous private lands under present administrative policies.

In an article on relation of grass-cover to erosion-control [J. Amer. Soc. Agron., March, 1935], H. H. Bennett, Director, Soil Erosion Service, makes the following statement: "Much has been said and written about the powerful effect of forests, in controlling erosion and increasing absorption of rainfall, but not nearly enough attention has been devoted to the similar and almost equal effect of grass as a stabilizer of land and as an effective means of increasing absorption. The importance of grass as a means of controlling erosion is so great that this paper may appropriately be prefaced with the assertion that where there is a good cover of grass there is no serious problem of erosion. For this reason it seems time for agronomists and all of those who are interested in the continuing welfare of the crop- and grazing-lands of the nation to think more of ways and means for increasing the use of grass, and those other thick-growing crops that function after the manner of grass, to the end that by this simple procedure more of the water may be retained where it falls, with less of it running rapidly into the streams, and with more of the soil held in sloping fields where it belongs."

In California we have a zoning system of range-lands for fire-protection with three zones established. Zone 1 constitutes tree areas where all fires are immediately suppressed. Zone 2

Table 3--Livestock grazed in the National Forests compared with total livestock in State of California, 1909-1934

Year	Grazed in National Forests[1]		Number in State		Per cent of state total grazed in National Forests	
	Cattle and horses	Sheep and goats	Cattle and horses	Sheep and goats	Cattle and horses	Sheep and goats
	1,000 x	1,000 x	1,000 x	1,000 x	per cent	per cent
1909	169	353	2,080	2,325	8.1	15.2
1910	171	398	2,580	2,563	6.6	15.5
1911	161	382	2,508	2,683	6.4	14.2
1912	180	445	2,495	2,725	7.2	16.3
1913	182	449	2,541	2,603	7.2	17.2
1914	188	433	2,517	2,624	7.5	16.5
1915	187	398	2,609	2,500	7.2	15.9
1916	194	417	2,661	2,505	7.3	16.6
1917	205	466	2,694	2,524	7.6	18.5
1918	223	582	2,705	2,832	8.2	20.6
1919	216	620	2,581	2,943	8.4	21.1
1920	209	529	2,473	3,022	8.5	17.5
1921	217	554	2,435	2,574	8.9	21.5
1922	207	509	2,469	2,369	8.1	21.5
1923	200	469	2,477	2,459	8.1	19.1
1924	199	474	2,536	2,618	7.8	18.1
1925	170	426	2,360	3,103	7.2	13.7
1926	170	417	2,326	3,256	7.3	13.1
1927	162	444	2,372	3,444	6.8	12.9
1928	159	444	2,381	3,573	6.7	12.4
1929	156	428	2,339	3,888	6.7	11.0
1930	154	416	2,234	3,490	6.9	11.9
1931	152	404	2,253	3,405	6.7	11.9
1932	153	388	2,154	3,235	7.1	12.0
1933	147	371	2,143	3,075	6.9	12.1
1934	147	358	2,194	2,918	6.7	12.3

[1] From annual reports of the United States Forest Service, District 5.

consists of lands adjacent to the tree-areas in Zone 1 and here fires have also been suppressed for fear of spreading into Zone 1. Zone 3 constitutes lands sufficiently removed from tree-areas for them to be considered safe to burn brush under properly controlled conditions after a permit has been secured by the owner from the State Board of Forestry. There has been considerable controversy on the zoning and particularly the liability involved in securing a permit for burning in Zone 3. This year the State Board of Forestry at the request of the livestock-interests has modified the methods of issuing burning permits and arranged for State Foresters to cooperate with land-owners in Zones 2 and 3 in burning brush-lands.

In the Karoo District of South Africa with soils consisting of shales, sandstones, and conglomerates rich in vertebrate remains, the plants consisting to a considerable extent of browse are practically all palatable and nutritious species. This explains the large herds of wild animals found in this Area.

In contradistinction the brush-cover in California hills are relatively unpalatable and grow to such density that livestock cannot move through them. Even deer are driven from such areas to find feed in more favorable localities and in some cases this includes alfalfa-fields. On the Stover and McKenzie winter range east of Red Bluff it is conservatively estimated that they carry 4000 deer because they adjoin a game-refuge at a higher elevation which becomes snow-covered in winter and the deer move out to the lower levels with the close of the hunting season and the approach of winter. This greatly reduces the livestock-carrying capacity. In general as the result of brush-encroachment we have been gradually concentrating our domestic animals and deer on a smaller and smaller acreage and thus leaving millions of acres of the brush-covered lands waste as a constantly increasing fire-menace.

Fire

The prevention and control of fire are unsolved problems. There is good evidence to advance the hypothesis that the only means of preventing fire-holocausts is by the judicious use of fire.

In Conservation for September-October, 1937, published by the American Forestry Association, J. H. Curry has prepared an article under the title "The future of fire-control" from which the following excerpts are taken: "The emergency-aspects of fire-control loom large. The ever-present possibilities of disaster tend to confine fire-control thinking to matters of the moment, the day, and the season. Seldom do foresters stand off to consider this problem in its broader aspects.

"There seems to be a hope that fire in America will eventually reach the minor status which it has always held in the managed forests of Europe. This line of reasoning I hold to be wholly fallacious.

"Forestry's present store of information and accepted skills and techniques in fire-control are meager. Consequently, the young forester finds himself ill-prepared for the job which often consumes the greater part of his efforts. The difficulty seems to be principally a matter of organization. Fire-control cannot complain of neglect in the relative distribution of funds.

"Other important phases of forest-administration, such as grazing, forest-management, and engineering, each have their specialists, while the most important job of all is administered directly by the supervisor, necessarily a man whose attention can not long dwell on a single activity.

"Research is urgently needed to provide better basic information for the foundation of fire-control work. The research-men assigned to this work are few in number and their attempts to specialize in any one phase of the problem frequently meet with disfavor."

The California Agricultural Experiment Station has a group studying the subject which was inaugurated at the urgent request of livestock-interests in northern California. The problems involved are so controversial and the variables so numerous that progress has been slow and studies largely confined to one small area in Shasta County. That increased ground-feed supply will be secured for three or four years following brush-burning seems to have become quite generally accepted.

Whether brush-cover can be returned to grass-cover by repeated burning and if so, whether or not too much erosion will occur in the interim to make the procedure practical are still unsolved problems that undoubtedly differ in different localities.

Grass-cover

In this State today grass-cover consists largely of annuals. At the time gold was discovered, the floor of the great central valley was covered with perennial bunch-grasses which have largely been destroyed by the plow. How far up the foothills they extended where overgrazing may have destroyed them is not known. However, the annual plants contain very good, palatable, and nutritious species and the limiting factor today is moisture rather than species. So-called overgrazing must be looked upon differently on ranges with annual cover as compared to those with perennial cover.

The annual cover in this State sprouts with the fall rains. In the very early green stages it is high in moisture but contains very important secondary dietary essentials including vitamins and minerals. In the short green stages it is very nutritious containing a high percentage of available nitrogen. We say it is a watered concentrate because although high in moisture the dry matter has the chemical composition of concentrates rather than roughage. It then more or less rapidly goes into the bloom and flowering stages followed by seed-formation. In maturing the seeds withdraw from the leaves and stems important nutrients. When their development is complete the seeds shatter and the plants dry up and die. During the remainder of the season the plants are bleached by the Sun and leached by the rains until they become a very poor roughage lacking in certain essential nutrients, notably protein, phosphorus, and vitamin A. Even though grazing renders the ground bare of foliage, full provision is made for the next generation by the seeds which shatter in the early summer, and which are unavailable to livestock except in the case of burs of bur-clover (Medicago hispida). These are ordinarily so numerous that their consumption does not under ordinary circumstances become so complete as to affect reseeding. In case the dry forage is not grazed or removed and too dense a cover is left, the flora will be changed and the range reduced in value. This is because the most valuable feed-plants, bur-clover and alfilaria (Erodium species), grow close to the ground in rosettes and will be shaded out by the dry cover and tall-growing species such as wild oats (Avena species), the brome-grasses(Bromus species), foxtail (Hordium murinum), and others. Thus, so far as livestock are concerned, an annual plant-range can be adversely affected by undergrazing and relatively unharmed by apparent overgrazing. How well the root-system of a dead annual plant holds soil from eroding as compared to the root-system of a living perennial one is a matter of conjecture. So also would be the method of procedure to get the perennial grass-cover established no matter how desirable it might prove to be.

Wild life

A discussion of this kind would not be complete without a few words regarding wild animals because they are in some cases directly responsible for some things that tend to be charged against domestic animals. I particularly refer in this respect to rodents and the most important species are pocket-gophers, ground-squirrels, kangaroo-rats, cottontails, and jackrabbits.

There is a fluctuation in the population of these species of rodents but the total of the five groups per acre at any given time is large although exact figures are not available. They have selective feeding habits on the plant-cover, both crown and roots, in the growing stage and also after seeds have shattered. In some cases seeds are accumulated in large quantities in their burrows. It has been estimated that a ground-squirrel will eat or destroy approximately one-half pound per day of feed during the season when it is active and the other species in proportion to their size.

Studies are being made on some of these questions regarding rodents at the San Joaquin Experimental Range of the United States Forest Service in Madera County with specially constructed enclosures on which a known population of single species may be maintained.

Conclusions

Livestock and game-animals constitute very important means of realizing the great potential value of our natural vegetation on uncultivated lands.

Domestic animals should be fitted into the forest-and-range picture on public lands to the best advantage of all factors concerned. Except in local recreational areas, reservoir-sites, very high altitudes and so forth, their elimination will not be to the best interest of the State and Nation.

Rodents are a factor of as great or even greater importance than domestic animals on plant-cover in some cases.

Feed for animal-life can be increased by controlling brush-cover; and such control tends to decrease the fire-hazard.

In studying methods of accomplishing control of brush-cover the judicious use of fire seems to be worthy of careful consideration.

The possibility of changing brush-cover on lands to grass-cover and the effect on erosion during and subsequent to the change if it can be accomplished need investigation.

The reduced cost of fire-prevention and fire-control under such a system, would justify the studies in addition to the manifest advantage to live-stock and game-animals.

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FLOOD-CONTROL

Leonard B. Chambers

The author could not be present and did not submit an abstract or manuscript for publication.

Corps of Engineers, U. S. A.,
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DISCUSSION

Chairman Adams called upon R. L. Wing of the Division of Water Resources, Department of Public Works, State of California, for a statement as to the policy in his organization as to whether vegetative cover on the watersheds was taken into consideration in designing spillways for dams.

R. L. WING (State Department of Public Works, Sacramento, California)--I have handled many of the studies on determining the sizes of the spillways and the sizes of the dams. We have used largely average conditions. Where there was lack of such data we used stream-flow records and estimated our discharges from those. We make or estimate without any particular relation to whether the forest-cover had been burned or not. We are looking for average conditions rather than some particular condition and we would rather have some idea what the major floods which occur with extremely rare frequency may be. Precipitation would be so intense sometimes that no matter what the soil-conditions and vegetation may be we, probably, would have floods because the watersheds are completely saturated, whether in forest or not.

J. W. GROSS (Consulting Civil Engineer, Sacramento, California)--I closed my discussion without a conclusion and I should like to make it now. The point that I would wish to stress at this particular time is that same point which Mr. Wing brought forth in his discussion as to the method and condition which the State Department of Public Works used in determining the maximum spillway-capacitios for floods. In other words, what he said was this: That at the time of the flood-period, that is any major flood say of a frequency of ten years or more, it is the conception that whatever the forest-cover may be, the precipitation will be at such a rate that it cannot be mechanically absorbed or retarded by any of the works of man or any of the growths we find on the hills.

If precipitation still occurs and the voids in the soil are completely filled, we may possibly acquire as much penetration as the voids can hold, assuming that air is excluded. Therefore, in consideration of the alleviating effect of soil on flood-runoff, we must consider the amount of water which the soil can take up at the time when heavy precipitation occurs. Let us assume for western conditions that floods come at the usual winter period. Certainly our experience is that they do not come with the first rains. Therefore, the soil in the mountain area is certainly not in a condition where it would absorb much water when intense rains occur after the soil-voids are filled. Only a small proportion of the precipitation could get into the soil and thereby retard the runoff materially.

H. I. SNIDER (U. S. Forest Service, Truckee, California)--During that heavy rain of December 9, 10, and 11, 1937, we had occasion to go out during and after the storm to see whether our roads were being washed down the river and it was very noticeable that on the north slopes where the snow was in good condition the runoff was very moderate. There was practically no superfluous runoff, more or less what you would see during a normal rainfall; while on the south slopes where the rain and sun had softened the snow, the runoff was tremendous, almost three times the amount.

Another thing contributing to the south-slope runoff was the fact that in many spots the snow was entirely gone and with the night's freezing prior to the rain the ground was left partially frozen on the surface. When the rain first began to fall it ran off like rain off a pavement. That did not last because the higher temperature melted the ground somewhat at the same time. But the condition of the snow and ground has a good deal to do with the amount of the runoff; and the fact that the snow covering the ground on the north was more or less open at that time and the ground underneath the snow was unfrozen tended to restrict it. Where the pack of snow was constant and thick enough to absorb water, it allowed the ground to take in water better than on the south slopes where the ground was frozen.

During the season two records were broken in our area. Both inside and outside the Forest there were no fires beyond grass and all fires were kept under ten acres. The other record was the recent storm, the greatest obtained from all local Weather Bureau records and old timers' memories, for a warm rain of that quantity and that time of year. While we had very high water, our successful efforts in protecting the watershed from great burns reduced this potential area of flood, for the runoff is considerably greater where there are burns.

E. J. KRAEBEL (U. S. Forest Service, Berkeley, California)--Mr. Gross has said that when rainfall comes at a rate of six inches per day, the soil is incapable of receiving that amount of rainfall at such a rate. That is, the rain comes faster than the soil can take it, even though the soil is empty of water. Essentially that is right. There is a difference, though, if the soil is bare. This is a mountainside of bare soil and may be shown diagrammatically.

When rain comes with considerable impact and in great volume, the first thing that happens on a bare soil is that the soil-surface becomes puddled. You can see people in gardens who puddle the surface and promptly seal the pores of the soil so that instead of having the penetration of which the soil is capable, a maximum or even a fairly good average, you get almost no penetration after a few minutes of driving rain.

Suppose that soil to be covered with forest-litter or densely covered with vegetation or even protected by trees. The forest-litter may be anything from a few needles lying loosely on the surface to a depth of a few inches. The openings in the forest-floor are pretty well covered with creosote-bush or clover or other vegetation. You all know that in forest-regions almost every inch of forest-floor is covered with something and beneath the shrubs lie accumulations of litter of twigs, etc. Therefore, the rain does not strike bare soil but the vegetation intercepts the rain and holds it back. The water enters the soil clear and you get a greatly increased penetration at a greatly increased rate for a long-continued time. The soil does not cease to absorb water even when saturated.

The water is in a continual state of flow downward. It always leaves an opportunity for more water to enter. I never have seen a soil-mantle lying on a completely impervious layer. Since all rock is fractured in California, particularly in the southern parts of the State, it is weathered to a depth of 50 and 60 feet in some places. Vegetation actually grows on these rocks which are so porous that roots have grown into them. Many of the soils in the Sierra Nevada are of igneous origin. In the decomposed granites of the South there is an almost unlimited capacity to receive water and they do receive it because we have springs throughout the Sierra Nevada.

There are two kinds of seepage--one, a very quick seepage coming out near the point of entry on the high mountain surface. And again, there is one which is slow, controlled, and regulated. There is another seepage which enters the soil anywhere on the slope and comes out only close to a stream-channel so that rivulets form very quickly in a prolonged storm. These rivulets are kept alive by quick seepage for one or two days, sometimes a week or more, and so feed the water which would run off incidentally in a matter of minutes and hours in totally bare soil. This process has been demonstrated by Nature, not by experiment, in southern California time and time again. It is the thing that explains our sudden mud-rock flows and severe damage that occurs. It is the kind of thing that happened during the Montrose-La Crescenta flood of 1934. It was first explained by the newspapers as having been caused by a cloudburst. It presupposes a downpour beyond the normal rates over a rather limited area. The storm extended over an area at least 60 miles long and 20 miles wide, and it is hardly to be called a cloudburst under those conditions; and yet on only one little area did it produce runoff at such a rate to cause a flood, and that was on the recently burned freshly denuded Montrose area. The puddling of the soil-surface was due to the fact that there was nothing there, no litter, no forest, no cover, no vegetation of any sort to delay runoff and compel it to enter into the soil as clear water.

This is a question, not an explanation. I gathered from the previous remarks that the recent storm, the recent runoff that brought Kings River from 400 second feet to 80,000 overnight, was not caused by the melting of snow due to the warm rain but that it was due to heavy precipitation rather than melting snow. I had reports there was a pretty heavy snow-blanket in the Sierras before that storm, that it was a little warmer at that altitude than it ever had been, and that the melting of the snow would appreciably contribute to the runoff. If it were old snow, fairly frozen and long packed, it would not have melted so fast. I think in this case it would have melted because of warm rain and would have contributed very largely to the flood.

W. P. BOONE--As a farmer may I ask a question of these men? I take a 20-acre piece of land and run four inches on it within 24 hours to irrigate my alfalfa and in a few hours it has all disappeared. Am I to arrive at the conclusion that most of it must have evaporated?

E. A. COLEMAN--Suppose we take a series of sponges. We will apply water to the top of the sponge. You all know if you put a sponge in water and take it out, it will hold water. If we pour water on it at a certain rate and do not pour it on at such a rate that some of it will run off, we can continue to pour water through that series of sponges forever. The rate at which the water will pass through the sponges is determined by the size of the pores and other physical characteristics, and the rate of runoff is determined entirely by the difference between the rate of application of the water and the rate of flow of water through the sponge.

I do not see why we cannot consider the soil in the light of the same picture. Soil-pores are not microscopic but are considerably smaller than the pores of an ordinary sponge. The principle is exactly the same. We do not have a layer of soil with a concrete wall around it forming an enclosed plot. We have a layer of soil with a gradation of subsoil of decomposed rock on down to a layer of well-fractured rock, so that there is a complete passageway through the soil. The runoff would simply be the difference between the rate it would pass through the soil and the rate which it was applied. We have information which leads us to believe that the rate which it will pass through the soil is dependent upon its moisture-content. In ordinary soils the rate of entry is a good deal greater in a dry soil than a wet soil. As the soil-moisture or depth of wetting increases, the maximum percolation-rate of the soil decreases and that may account to a certain extent for the actual measurement of greater runoff from saturated soils than dry soils. I just wanted to draw that picture to demonstrate one theory why Mr. Gross' contention of a certain amount of water being all that was available for storage could not possibly hold.

G. D. CLYDE (Utah Agricultural Experiment Station, Logan, Utah)--Mr. Gross made some remarks which I would like to draw to your attention again because I, for one, may have a mistaken conception of what he was attempting to present to us. He discussed phenomena of snow melting and ended up with this statement, as I recall it, that we need not worry about floods from melting snow because of warm rains. Of course, that is quite contrary to the opinion of many people. I would like to review some experiences I have had in connection with the phenomena of snow melting to present to you a little different point of view from that expressed by Mr. Gross.

Now, as you all know, the snow-accumulation in our high watersheds becomes rather great by the end of the season. In terms of feet of water, we may express magnitude of that accumulation in terms of say five or ten feet of water at the end of the season. That is a big body lying as a blanket in snow-storage over rather large areas on a steep topography. Now, whether or not that constitutes a potential flood depends on the rate of melting, how fast it leaves that ice stage and is converted into a water-stage. He spoke of melting as being due to heat from the atmosphere, heat from the Earth, or heat from the rain. As a matter of interest, I spent one season on one of the watersheds in Utah--a period of about four months during the melting season for the purpose of making observations on the phenomenon of melting. Thermographic reports were made of temperature, daily density-measurements were kept, soil-moisture determinations were made under the snow-cover. I became interested in the melting due to the atmospheric temperature. I wondered how fast the water which melted on the surface of the snow went through the snow-layer into the ground, a snow-layer about five feet deep. I cut a trench in the snow and placed over the snow a little fluorescein. The entire surface of the snow was wet, saturated. It only took a minute or two for the fluorescein to go from the surface of the snow to the ground. After nightfall when the surface had become frozen and the temperature had dropped to below zero, I repeated the observation and soon learned that the fluorescein did not go down at all. The next rainfall that came along I made another test and found that the time of travel from the snow-surface to the ground was much more rapid during this warm rain than it was during ordinary melting.

One of the other observations I made was this: When snow melts, first we observe a settling of that snow-cover, and then we observe an accumulation of the water in the lower layer of the snow. It seemed to build up a capillary head of from one and one-quarter to three or four inches before the water broke from the snow and began to run away. That height of the rise was not constant. The rise was characterized by intermittent layers together with ice-layers at the beginning of the melting period. Before the water begins to run from the snow, the snow-layer begins to have a more or less uniform structure, and water seems to pass through those particles without breaking down the snow-structure. The interstices, however, become larger as the melting advances until the last snow to go becomes very granular with large interstices.

I observed also in many Pacific areas at high elevations where snow had increased in density there was a distinct change in color of the surface, indicating a saturated condition before the snow-surface would break. Now we find that this snow may melt and change from ice to water at a density from 20 to 60 per cent. We may get melting at 20 per cent density. Ordinarily our experience has been that melting begins at 35 to 40 per cent density. Suppose melting comes at the end of the season. We may expect a sudden and prolonged temperature-rise. We have say five feet of water in the snow-layer and the density of that snow-layer is 45 per cent and we meet a sudden temperature-rise and if the temperature rise is sufficiently long, we may get a disappearance of the snow-layer within a relatively short time. If we add to that a rather heavy rain, the runoff may constitute a rather serious situation.

Taken alone, I think the runoff is not serious. To attempt to verify this fact, I set up a system for making rain artificially, using water of different degrees of temperature. I found that it took a long time to melt that snow-cover with rain alone. If this snow-layer containing such large quantities of water at a near saturation-stage or near maximum density-stage goes through a season of prolonged high temperatures, I think there is considerable danger of a potential flood. A year ago, on the Davis County Watershed in Utah, where the Forest Service had been conducting their terrace-experiments for controlling erosion, the snow-cover as measured on the terraces was 12 feet, containing 40 per cent water--that is, five feet of water over those terraces. Not a drop of water ran off. The melting took place in such a way that the entire accumulation of water was absorbed. It happened that the melting period was slow. We had a warm period during the day and then a cold spell at night. There was no prolonged high temperature. I believe that snow-cover does constitute under certain conditions a serious potential flood. For that reason I would not like you to go away with the feeling we have nothing to fear from stream-flow resulting from snow-melting.

S. B. MORRIS (Stanford University, Stanford University, California)--I think Mr. Kotok has called on me just because he knows I have experienced some of the matters he has related, particularly in the region above Devil's Gate Dam where rainfall of some six inches in a day or two causes a large amount of silt to enter the Reservoir. This was carefully measured. I do not have the figures now; perhaps Mr. Kotok does. As I recall, the material which entered the Reservoir had the average volume of silt of something like 25 to 30 per cent of the total flow coming into the Reservoir. It flowed down like poor concrete and is a condition which I had hoped to charge up to the building of highways up there if it had not been that a fire interfered. We were looking forward to an excellent measurement of the debris that came from this highway work, but before we got the measurement a fire intervened.

J. D. SINCLAIR (United States Forest Service, Glendora, California)--I would like to to carry this subject that is now under discussion here across the Atlantic for just a few moments. I have recently had opportunity to make observations in Central Europe regarding the matter of vegetation and its use in erosion-control and stream-regulation in the region of the Alps. The use of vegetation in the older countries where they have been dealing with storm-control has extended over the last two or three hundred years. It is true that the use of engineering works is a very important factor for downstream-work and also for the regulation of the smaller stream-channels themselves. But the Europeans lay great emphasis on the vegetation upon the watersheds above these downstream-channels and smaller streams also. They are dealing in places not with watersheds which have been denuded of vegetation but they are extending the vegetation up the mountains where the elements have not permitted it to grow naturally. That is, they are attempting and are accomplishing their purpose of getting a forest-covering on watersheds in the Alps which would otherwise be eroding very badly.

I wish you had the opportunity which I had to see in not only one instance, but over and over again, in Austria, Switzerland, eastern France, also northern Italy and southern Germany, streams running clear from the forest-covered Alps and watersheds. Europeans have more respect for vegetation than I believe any of us have here.

R. E. HORTON (Consulting Hydraulic Engineer, Voorheesville, New York)--I feel that the relation of vegetation to the hydrological cycle is so complicated and so variable under different conditions that it is impossible to state any general conclusions which would be universally valid. Let us take the example of the very excellent picture drawn on the blackboard. If it were flat land with the original vegetation cleared off, a very different picture would be presented--at least if it were an eastern forest on glaciated land where there were a large number of depressions where trees had fallen. When the forest was cleared away and tillage followed, these depressions generally became filled if they were on a moderate slope. Studies which I have made by personal inspection of the topography showed that easily 40 per cent of the ground-area is now tributary to the depression if those depressions are not destroyed by cover-cultivation. You have a factor that may be more important than any of the factors mentioned thus far.

Something has been said about the changes of infiltration-capacity during rain. I have not found many instances where the change of infiltration-capacity was due to puddling. I do not dispute that that may be true. But if the soil were cultivated and rain falls on it, then for certain types of soil very rapid packing of the surface occurs. Soils range in texture from packed clays to porous sands. We must be careful about broad generalizations here. Sand hardly packs at all; its infiltration-capacity may be practically unchanged except in washing.

Something has been said about the filling in of the pores of the soil with fine material. That also occurs. But I witnessed some experiments of the Soil Conservation Service this summer on runoff using artificially produced rain. The soil-type was new to me, I presume of volcanic origin, of tremendous ability to absorb rain. With a rain of six inches an hour the soil was absorbing at the rate of six inches an hour. Experiments were carried out where there was a rain-making machine placed along a railroad right-of-way where there was no cultivation for generations. Just outside the fence there had been severe overgrazing. There was practically no difference in infiltration-capacity. In some of the experiments, instead of the infiltration decreasing, I found the infiltration-capacity of the soil increasing. I am not sure that we have the correct explanation. Dr. Lowdermilk and I have talked this thing over. During the dry period when dust-storms occurred, excessively fine dust was deposited on that soil. If the water was applied at a sufficiently high intensity to carry away that film through which little water could pass, larger pores underneath opened up and the infiltration-capacity increased. I experimented with many eastern soils where, if there was any considerable amount of colloid material in the soil, I invariably got a decrease. I think, as one of the preceding speakers pointed out, that infiltration-capacity is the fundamental concept to be kept in mind in studying runoff-phenomena, because runoff is just the difference between rains and absorption.

E. I. KOTOK (U. S. Forest Service, Berkeley, California)--One closing comment. There is no sharply defined issue, as one might expect. I think there is an enormous field yet to be explored. We, as foresters, appeal to the hydrologists and the engineers not to forget an important part of the equation and the extent which the vegetation plays in that equation will vary from time to time and from place to place. Do not ignore it, is all we ask. While I am on my feet I want to extend to you who have not had the pleasure of visiting the San Dimas Forest, an invitation to do so. It is dedicated to the study of those particular problems which we have taken up today. Those of you who have been there know that we are there in one central place to attack the problem on all fronts and let the chips fall where they may.

M. P. O'BRIEN (University of California, Berkeley, California)--There is one aspect of flood- and erosion-control which may seem rather remote to you here but has been of interest to me over the last ten years. That is the matter of beach-preservation. It seems that the material which forms our beaches, particularly on the California Coast, is derived from the drainage-basins and not from the cliffs. An examination of the material indicates that it cannot come from cliffs but must come from drainage. As to the accelerated erosion during man's occupancy of this State, at least the earliest surveys of our beaches do not indicate an increase in beach. The beach is probably a stream of sand blown into dunes which come from our drainage-channels or drifts in from the sea. If there had been any marked increases in erosion, we would expect to have wider beaches, more beach-material, but that seems not to be the case. There are some cases where the evidences indicate an increased accumulation. It is said that at Newport Bay in the early Nineteenth Century the ocean beat against the cliffs; but general surveys back in 1860 show there has been no increase in the beach which may indicate no marked increase in erosion.

Another aspect of erosion-control is that if the material which forms the beach is derived from the mountains and drainage-channels, the reduction in the amount of material is going to seriously affect the beaches which represent a very large investment and which I think should be considered. It may seem remote to you, but I think we have enough evidence now to show that flood-control measures should be accompanied by beach-control measures.

H. E. REDDICK (U. S. Soil Conservation Service, Santa Paula, California)--I could talk some time on that last subject of beach-erosion. In the last few years in many of our towns along the coast we have had an increase in width of beach. Whether that material came from the floor of the channel or lands, I cannot state. There is no doubt of erosion.

G. D. CLYDE--The engineers say if you vegetate mountains, the result is transpired water and you reduce your flow. I think you will agree. But what about the invisible flow? Why can not we coin a term for the water available for use? We can put that water into storage either above ground or under ground. The things we are interested in are: (1) The maximum amount of net usable water, municipal use or whatnot; (2) control of these waters to prevent their doing damage; (3) control of these waters to prevent the removal of the earth-mantle.

No matter what we do along one channel, we interfere with something on the other channel. For example, if the erosion-people put in check-dams on certain streams, they are going to interfere with water-rights. Where are we going to draw the line? It seems to me to be a question of the maximum public benefits. When we speak of this term "conservation of water" and whether or not forest-cover or vegetative cover will conserve water, we must keep in mind the time of flow and the usable yield.

J. J. PRENDERGAST (Redlands, California)--I have been interested in taking all this in. I will say that I think there is a conflict right along between the different elements. This point of the net usable accumulation, the net usable runoff, is a very important one. We have to give and take on that. You cannot figure it in the terms of gross, you have to figure it in terms of net.

G. H. HART (University of California, Davis, California)--We want to get more information and then maybe these controversial matters will not be nearly so controversial. Plain common sense will answer some of the questions after we get the data. I admit I am still a little biased on the side that you can not, while Mr. Kotok is on the side that you can. We can both budge a little, but engineers need exact measurements, and we are, therefore, anxious for the engineers to come in and do the exact measuring so that we cannot budge so far on either side.

Chairman FRANK ADAMS (University of California, Berkeley, California)--I have attended a good many of these meetings at which these subjects were discussed. The discussion today shows that people with very antagonistic attitudes and very different points of view are coming together. I believe it is a real accomplishment. All we want is the truth.

Morning Session, January 8, 1938

The Saturday morning meeting was under the chairmanship of Dr. B. M. Woods who introduced Professor Norman E. A. Hinds.

GEOLOGICAL EVIDENCES OF RECENT FLOODS

Norman E. A. Hinds

(Stenographic transcript of an informal paper with a fine series of lantern-slides)

In the present-day discussion of floods, flood-protection, and soil-erosion, the impression is very commonly given that there were no serious floods in America before the brief epoch of rapid spread of population, intensive cultivation, and large-scale stock-raising which have characterized the last decades of our history. Great stress is laid upon the fact that floods have been intensified by the destruction of the vegetative cover, by too intensive cultivation, by over-grazing, and by various other causes to which our people have been a party. But, when we examine the abundant record preserved at many places in our country and extending far, far back of the brief epoch of human occupation, the evidence is quite out of accord with this opinion.

A short time ago, I was reading of a great storm over much of northern California in 1862, which was before man had done any serious damage to the vegetative cover. Nevertheless, great floods poured down mountain canyons and out into the broader valleys. Many people were drowned and there was much damage to property. Large areas of the flatter country were inundated. Some houses were destroyed and others were covered almost to their tops by the waters. Bridges and roads were washed out. The devastation of 1862, the devastation of a few weeks ago in practically the same region, has happened countless times in the past and will happen countless times in the future, no matter what efforts man may make to prevent it. This is testified to by the enormous mass of flood-sediment which has been piled up through the whole length of the Sacramento and San Joaquin valleys over a very long period of time. Great floods always have been and always will be a normal process of Nature.

Rivers deposit sediment sometimes actively, sometimes feebly, during their normal flow, but deposition during normal flow is in the immediate channel. Such sedimentation is not of great consequence except in very large, slowly flowing rivers like the Mississippi, and, even in these great rivers, deposition during low or normal water is insignificant as compared with that during a flood-cycle. Over the Earth we find enormous areas and great thicknesses of sediments deposited by streams of all sizes--in fact most of the sediment laid down on the Earth above sea-level is of fluvial origin. Study of these deposits clearly indicates that they were accumulated when the streams were swollen with flood-waters. There is abundant proof that floods as great as, or greater than, floods which man has observed on any river of the Earth have occurred many times during the recent and distant past. We are aghast at the tremendous floods of our times; we compare them with other floods of which we have actual record, but our period of observation has been very short, and consequently we cannot determine what may be the maximum flood to which a river is subject. We have not enough records, for example, to prove that the 1937 flood of the Ohio was the greatest flood which has come down that River. We are prone to pass lightly over the brevity of our time upon the Earth and especially over the limited time during which actual records have been kept.

The geological record on this subject is abundant and clear-cut. When we examine the geological agents, we find that all of them are subject to occasional catastrophic behavior. The catastrophe is a rule of Nature. Consequently, we find through the course of history that all rivers have been subject to a great many moderate floods and to a smaller number of great magnitude. Rivers are doing a certain amount of erosion and deposition all of the time that they flow. But there are certain times when rivers rise to greater and occasionally to greatest peaks, and then their activity increases enormously. Erosion and deposition are speeded up by leaps and bounds. I have studied a host of streams and their activity during flood-time, and I constantly marvel at the destruction and accumulation which can take place in a very brief interval of time. The desert is a marvelous place to study stream-action, for the drainage-courses--dry most of the time--are filled by the flash-floods particularly of the summer, and the short-period streams do an astounding amount of work in a few minutes or a few hours. In the Sierra Nevada, the streams accomplish very little during the dry months of the summer and fall, while during the rainy months their erosive activity is greatly increased. Where they emerge upon the floor of the Great Valley, deposition is very rapid during flood-time, and, of course, this extends into the main streams, the Sacramento and the San Joaquin. Man found it necessary to erect Boulder Dam as a barrier to the great floods of the Colorado; in the flood-plain of this mighty stream there is endless testimony that such floods have occurred ever since it began to flow.

Another thing that we must bear in mind is that destruction of plant-cover is not solely the work of man. At many times in the past, there has been ruthless devastation by Nature herself, and one of these times comes very close to our history. On several occasions during the last million of years, chilling of climate caused the covering of huge areas of the Earth with glacial ice. Ice-covered areas were devoid of vegetation and similar conditions prevailed in the chill regions far beyond the limits of the ice. The recent expansion of arid conditions in many parts of the Earth also has been highly inimical to the plant-population. When the glacial ice melted, floods greater than man has ever seen took place, and record of them is strikingly preserved in many places over the Earth. The spread of desert areas also has developed greater acreage where speedy flood-erosion and deposition can go on. Such climatic fluctuations must be understood in order to correctly interpret the activities of streams. Also the rock-record which has been left for us is most important in this study.

I do not think that there is the possibility of floods at the present time which can compare with those occurring a few thousands of years ago during the waning of the ice of the last glacial epoch. The sedimentary record of the magnitude and violence of these floods greatly exceeds that which is being accumulated today. Very imposing parts of this record can be seen in many parts of our great West. Of it I shall show you pictures later. When I look at the rock-record, I feel that we must not lay too much stress upon what is happening at present; undoubtedly streams are being affected by devegetation and other activities of man. These are serious problems of local and national concern. Measures, costly though they may be, must be undertaken to offset some of the damage which we have caused if our natural heritage is to be preserved. But these measures must involve a most careful analysis of what has gone before. Nature has caused appalling damage by floods long before man ever appeared upon the Earth, and will continue to cause such damage as long as streams flow. To repair and retard human damage, we must understand how Nature has worked in the past and is working today. Study of the geological record is fundamental to an understanding of the action of natural forces and agents.

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CAUSES AND CONTROL OF MAJOR FLOODS

R. D. Goodrich

For the purposes of this paper, a major flood is one that is so large and so infrequent that such a flood will occur on any given stream perhaps only once or twice in a lifetime.

Many factors are involved in the production of a major flood, but they usually do not all operate with maximum force and effect. An excessive amount of precipitation is, of course, the most important cause of unusual floods, but an unusually heavy rain does not always cause an unusual flood. Some of the climatic factors are temperature, wind, direction of travel of the storm, duration and frequency of storms during the flood-season, time of the year, etc. Physical conditions which produce variations in floods from different drainage-basins include variations in the geology and topography of the basins, as well as variations in different parts of a given basin. Such conditions, together with the size and shape of a drainage-area, are more or less fixed but may develop differing flood-conditions for storms of different type and direction of travel. Still another set of variables depends upon the condition of the surface upon which precipitation falls, whether it is rock or soil, bare or covered with vegetation, and the character and condition of the vegetation; the character and condition of the soil, whether hard or loose, dense or porous, its moisture-content or degree of saturation, elevation of the groundwater, presence or absence of snow, and whether or not the ground is frozen.

Finally, a long list of variables depends upon the presence of man himself. Is the land used for pasture and to what extent? Is it bare, cultivated soil, or planted to crops? The character of the surface, the kind and condition of the crop, and even the direction of plowing and cultivating all have very marked influence on surface-runoff and, consequently, upon flood-volumes and heights. Again, has mans occupancy of the land been such as to result in ordinary or intensive farm-operations; has the land been smoothed only, or has it been terraced? Have natural drainage-channels been interfered with by the construction of railroads, highways, canals, and levees, or has the natural condition been still further altered by the building up of a town or city with greater and greater encroachment upon the normal channel of the stream? All these conditions and others such as the amount and condition of forestation have very material influence on the size of floods. To evaluate the effect of each of these variables is next to impossible, but from the list affecting the magnitudes of floods, it is very evident that ordinarily they will not all operate to cause maximum runoff from all storms. In fact, the opposite is more often the case. However, there will certainly come a time when a great number of conditions will favor maximum runoff and flood-concentration, such as frequent rains for some time with saturated soil in the lower valleys and frozen ground at higher elevation covered with an accumulation of snow. When this is followed by a stationary or slowly moving storm-center with continuous heavy rains and rising temperatures, a phenomenal flood will result.

Observation and experience indicate that when a very large number of factors are so combined the size of the resulting flood may be far beyond all previous experience or expectation. As an illustration, the highest flood on the Damodar River in India was estimated at 422,000 cfs for 56 years of record [see 1 of "References" at end of paper]. The following year the maximum discharge was 650,000 cfs--an increase of more than 50 per cent above the previous maximum. A similar illustration occurs on the Arkansas River at Pueblo, Colorado [2]. Under very different climatic conditions, with a continuous record of about 40 years, a maximum rate of discharge of 100,000 cfs occurred in June, 1921. Prior to that the maximum observed flow was only 10,700. This flood amounted to more than 20-cfs runoff per square mile in a semiarid locality. One other illustration which may be of more local interest is found in the record of floods on the Kings River, California. Prior to 1914 the observed maximum was only 33,000 cfs, but in January of that year there was a flood of 60,000 cfs. The 1914 flood was the highest since 1862 and was, therefore, the maximum record during a period of over 70 years. It was nearly double the previously observed maximum and a third larger than any subsequent record, with the possible exception of the unusual flood of December, 1937.

With these few notes on the variables affecting the magnitude and frequency of major floods and the relation of some of these floods to those which are more commonly observed, we may ask whether man's occupancy of areas subject to floods has had or can have any effect on their magnitude and height, and in what manner and to what extent is it possible to control them. Typical streams subject to great floods are the Mississippi and Sacramento and their numerous tributaries, in this country, and the Nile, Po, Ganges, and Huang Ho among the rivers of other countries. The story of such rivers is usually complicated, but when reduced to the simplest terms the principle elements of the picture are: Sources in high mountainous areas where many tributaries gather their supplies of water and debris, and then flow down the steeper mountain slopes, through the foot-hills, and finally discharge across the plains to end at an ocean or some one of its major arms. (Note--Deltas are formed in non-tidal seas, or where tides and currents are small.) During geologic ages such a river probably built its channel to "base-grade," by excavation or erosion in the upper reaches and by erosion and filling in the present plains-areas. Any subsequent tilting, rise, or subsidence of the land would upset the equilibrium and the stream would again begin cutting or filling its channel in an endeavor to re-establish its base-grade. By this is meant that the grade and load carried by the stream and the work done in erosion and overcoming resistances to flow and transportation of debris, are so adjusted to the volume and velocity of flow that the energy of the stream is used as it is developed by the available fall.

After great rivers had done their part in building up the great and fertile plains of Egypt, India, China, Europe, and our own country, man appeared and occupied these areas in order to secure greater returns for the fruits of his industry.

At first he was content to adapt himself to the conditions as he found them in Nature. In Egypt and in many other localities the streams annually overflowed their banks, spread over the valleys for a time and then receded, leaving the ground saturated with live-giving moisture and a layer of new rich soil of greater or less thickness. In other valleys, inundation was not an annual event but did occur every few years. In time this process of frequent overflow caused the banks of many rivers to become gradually built up so as to confine these channels within more definite limits, as has been the case with the Mississippi and the Sacramento. As the land farther from the stream became relatively lower it drained more slowly and the farmer was delayed in his planting and harvest. Furthermore, as the river-banks were raised, more years passed without overflow and, of course, without the addition of new fertility to the soil, but without very appreciable reduction in the ordinary crops. What could be more natural than to assist Nature and build up the rivers' banks more rapidly? Thus, river-dykes were invented.

Soon, however, there followed difficulty, damage, and disaster. With a stream dyked off from its valley on one side only, the other side must inevitably be flooded more frequently. With both sides of the channel lined with levees to protect the rich river-bottoms from overflow, the larger floods must rise to greater heights. Then the entire flow must pass at once and more rapidly down the restricted channel. Formerly, a part, often a very large if not a major part, flowed out over a broad valley, was detained in its progress and dropped most of its load of silt, instead of being forced to carry it on to the sea.

With levees built up to considerable height all ordinary floods would be confined within the more limited channel of the river and towns and villages and even large cities have grown up and flourished on the banks within the limits of many flood-channels. Man has greatly increased his wealth by this use of the river's domain, but he has at the same time increased the risk of destruction from the more infrequent major floods. And destruction has always followed unorganized individual efforts for the control of the seemingly larger floods which result from such efforts. To meet this situation individuals have been forced to pool their resources and ask Governmental agencies to take over the work and responsibility, as well as the financial burdens of flood-control.

In California, State and Federal agencies united for the construction and maintenance of the Sacramento Valley system of control-works and have operated but a few decades. Government works were undertaken on the Mississippi about a century ago. In Italy the works on the River Po have been under organized governmental supervision for centuries. In China flood-control has been a governmental function for more than 3000 years. The Nile has been similarly managed for probably 5000 years.

There are two well-organized methods of attempting to control floods, both of which are based on observations of the behavior of rivers and floods without interference on the part of man: First, by means of dykes and channel-rectification, and second, by means of storage of flood-waters. In any general plan, the less the departure from Nature's methods, the larger will be the measure of success. In most cases it is to be expected that a combination of the two methods will give the best results with the least cost.

The plains of Shantung and Hopei in North China are criss-crossed with the remains of ancient dyked channels of the Yellow River and other lesser streams. Here the transition from mountains and foot-hills to the regimen of the plains is so abrupt, the load of silt being transported is so immense, and control has been attempted for so long a time, that the uniform, gradual and natural up-building of these plains has been impossible during the past 4000 years.

The writer observed while on flood-control work in China that the greatest success seems to be attained with a dual system of dykes as used along much of the Yellow River. If a breach occurs in the line next the river, the area between the two lines becomes a very effective storage-basin and absorbs a portion of the flood, holding it till the peak is past, when it is returned again to the river. Cross dykes between the main lines tend to prevent the flooding of too large an area from any single breach.

Immediately below a break in a dyke much damage is sure to occur. A great hole will be scoured to considerable depth and a large area of tillable land is usually covered with sand and mud so that it may be rendered useless for decades. If the breach occurs near a village still greater damage and destruction will result. These conditions naturally led to the construction of openings with masonry-abutments and wing-walls, and well-paved or masonry sills and aprons at the entrance of short channels so as to permit of spreading the flow without destructive scour. Sometimes a series of piers between the abutments permitted the closing of the structure by stop-logs. If the sill is of considerable length, a temporary dyke of earth is built along the crest to a somewhat lower elevation than the top of the main dyke. This portion can then be cut or will be overtopped before a flood reaches an elevation which would endanger the main lines of levees.

Another development in China was the use of a flood-relief channel with a structure similar to those just described, at the entrance. These channels might discharge into the main channel at some lower point, thus constituting a by-pass as used in the Sacramento Plan. Or they might discharge into a low area or into another river-basin, or the water might be allowed to spread over cultivated land without doing much damage--unless it happened to occur at a time and place where it augmented flood-waters from another source. Several of these devices have been adopted in the Mississippi River Plan.

In Holland and Italy another development has taken place as an outgrowth of the dual system of dykes. In this type the inner dyke is constructed with rather broad base and flat slopes well sodded and maintained. The height of the dyke is only sufficient to protect against ordinary floods, while major floods flow over the top in a broad thin sheet with but little resulting damage and without carrying sand and silt from the river's bed to be deposited upon the land. If the outer levees are at a sufficient distance from the river, an adequate flood-channel is provided for the largest floods, while losses are reduced to a minimum. No permanent improvements are permitted within this flood-channel, except the most necessary ones required for facilities of transportation and maintenance. Cultivation is permitted between the levee-systems under certain restrictions and the area is gradually built up and fertility maintained by occasional over-flow and deposits of silt.

Along the Po River, in Italy [3], levees have been successfully maintained for the past 900 years or more, largely using the dual system just described. While disasters have occurred, the levees and flood-plains have been built up until the River seems to have come to base-grade with relatively stable equilibrium between scour and deposition. Most of the silt, which is less in amount than that carried by the rivers of North China or by the Colorado, is transported to the mouth and deposited in the sea. As the delta is extended the River is lengthened, so that some means must be provided to compensate for the resulting changes in grade if equilibrium, and the river's base-grade, is to be maintained. This may be done by natural means by deposition along the course of the river or to some extent by artificial works, such as cutting off bends and other channel-improvements.

Temporary storage of excessive flood-waters has been mentioned as a means of providing flood-control. Valley-storage in the flood-plain itself is the most effective means for reducing the height and resulting danger of major floods. This practice, however, usually necessitates at least the partial withdrawal from cultivation of some of the most productive land in a river's drainage-basin. Restricted valley-storage can be utilized to very great advantage by the construction of ample flood-ways as has been done on the Sacramento and the Mississippi. However, above the points where such flood-channels may be made available there are always reaches on any river subject to great floods, which can be protected only by levees built at great expense and even then not without great risk and danger of failure. Furthermore, the construction of high dykes is prone to give the uninformed a sense of security which too often proves to be unjustified. The building of Shawneetown, Illinois, on the banks of the Ohio and its proposed removal to a point three miles inland, is a case in point. Other cities and towns [4] are also wisely considering the abandonment of low areas subject to overflow by major floods, as at New Albany, Indiana, which plans to move residence and business back to high ground and park the river-front to avoid serious flood damages in the future [5]. It should be thoroughly understood that any restriction of, or encroachment upon the flood-channel is necessarily accompanied by an increase in height and velocity of passing floods. The only possible advantage to be gained, aside from the use of the land protected, is the increase in scour, if this can be confined to the bed of the stream. (The bed of the lower Colorado has been found to scour two feet for each foot of rise of the surface; the same is true on the Yellow River.) Unfortunately, with the increased velocity, the current too often attacks the river's banks and levees. Hence, special works are frequently required for bank-protection. This may consist of a great variety of revetment-construction or of spur-dykes or groyns with rip-rap to protect the exposed portions, or both may be used, depending on circumstances.

Many illustrations are to be had in Nature of the great regulating effect of lakes upon river-flow. The most striking example is to be found in the very uniform outflow from the Great Lakes which only varies about 20 per cent above or below the mean discharge at Niagara [6] Among the oldest dams built for storage for flood-control are the detention-basins on the Loire in France built in 1711 [7]. The best known reservoir-projects in this country for flood-protection are on the Miami River in Ohio, those for the control of floods at Pittsburgh, and the more recent ones on the Muskingum River. These projects represent the two types of detention- and storage-reservoirs both of which have advantages in certain cases. The disadvantage in storage for flood-control is usually the small percentage of the drainage-basin of a large river which it is economically possible to control. Whenever reservoirs can be justified, in the writer's opinion, they provide the best means for flood-control. Although any reservoir, even when full, will generally reduce and retard a flood-peak which passes through, to be most effective it is evident that the reservoir must be empty. In the eastern section of the country flood-control reservoirs are seldom, if ever, used for any other purpose, but here in the West it has been found that reservoirs may be built for the combined use of irrigation, power-development, and flood-control with the advantage of reduced cost per unit of storage chargeable to each use [8].

The largest flood-storage in the West today is, of course, to be found in Lake Mead. This type of storage is most effective between the Reservoir and the next large tributaries. On the

Colorado at this point the maximum flood, estimated at 200,000 cfs will be reduced to 75,000 cfs. This has made possible a complete change in the type of bridge which can be used on this River, from very expensive long-span truss-bridges with deep and difficult foundations, to the far cheaper trestle-type recently completed by the United States Bureau of Public Roads at Parker, Arizona [9].

During the past few years the writer has been engaged on investigations involving the economic justification for two flood-control projects on the West coast, covering the valleys of the Sacramento and the Williamette rivers. In both cases it appeared that the most complete, practical, and satisfactory plan involved the use of both levees and reservoirs, the latter to be used for other purposes, as well as for flood-control. That reservoirs can be so used in the Pacific Coast Area was shown conclusively by studies made by the Water Resources Division of the Department of Public Works of California [8]. The principal reason for this is the limited flood-season and the dependable runoff from rain and melting snow after the danger of major floods has past.

An exhaustive investigation is necessary before sound conclusions can be reached as to the magnitude and frequency of great floods; and the means and extent of control which can be justified, is a problem in engineering economics. The control of ordinary floods is more a problem of the maintenance of local production, industry, and services. However, as the desire or necessity increases for the control of major floods, the problem takes on broader aspects and involves questions of public policy and general welfare which may become national in scope.

The first problem usually met in undertaking an investigation is that of securing adequate maps and records upon which to base the necessary studies. Topographic sheets of the United States Geological Survey provide excellent base-maps for general purposes, but they are seldom available for the entire drainage-area involved. For special studies they are not usually in sufficient detail; hence, a large amount of surveying and mapping is necessary for complete information. General maps should cover the entire drainage-basin of the main stream with all tributaries. Exact information is also necessary as to the location and extent of areas subject to overflow and sites which may be suitable for flood-storage, both in the valleys and above them.

Probably it is safe to say that up to date, no flood-investigation covering any large area in this country has been undertaken with really complete and satisfactory records of stream-flow as a foundation. When available, the publications of the United States Geological Survey are invaluable, with records of State and private agencies next in order of importance. The completion, extension, and analysis of all available records of rainfall and runoff are necessary in order to estimate the stage and frequency of lesser floods which begin to cause damage, as well as to determine the average flood. The difficulty of estimating the discharge and frequency of major floods increases with their magnitude, and as the length of the period of record decreases. However, the best possible estimate of an approximate maximum flood is essential.

That flood-probability studies as related to an analysis of a flood-control project, are of value and that they may be relied upon to indicate the weakest points and the relative frequency with which project-capacities will be equalled or exceeded, is convincingly shown by the records of the flood of last December on the Sacramento. Reports in the technical press [10] state that this flood was 18 inches higher at Red Bluff than the previous high record of 1909 and also that the levee broke in three places so as to flood all of Butte City.

The writer's study of flood-probabilities [11] in the investigation previously mentioned shows that the reach of the Sacramento River above Moulten Weir, which includes Butte City, is the section where floods will most frequently exceed the designed capacity of the project. It also showed that the average period between floods equaling or exceeding the channel-capacity is about 21 years. The December 1937 floods reached a maximum discharge at Red Bluff of 290,000 cfs or more, which is 30,000 cfs more than the channel capacity of the reach. The next previous major flood which equalled the capacity of this channel was in February 1915, giving an interval of almost 23 years. This close agreement between the actual and the computed exceedence periods is, of course, accidental and not significant as to the exact interval between major floods. What is significant, however, is that the analysis showed the most vulnerable sections of the project were, in the order of vulnerability, near Butte City on the Sacramento, near Oroville on the Feather, and near the City of Sacramento, on the American and Sacramento rivers.

Having secured all possible data on floods in the area under investigation, together with the location and capacity of storage-sites, a plan of operation of reservoirs must be set up and floods routed through these reservoirs and the channels below, to determine the effect of storage upon flood-heights throughout the river-valley. The location and height of levees to secure equal or greater protection should also be determined. The estimated cost of each plan should then be determined and compared, together with the probable cost of modified plans which would include partial protection by each method in order to arrive at the most economical general plan. The storage-capacity which can be provided economically will usually be equivalent to a depth of five to ten inches of water upon the drainage-area above the point of storage. One great advantage in the use of storage for flood-control when it can be shown to be effective, is the reduction in the height of major floods and the decrease in the area subject to damaging overflow or the smaller leveed channels which would be required for the discharge of flood-waters.

Finally, the benefits which would result from the construction of works for flood-control must be determined and capitalized for comparison with the estimated costs. Only in this way can one arrive at any sound conclusions as to whether or not the great outlays involved in comprehensive plans for the regulation and control of major floods can be justified.

It has sometimes been the practice to estimate flood-damages in a given area and use the amount so determined as a measure of the cost which might be justified for the construction of flood-protection works. The Committee on Flood Protection Data of the American Society of Civil Engineers does not approve of this method [12]. The writer agrees that this method should not be used when any other means is available for arriving at a reasonable estimate of the benefits which would result from flood-control. On the other hand, he does not agree that "Damage and benefits cannot be equated" [12]. The two are related, in our opinion, at least to the extent that any reduction in annual costs which must be met by agriculture, industry, and business, due to damage and expense, constitutes a very real and direct benefit from flood-control. If then, the damages for unregulated floods for a series of 50 or 100 years are computed and also lesser damages which would result if the same floods had been regulated through the operation of reservoirs and other control-works, information will be available for a very useful measure of the value of any plan for control of floods.

Damages should be appraised for an ordinary and a major flood and also estimated for the probable maximum flood. The total damages for the series of floods for which records are available can then be computed by the historical method suggested by C. I. Grimm [13] or the probable damage which would result during the useful life of the project may be estimated by the flood-frequency method as used by the writer. Next, the damages, if any, resulting from floods as regulated by the proposed project, are computed in a similar manner. The total damages for the given or assumed series of years both for uncontrolled and controlled floods are then reduced to average annual amounts. The difference in these two quantities represents a saving in costs to the community and, therefore, a direct and material benefit as a result of the project. To this type of benefits should be added others such as the value of crops from reclaimed areas not previously cultivated, the difference in value of crops formerly produced and of higher-grade crops if they can be substituted, benefits from enhanced value of land, both urban and rural, etc. The total annual benefits may then be capitalized to show the economic value to the community of the flood-control project. If the cost of the project for flood-control does not exceed the capitalized value obtained, as above described, it is economically sound.

The survey and appraisal of flood-damages and benefits is fully as important as the investigation of flood-conditions and the design of the project. It should include the detailed study and determination of damages in both urban and rural areas, covering damages to goods and properties, both public and private, loss of profits to business and services, damage to agricultural crops, land and improvements, damage to railways and highways and loss of transportation-business, livestock and damages to river-channels and structures.

In conclusion it may be of interest to give some results from the recently completed investigation for the Willamette Valley in Oregon [14]. Sufficient data were available to apply the historical method to the determination of damages and benefits on two-thirds of the total area involved, while the flood-frequency method was used for the estimates covering the remaining area. The total flood-damage estimated on the historical basis for a period of 47 years, assuming the present development of the valley as constant, for that time, amounted to nearly $81,260,000. Using the flood-frequency method alone, the total damage was estimated at $78,940,000. The difference of $2,320,000 is only three per cent of the total. The corresponding estimates of the annual benefits creditable to the proposed reservoir-protection were respectively $1,344,000 and $1,253,000. Here the difference of $91,000 annual benefits determined by the two methods is seven and one-quarter per cent. These results are very satisfactory, as differences twice as great as these might have been obtained without altering final conclusions.

The great flood of 1861 in the Willamette Valley probably covered an area of more than 350,000 acres, including property valued at more than $100,000,000 with the present development. No estimates were made of probable future development in this region nor of the amount of damages which would result under such conditions. It was also estimated that the construction of the proposed reservoirs for flood-control would increase land- and crop-values which capitalized would amount to an added benefit of about $10,000,000. It is further estimated that the reservoirs proposed in this project would reduce annual flood-damages to less than 12 per cent of the present amount.

If this paper has contributed even in a small way to a more general knowledge of the causes and damages resulting from great floods and to a greater appreciation of the possibilities and benefits of flood-control, its purpose will have been accomplished.

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[14] The data quoted are taken by permission from App. B, Flood control, accompanying report on Willamette River and Tributaries, Oregon, dated April 17, 1937, which was made under the direction of the Division Engineer, North Pacific Division, U. S. E. D.

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THE EFFECT OF SILT-REMOVAL AND FLOW-REGULATION ON THE REGIMEN OF RIO GRANDE AND COLORADO RIVERS

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The completion of Elephant Butte Dam on Rio Grande in 1915 inaugurated a new regimen of flow-characteristics for that Stream and the closing of the tunnels at Boulder Dam on February 1, 1935, will cause Colorado River to turn over a new leaf and begin a better regulated life.

The changes in regimen are far reaching and of great importance to the developmental program below these structures. Two factors are mainly responsible for the changes: (a) Floods originating on the main river above these reservoirs are largely absorbed and the outflow is almost wholly under control; (b) instead of the silt-laden water that formerly passed into these reaches there now issues from the control-gates at the dams, rivers of bluish green clearness with hardly a vestige of silt--an absolutely novel experience in the life-history of these streams.

Rio Grande below Elephant Butte Reservoir--First, let us consider the 300-mile reach of Rio Grande between Elephant Butte Dam and the beginning of the canyon-section at Fort Quitman 150 miles below El Paso. Except for a few canyon-sections the River flows through alluvial valleys, practically all of which are irrigated.

Before control began the River carried immense quantities of silt. It flowed in a wide, meandering, sandy channel, subject to floods of menacing proportions. Water was too abundant during the snow melting period in the spring and too scarce in late summer. Table 1 shows the annual runoff, flood-peaks, and silt-content of the Stream before and after the beginning of a controlled regimen. San Marcial, New Mexico, is just above the reservoir. El Paso, Texas, is 145 river-miles down stream and about half way to the canyon.

The mean annual suspended load at El Paso was about eight per thousand by weight before operation of Elephant Butte Reservoir began and since then it has dropped to less than two per thousand. From the standpoint of canal-operation, controlling the flow has changed the problem

Table 1--Flood- and silt-data, Rio Grande

Station	Period	Years	Before operation of Elephant Butte Dam	After operation of Elephant Butte Dam
			Mean annual runoff in acre-feet	
San Marcial, N. M.	1897-1914	18	1,150,000	
	1915-1931	17		1,510,000
El Paso, Texas	1897-1914	18	812,000	
	1915-1930	16		638,000
			Maximum discharge in cfs	
San Marcial, N. M.	Oct. 11, 1904		33,000	
	Sep. 29, 1929			33,000
El Paso, Texas	June 12, 1905		23,700	
	Sep. 3, 1925			9,160
			Mean annual silt-content in tons	
San Marcial, N. M.	1897-1914	18	22,800,000	
	1915-1931	17		21,700,000
El Paso, Texas	1906-1909	4	17,500,000	
	1916-1925	10		550,000

from one of silt to one of sand. The suspended matter has largely disappeared and the sand-bed load has become of paramount importance.

In spite of the inconvenience and damages resulting from floods they do have a certain economic value in freeing their channels of the detritus that accumulates during periods of lower flows. The normal stream-bed has acquired an area and slope commensurate with its flood-characteristics. After regulation, those floods disappear; the more uniform flow can be carried on a lesser gradient and occupies a lesser cross-sectional area, and the River immediately begins to adjust itself to these new conditions. The margins and overflow-areas become over-grown with hydrophylic plants and the stream-gradient becomes flattened.

Figure 1 shows the change in gradient that has accrued in this reach of Rio Grande. The horizontal line represents the water-surface according to a survey made in 1917. The solid irregular line represents the change in water-surface as determined by a survey made in 1932 after a lapse of 15 years. The light line marked by crosses is the average change in each 50 miles reach. It will be observed that the River has lowered its channel at the head of the reach about three feet and raised it at the end about one foot--an overall change of four feet in 300 river-miles. Were it not for the numerous dams and riffles the change in gradient would have been more pronounced. Much coarse detritus is brought into the channel of the river by flash-floods on the ephemeral tributaries. There are no major floods now to carry off these accumulations and the River can only gnaw slowly at their bases or go around them by eating away the opposite alluvial bank.

On the flattened gradient a flash-flood from tributaries of relatively minor discharge such as the one of September 3, 1925, now causes almost as much damage as formerly did a major flood of treble the discharge.

The new regimen on this Stream has not as yet been fully established. The four dams above El Paso have limited the retrogression to the reaches between them. Below El Paso, however, it is likely that the bed of the river will continue to rise in spite of the three dams in this reach.

Above Elephant Butte Reservoir the stream-bed is being agraded by the backwater-effect of the Reservoir. At San Marcial, which is 40 miles upstream from the upper end of the reservoir-level that has existed during the past 15 years, the channel has been raised some ten feet. The town of San Marcial has been virtually abandoned, as it is subject to flooding with every freshet.

The effect of retrogression-tendencies has been partially counteracted below El Butte by the rectification-program [see 1 of "References" at end of paper] that has been under way during recent years. The channel has been straightened and the Stream confined between banks--the middle of the rectified channel becoming the new boundary between the United States and Mexico. This rectification-program is to be extended upstream from El Paso to near Caballo Dam about 29 river-miles below Elephant Butte Dam. This will almost obliterate the retrogression-tendencies by shortening the channel and thus steepening its gradient.

Colorado River below Lake Mead--Forces of the same nature have been at work to produce a

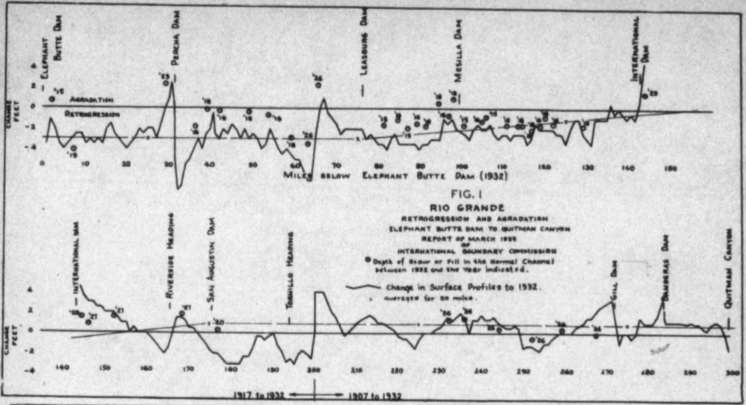


FIG. 1

RIO GRANDE

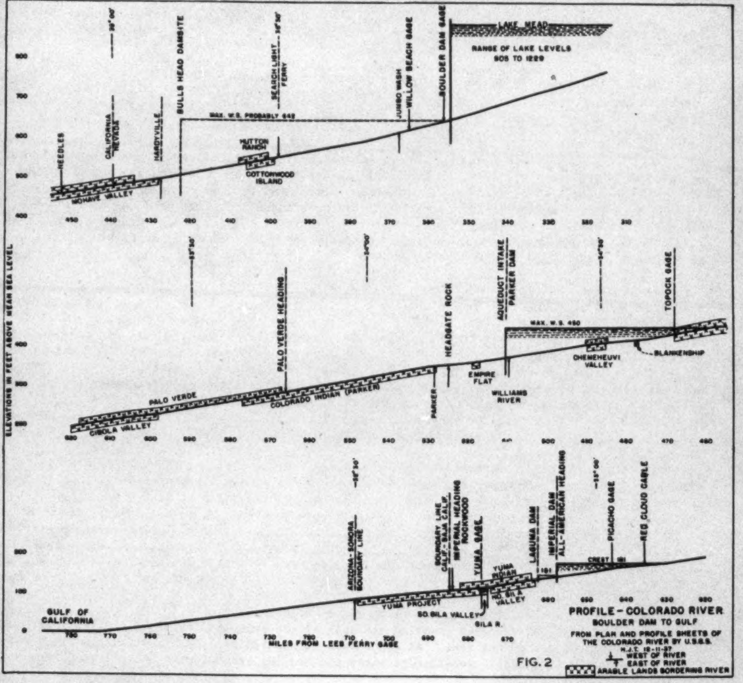
RETROGRESSION AND AGRADATION

ELEPHANT BUTTE DAM TO QUITMAN CANYON

REPORT OF MARCH 1938

OF

INTERNATIONAL BOUNDARY COMMISSION



PROFILE - COLORADO RIVER

BOULDER DAM TO GULF

FROM PLAN AND PROFILE SHEETS OF THE COLORADO RIVER BY U.S.G.S.

FIG. 2

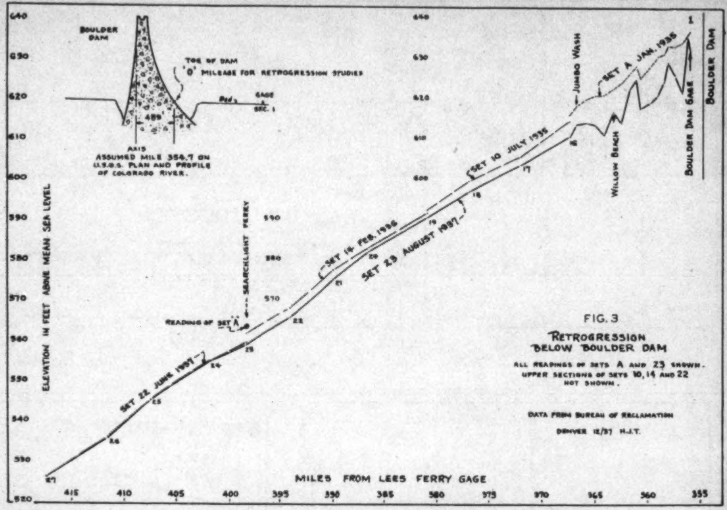


FIG. 3

RETROGRESSION BELOW BOULDER DAM

ALL READINGS OF SETS A AND 23 SHOWN. UPPER SECTIONS OF SETS 10, 14 AND 22 NOT SHOWN.

DATA FROM BUREAU OF RECLAMATION

DENVER 12/37 H.J.T.

changed regimen on this reach of Colorado River since storage in Lake Mead began.

In order to understand what may happen, a profile of the river from Boulder Dam to the Gulf of California is shown in Figure 2. There will be two large dams below Boulder. Parker Dam, 155 river-miles below, is now under construction. Bullshead Dam, 67 river-miles below, is contemplated to create a regulating reservoir for water-releases from Lake Mead whenever the power-demand sufficiently exceeds the irrigation-demand to warrant its construction. This may not be for 15 to 20 years. Imperial Dam, 148 miles below Parker Dam, is nearing completion, and five miles below it is Laguna Dam, completed in 1909.

Parker Reservoir, in spite of its proximity to Lake Mead, will ultimately silt up until its dead storage of approximately half a million acre-feet is exhausted, and establish a gradient of alluvium upstream from the normal limit of backwater. Bullshead Reservoir reaching almost to Boulder Dam will be supplied with clear water and can only fill with silt that may be washed into it from lateral drainage, which obviously will be a very slow process. In the lower end of the Mohave Valley above Parker Reservoir the river-bed will be agraded, requiring extensive diking and probably drainage by pumping before those areas can be reclaimed. In the upper end of the Valley the River may be slightly intrenched. If the new river-gradient can be established between Boulder Dam and Parker Reservoir before Bullshead Dam is built the construction of that Dam will have little effect on the river-channel below it.

The amount of retrogression that has already taken place since February 1935 is shown in Figure 3. Cross-sections have been established below Boulder Dam at intervals of about five miles or less. Cross-sectional areas are determined and bed-samples taken at stated intervals. The profiles show the mean elevations at the river-bed between Set A, taken in January 1935 covering a distance of 13 river-miles, and Set 23 taken in August 1937 covering a distance of 63 river-miles below Boulder Dam. There was some retrogression between Set A of January 1935 and Set 10, the first one below, taken in July 1935; also between Sets 10 and 14 and between Sets 14 and 22, so that the upper line is not quite the true profile prior to the closure of Boulder Dam by-passes.

Note that there are riffles in the river-bed at miles 1.5, 2.4, 6.4, 8.5, and 11.3 below the Dam. Over the first two the mean lowering has been of the order of three feet while between them the bed has lowered nearly ten feet. At Jumbo Wash it has lowered about six feet on the riffle. There are some places in the river-channel where the bed has been lowered 20 feet and over, but they are not evident from the mean bed-elevations. These riffles are probably covered by coarse detritus brought in by side drainage-channels. As the bed lowers more of such debris-cones from side arroyos will doubtless be exposed, and in the absence of scouring floods it will take many years before any semblance of a stabilized gradient for the new regimen can be established. It will rather consist of a series of shallows on the riffles with a lowering of the bed immediately below each, while maintaining the bed above the next one, until the coarse material in it can be corraded sufficiently fine to be carried away, or undermined on the downstream side which in some cases may be a very slow process. Since the storage in Lake Mead began the outflow has been kept at a minimum in order to fill the Reservoir to its normal operating level.

Studies are being made for a diversion-dam in the vicinity of Headgate Rock 15 miles below Parker Dam for the Colorado River Indian Project, the construction of which has been authorized by Congress. If it is built, it will become the head of retrogression for the 133-mile reach between it and Imperial Dam. The area above Imperial Dam will soon silt up to the spillway-crest. The total fall from Headgate Rock to the crest of Imperial Dam is 174 feet in 133 miles, an average gradient of 1.3 feet per mile.

This gradient is commensurate with the past flow-regimen of the River. The average maximum flood-discharge originating in the Colorado River in this reach during the past 36 years was about 100,000 cfs. The flood-peaks of Williams River which enters Parker Reservoir have reached a maximum of 56,000 cfs since 1928; larger floods have undoubtedly occurred. They, however, are winter floods and do not coincide with the spring floods on the Colorado.

The operation of Lake Mead will put the Colorado River floods under virtually complete control and the average annual spring flood-discharge may be of the order of 75,000 cfs. It is anybody's guess as to what the new gradient will tend to become. If there were no change in area, the slope would vary as the square of the discharge but the reduced area will limit that tendency. If we assume the new regimen will tend to a slope that varies as the first power of the discharge, the new slope would tend towards a value of one foot per mile. If it were possible to produce a slope that would be uniform from Imperial Dam to Headgate Rock, it would mean a lowering of the river-bed at the latter point of about 40 feet. It is practically certain, however, that such a uniform gradient can not be established.

In this reach the River is flanked by alluvial bottom lands on one side or both for 98 miles of the distance. Above the Palo Verde Canal Heading, however, there is a canyon-wall on the California side for 38 miles. On the Arizona side opposite the Palo Verde Area is another 21 miles of bank with canyon-walls, and below Cibola Valley there are low canyon-walls on both sides of the River to Imperial Dam. It seems reasonable to suppose that there are a number of debris-cones of coarse material brought in by arroyos in these canyon-walls that are now buried under river-sands that will be exposed as retrogression proceeds.

A series of borings [2] in the bed of the River between Parker and Laguna Dam, however, failed to disclose any important control-points except in the canyon below Parker Dam and near mile 55C of Figure 2. If such debris-cones do exist, some fall will be concentrated at them and thus the total fall between Imperial Dam and Headgate Rock will be greater than indicated, resulting in less retrogression at the Parker end of the reach.

Williams River floods will be partially controlled in Parker Reservoir but any flood bringing in 200,000 acre-feet will exhaust the operating capacity of that Reservoir and thereafter the entire discharge must pass down the River. Williams River flood will therefore tend to steepen the gradient by filling the retrogressed channel at the upper end of the reach. Thus there are two influences operating against the establishment of a uniform flatter gradient above Imperial Dam than now exists.

From these considerations it seems unlikely that a lowering 40 feet at Headgate Rock will occur, but it might reach half this amount.

A bridge has recently been built at Parker [3] five miles below Headgate Rock. It is supported on H-beam piling to depths of 50 to 60 feet in river-bed sands. In the rare contingency that a lowering of 40 feet should occur, it will just be too bad for that bridge, and if even half of it should be realized, the bridge will be left on rather wobbly proxies for its present supports.

Between Imperial and Laguna Dams the river-channel may be built up as a result of the desilting operations at the Imperial Dam. However, this silt must be passed on to the lower river by flushing the channel at intervals.

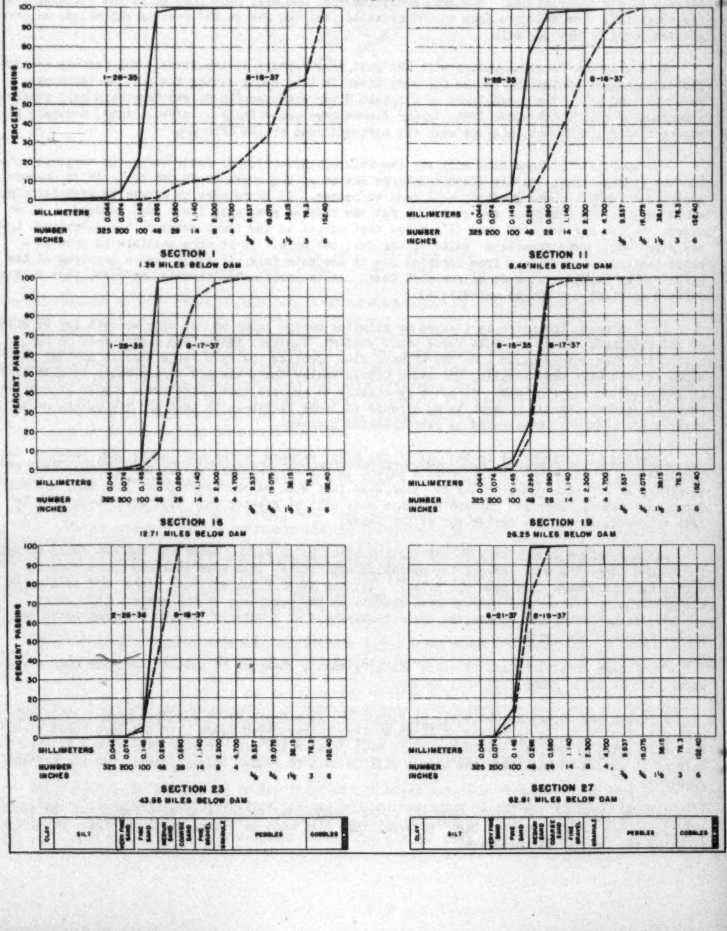
During the low flows of the past four years (1934 was naturally low and the others have been kept low by storage in Lake Mead) only the very finest sediment has been carried through to the Gulf. The ordinary load has been deposited on the surface of the delta [4]. After sufficient storage has accumulated in Lake Mead, mean annual discharges of the order of 18,000 to 20,000 cfs will be released at Boulder Dam of which about two-thirds will pass Imperial Dam until such a time as irrigation-developments can absorb this surplus. Just what will happen in the reach from Laguna Dam to the Gulf cannot be predicted. In the early years of operation, there may be

COLORADO RIVER BASIN

COARSENING OF RIVER MATERIAL BELOW BOULDER DAM

FROM BUREAU OF RECLAMATION DATA ON BED SAMPLES

FIG. 4



SECTION 1
1.26 MILES BELOW DAM

SECTION 11
8.46 MILES BELOW DAM

SECTION 16
12.71 MILES BELOW DAM

SECTION 19
26.25 MILES BELOW DAM

SECTION 23
43.95 MILES BELOW DAM

SECTION 27
62.81 MILES BELOW DAM

some retrogression below Laguna Dam. Ultimately, however, the reverse may be true.

There is over a hundred river-miles of alluvial deposits between Laguna Dam and the Gulf through which the River flows on an average gradient of 1.5 feet per mile. Below Yuma, Gila River must be reckoned with. During the past 35 years it has passed six floods in excess of 60,000 cfs and one peaked at nearly 200,000 cfs. However, ten years may elapse between floods of such magnitudes so that there might be an opportunity for some retrogression in the reach below Laguna Dam between Gila floods in the early years. In later years, however, when all the water available to the United States is being diverted, the reach below Laguna Dam may be built up in the same manner the delta has been built up to its present configuration.

On Rio Grande the statement was made that the silt-problem had been changed to a sand-problem. That will also hold to some degree at least for Colorado River. Figure 4 shows how the bed-material has come to contain a greater per cent of coarse material by removal of the fines in the reach now undergoing retrogression between Boulder Dam and Searchlight Ferry. Note that the effective size (read on the ten-per-cent line) of the bed-material at Section 1 has increased twelve times from 0.09 to 1.10 mm and that this coarsening diminishes downstream.

In conclusion it is well to point out that it will take many years to establish the new regimen, possibly a matter of 30 to 50 years, that no one can predict with accuracy what will happen. All that can be done is to adopt an attitude of "watchful waiting," and in the meantime gather complete physical data and meet each contingency as it arises.

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[3] Eng. News-Rec., December 16, 1937.
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Consulting Engineer,
Portland, Oregon

DISCUSSION

F. C. SCOBEY (U. S. Department of Agriculture, Berkeley, California)--In the last paper on floods turned out by the United States Geological Survey there is listed a flood of 240,000 second-feet in Yuma. That flood should not be charged to the Colorado. It was a flood from the hills. That was in January, 1916. Although measured in the Colorado and listed there, it was a flood of the hills and should not be considered as of the Colorado. It was only a flood of 40,000 or 50,000 second-feet in the Colorado that time. Boulder Dam would have had no effect whatever on that flood.

I think the greatest evidence of a real flood far beyond anything ever measured in the Colorado lay in the gravel-bed used for the material of the Boulder Canyon Dam. The gravel-bed was one-half mile long, one-quarter mile wide. Those of you who were down there during construction saw the shovels working where there was a face exposed 20 to 25 feet high. There is not a single break in that face. In other words, that gravel-bed must have come out of that Canyon all at one time. It is an absolutely homogeneous bed of tremendous volume that could have only come from one flood. Walker Young told me that they had given up hopes of finding material for Boulder Dam until this bed was found. This particular bed had just enough gravel to complete Boulder Dam.

N. E. A. HINDS (University of California, Berkeley, California)--I might say that the phenomenon of these great gravel-beds is to be found all along the way of the Colorado. In the Grand Canyon and various other places, we have this demonstration of apparently short-time rush of great volumes of gravel under what we might call a superflood. I am very much of the opinion those were accomplished during the snow-melt of the last glacial period.

C. J. KRAEBEL (U. S. Forest Service, Berkeley, California)--The flood of 1861 on the Willamette in Oregon has been mentioned as well as the flood of 1862 in California. This may be a matter of dating 1861-62 or 1862-63; but as we have been going up and down California attending flood-control hearings in the past six or seven months, we have heard the same story over and over again in almost every community, that within the lifetime of some of the men present at those hearings the rivers in their locality have risen to flood-stages by reason of the fact that the flood-beds have become silted up. They cite river-heights of 40, 50, 60, or even 70 years ago as compared with river-heights of today. The old-timers give us a picture of what the canyons looked like at certain bridges where they fished. Since their youth these places have become silted. A river of sand extends miles up and down stream. There has been change in the condition of the watershed-logging, followed by conversion from timber-type to grazing land. Presently overgrazing brings about a whole new cycle of erosion in that watershed which brings down a continuous stream of sand. In many rivers that sand-stream in motion amounts to hundreds of thousands of yards per year. It will continue to cause trouble for perhaps a century or more even though we do control the headwaters.

It is the phenomenon or practice of man's use of land which must be taken into consideration in any flood-control plan. I should like to see some consideration of treating the matter at its source. It is quite customary to belittle that factor. But I think the time has come when we have got to recognize it. If we are going to build costly works in the lower region of rivers, build levees and dams, we must provide for maintenance of these structures and not expect to write them off in a matter of 100 or 200 years. That is not long enough for modern communities. We must give some attention to the watershed if nothing more than to make an investigation to see if conditions warrant building these costly structures downstream.

F. E. MATTHES (U. S. Geological Survey, Washington, D. C.)--May I add a word to Professor Hinds' remarks. He has given us an excellent perspective of the relative magnitude of floods of the present time as compared with those of glacial times, and has shown us on the screen some very telling examples from California and other parts of the western United States. I believe we can find even more telling examples here in California in some of the gravel-deposits at the foot of Sierra-Nevada Mountains which are now being dug up by great hydraulic dredges such as those near Marysville. If you see the material with which those dredges are working in a very nearly flat country mined out from the base of the Sierra-Nevada Mountains, you wonder at the extreme coarseness of the average boulder, which probably measures about one foot in diameter, with plenty of them running up to two or three feet in length. That material is out-wash from ancient glaciers carried down at the time when the glaciers melted. The material goes back 10,000 to 15,000 years, to the end of the glacial epoch.

What kind of material do these same streams carry today? You will see streams carry today even in superfloods boulders two or three feet in length. The present-day upstream-channels consist mostly of sand and fine gravel. Just recently the superflood in the Sierra Nevada broke all records so far as the white man can remember. It tore out buildings and bridges in Yosemite Valley and washed out modern highways and railroads long distances down into Merced Canyon. It is interesting to see now whether that superflood has carried any material comparable to that of the glacial epoch. I recommend to any of you who want to make a comparison of the superfloods of glacial times and those of today that you make a study of the material which we are digging up at the present time.

J. W. GROSS (Consulting Civil Engineer, Sacramento, California)--For the point of clarification of data on California storms and to identify the storm-period, it might be well if at this time we went a little into detail as to the dates when those particular floods occurred. Records show that the flood of 1861 began on December 10. The flood of 1862 in this locality--the Sacramento Valley--was at its maximum on January 10. Throughout the whole period the whole coast was in a state of storm. When you identify the storms of the Willamette with the storms of the California Area, you may be sure they were one and the same storm.

Chairman **B. M. WOODS** (University of California, Berkeley, California)--One of the most interesting observations made to me in Washington this fall is to the effect that probably the use of advisory committees and the inter-relationships among governmental committees had increased at a greater pace in the last few years than in any period in the country. I call upon Dr. R. E. Horton to say a few words about the Soil Conservation Service Research Advisory Committee.

R. E. HORTON (Consulting Hydraulic Engineer, Voorheesville, New York)--I wish to convey very briefly a message from the special Committee which has been appointed by the Section of Hydrology of the American Geophysical Union as an advisory committee to the Soil Conservation Service. A number of us felt that a situation had arisen which was somewhat critical. Dr. Lowdermilk requested that the Executive Committee of the Section should appoint a committee to act as an advisory committee, as follows: L. K. Sherman, Chicago, Illinois; J. E. Church, Reno, Nevada; A. F. Meyer, Minneapolis, Minnesota; W. W. Horner, St. Louis, Missouri; F. J. Veihmeyer, Davis, California; C. M. Saville, Hartford, Connecticut; R. N. Towl, Omaha, Nebraska; R. E. Horton, Voorheesville, New York, Chairman.

That Committee has perhaps no very definite function, but as I see it we have two functions, which I wish to explain. One is to help others more competent and informed to get into a position where they can answer intelligently and reliably questions which may be asked as to what the Research Division of the Soil Conservation Service is doing and why it is being done. Another function is this: The Soil Conservation Service started out more or less on an emergency-basis and established an extensive series of research-experiments, spending a considerable amount of money in developing them. It went ahead with quite an extensive program of soil-conservation with arrangements for payments to the farmer. We have nothing to do with that. It is probably unfortunate for us that those things were all grouped together with the Research Division. It gives us an additional hurdle to jump because many persons think, at least in the East, it is a thing of the farm. They know nothing of the research-aspect of it. More seriously than that, it is very natural for men who seem well informed, whether they agree with it or not, to ask this question, "Why, the Soil Conservation Service has gone ahead in my State on an elaborate program. You mean to tell me that they did not know what they were doing? If they did know what they were doing, what is the need for such elaborate research?" I know you have met these questions.

I regret that time will not permit me to go, even to the slightest extent, into the question of why hydrological research is needed. I know you have more or less information as to the reasons. If I had time, I could give you some of them.

The experiment station has a number of places where they collect data. I would be very pleased if you would make it a point to visit these stations and become familiar with the work they are doing. If anything occurs to you in the way of things that should be done or should not be done, Dr. Lowdermilk or I shall be glad to receive your suggestions.

The stations where research-projects are being undertaken at which runoff measurements are made are located as follows (at those marked by the letter "a" experiments are not yet established): Guthrie, Oklahoma; Temple, Texas; Hays, Kansas; Tyler, Texas; Bethany, Missouri; Statesville, North Carolina; Pullman, Washington; Clarinda, Iowa; La Crosse, Wisconsin; Zanesville, Ohio; State College, Pennsylvania; Ithaca, New York; Safford, Arizona; Gallup, New Mexico; Spartansburg, South Carolina; Athens, Georgia[a]; Dixon Springs, Illinois; McCredie, Missouri[a]; Beemerville, New Jersey; Marcellus, New York[a]; and Amarillo, Texas[a].

In addition to that message, I wish to say this. This Committee of the Section of Hydrology is a special one. Sooner or later it will complete its work and fold up its tent. Certain research-work is carried out by a number of development-agencies--the Forest Service, Geological Survey, Tennessee Valley Authority, the Weather Bureau, and perhaps other organizations. I have talked to the chairman about it and with Dr. Lowdermilk, and I am sure we would all of us be glad if the Forest Service and others would cooperate with an advisory committee of the Section of Hydrology to be appointed. They do not have to follow our suggestions. That would enable them to get the benefit of whatever may be in the Soil Conservation Service which would be helpful in getting something we very much need.

F. C. SCOBEY--Those of you who are members of the American Society of Civil Engineers will recall that Dr. Grover of the United States Geological Survey had a paper recently on the flow of silt through Lake Mead. That, at first, was thought to be unprecedented, but was found to be quite common on the Rio Grande at Elephant Butte Reservoir. In some of the discussion since that paper was published, it has been suggested that if more of that were done it would solve the problem of getting the silt out of the Colorado River. The question arises as to whether it would be advisable to get the silt out of the Colorado. You have some ten or twelve large sites upstream that can be developed in the future. The life of the Reservoir has been estimated to be about two or three hundred years. Would it be advisable to test it out on the stream?

It seemed highly desirable to get the Colorado in a state of regimen below the Dam because there is no set-up now for any material flow below Yuma. Yuma is a critical point. It is the last town before you get into northern Mexico. If the Metropolitan Aqueduct and the Imperial Canal desilt their water, what would become of the river-bed at Yuma and below? You see the problem that confronts us at Yuma. So, it would seem better to hold the silt that comes to the Reservoir than to bring it out onto the flood-zone. This particular case allows room for thought.

EVAPORATION AND RUNOFF FROM SNOW IN THE ALPINE ZONE OF OUR WESTERN MOUNTAINS

FRANÇOIS E. MATTHES

(Summary)

In this informal paper the processes of snow-wastage at high altitudes were discussed and a number of slides illustrating suncups and sunpits in different stages of development were shown. It was stressed that these features are peculiar to the region above the timber-line, which biologists term the Alpine, or Arctic-Alpine, Zone. That regions is, as a matter of fact, primarily and fundamentally a physiographic zone distinct from all other physiographic zones at lower levels by reason of an unusual combination of climatic, hydrologic, and geologic factors.

Owing to low air-temperatures all the year round (freezing temperatures being frequent even in midsummer) but a small proportion of the snow-pack in the Alpine Zone is transformed into melt-water. On the other hand, because of the intensity of radiant solar heat at high altitudes, wastage of snow by direct evaporation is very great. This is so especially on those of our western mountain ranges that have a relatively dry summer climate.

Radiant solar heat tends to accentuate the irregularities of the snow-surface, and if permitted to act without interference for several days in succession, produces shallow cup-shaped hollows (suncups). These under favorable conditions gradually deepen to roughly cylindrical pits several feet in depth (sunpits). The jagged pinnacles of crystalline snow that remain standing between the sunpits are the familiar nieve penitente described by explorers and mountaineers. Suncups and sunpits may be seen on our western mountains every summer; nieve penitente, on the other hand, are relatively rare.

The runoff from snow-fields in the Alpine Zone decreases markedly toward the higher levels. Above altitudes of 11,000 to 12,000 feet, according to latitude, melt-water is so scanty that it forms no channel-making streams or streamlets. Above 13,000 feet the snow-fields yield but small trickles that do not reach the valleys below. It follows that these upper snow-fields are in no sense the sources of streams and are to be excluded from any estimates of runoff based on snow-surveys.

U. S. Geological Survey,
Washington, D. C.

RECENT STUDIES ON QUALITY OF WATER FOR IRRIGATION

Carl S. Scofield

Introduction

The title assigned to me begins with the word recent. I have interpreted this as confining me to the present century. Thus I have not here included references to the work done in this field by Professor Hilgard and his associates here in California nor to the work done here and elsewhere in the West by the staff of the United States Geological Survey--all prior to 1900. For reasons of convenience I have elected to discuss the work on quality of irrigation water on the basis of drainage-basins. This has some disadvantages because some of the work has not been so classified or reported. For example, one of the earlier and very valuable studies, made by the United States Geological Survey in 1905 and 1906, and reported in Water-Supply Paper 274, is arranged alphabetically by the names of the streams and tributaries.

I shall not attempt to catalogue all of the work that has been done on this subject even in recent years. Some of this work has been incidental to hydrological or other investigations or has dealt chiefly with water-quality for domestic or industrial uses rather than for irrigation. I shall try to review briefly such work as has come under my own observation during the last 30 years. That review will merit consideration more for what is omitted than for what is included. By that I mean to point out that for the irrigated West as a whole our information about the quality of water is woefully inadequate. I shall list and describe briefly the work that has been done in five or six drainage-basins. The other 40 or 50 natural subdivisions are not mentioned because there is nothing to say.

This is, of course, only partly true. I know it to be a fact that water-analyses have been made for streams and basins I have not mentioned. Nearly every experiment station in our western

States has been for years making water-analyses. Many private or service laboratories also make such analyses, often in large numbers. I think I am safe in estimating that the number of water-analyses made but not published exceeds by a good deal the number of those made and published. This seems to me unfortunate. It costs something, often several dollars, to collect a water-sample, to ship it to a laboratory, and to make an analysis. That may be and probably is worth doing even if the results are not published. But I think there can be no question that it would be worth much more if the location data and the analytical results were published and thus made available to others.

In the review here presented reference has been omitted concerning a number of recent studies of water-quality because the results have not been and are not likely to be published. Similarly some excellent current work goes unmentioned for the same reason. There are several reasons why such work is not published. For example: (1) Some people consider the information confidential; (2) location-data are lacking or inadequate; (3) the analytical methods used, for reasons of economy, are not regarded as accurate enough to publish; (4) effective publication often calls for interpretive or explanatory discussion and that is not always available.

These and other reasons seem valid enough. But the fact remains that there is in the aggregate a lot of valuable information on the quality of irrigation-water that is now serving no useful purpose because it has not been published.

Methods of sampling and of analysis

Because of the fact that recent studies of the quality of irrigation-water have been made by a number of different agencies and often with different objectives, it is probably not surprising that different methods of sampling and of analysis have been used. Also it may be noted that very different systems have been followed in reporting analytical results. To anyone confronted, as I have been, with the task of assembling for one report the data from several laboratories relating to one area, it seems much to be desired that there should be more uniformity of methods. On the other hand, it is not to be denied that there are disadvantages and a real hampering of progress in too much insistence on uniformity of methods. I would rather see such work published even with the greatest diversity of method than to do or say anything that would tend to restrict such work and its publication because it failed to conform to certain standards.

Following this disclaimer let me say that I think there is room for improvement in this matter of methods. First as to location-data. The fundamental requirements are simple but it must be admitted that they are too seldom fulfilled. These are: (1) Where was the sample taken? (2) when was it taken? (3) who took it? and (4) what does it represent in volume or rate of flow? Each of these merits more elaboration than I shall take time to indulge in. Every collector should keep in mind that he owes it to his colleagures to record this essential information. Without such information the subsequent analysis is of limited value or may be quite worthless. It is my firm conviction that more laboratory-work is wasted or rejected because of insufficient data as to location and description than for any other reason. As a matter of fact we refuse to analyse water-samples unless they are accompanied by complete and acceptable descriptions.

As to methods of analysis the current deficiencies are less acute or deplorable than with sample-descriptions. But there are some deficiencies. One of the most striking and general is the failure to determine the total concentration of the dissolved solids. There are several methods of doing this. One is the electrometric method; another is the gravimetric method. Neither of these is very expensive and they are both fairly accurate and give significant results. It seems to me that if a water-sample is worth taking at all it should be worth an accurate determination of total concentration--not only because that is an important characteristic in itself but also because it affords a valuable check on the significance of the detailed analysis. In respect to the composition of the dissolved solids or what we call the detailed analysis, it must be admitted at once that different investigators have different objectives and will have different ideas of what constituents are important and should be determined. No one seriously proposes that routine detailed analyses should be "complete." There are too many dissolved constituents in natural waters.

For most routine detailed analyses of irrigation-waters, I should like to see at least six or seven constituents determined, namely, calcium, magnesium, and sodium for the bases, together with the carbonate-bicarbonate, sulphate, chloride, and nitrate for the acids. In some situations the determination of boron is indicated but its natural occurrence in potentially injurious concentrations appears to be confined to limited areas. It may be remarked that I have not included in this list, silica, iron, alumina, and potassium; nor have I mentioned the determination of the hydrogen-ion concentration. These are welcome additions to the list and in some situations they may be important. This may be said also for fluorine and selenium. But these determinations add to the cost of laboratory-work and my aim here is to point out what I believe to be the most essential requirements for a detailed analysis of irrigation-water.

Possibly I ought to say something about methods of reporting water-analysis. But this is a provocative subject, rather like religion or politics. I have some very definite ideas as to the methods of reporting analyses and as to computing from detailed analyses certain significant characteristics of irrigation-water such as the percentage of sodium. I could discourse at length on methods of computing salt-tonnage and salt-balance and of comparing waters of one type with those of another. But the subject assigned to me is recent studies on quality of water for irrigation. Probably I would do well to proceed with that.

The boron-survey

The discovery, by Dr. W. P. Kelley, of the occurrence of boron in toxic concentrations in certain irrigation-waters in California led to an investigation which involved the quality of irrigation-water in areas where boron was known or believed to occur. This survey was not limited to boron but included the measurement of the concentration of the total dissolved salts and the determination of a number of the dissolved constituents. In some areas and in some drainage-basins the investigations as to water-quality were fairly detailed and comprehensive and included both surface- and ground-waters. In other areas or drainage-basins where boron was not found in toxic concentrations the surveys were not completed.

The boron-survey served a useful purpose, however, in relation to the general subject of the quality of irrigation-water. In connection with it progress was made in the technique of collecting water-samples, making analyses, and reporting the findings. As the survey went forward it became apparent that while boron was an important constituent in determining water-quality in some situations. there were many other situations in which other constituents or characteristics were dominant. The boron-survey was useful also in focusing the attention of irrigators, engineers, and credit agencies on the subject of the quality of irrigation-water in relation to the sustained productivity of irrigated land. It soon became apparent that while in some situations the accumulation of boron was injuring crops there were many other situations in which crop-injury was caused by other constituents of the water.

Meanwhile investigations of the phenomenon of base-exchange in the soil, conducted by the University of California under the leadership of Dr. Kelley, and similar investigations by other institutions were showing that the sodium-constituent of irrigation-water is potentially injurious to the physical condition of the soil. This finding pointed to the need of taking into account, in water-quality investigations, not only those constients that may be directly injurious to crop-plants but also the sodium-constituent. The present trend of thinking is that the adverse effects of sodium are conditioned by the associated and relative concentrations of calcium and magnesium. This has led to the inclusion in water-quality reports of the criterion of sodium percentage.

The quality-of-water findings of the boron-surveys have been published in two reports: (1) Boron in irrigation-waters [Tech. Bull. No. 264, U. S. Dept. Agric., November, 1931] and (2) Boron in soils and irrigation-waters and its effect on plants, with particular reference to the San Joaquin Valley of California [Tech. Bull. No. 448, U. S. Dept. Agric., February 1935].

A third report now in preparation deals with the quality of water in the drainage-basin of San Benito River in California.

The South Coastal Basin of California

In connection with a comprehensive investigation of hydrological conditions in the South Coastal Basin of California, conducted by the State Division of Water Resources, it was decided in 1931 to include the quality of water. The South Coastal Basin includes the drainage-areas of the Los Angeles, San Gabriel, and Santa Ana rivers. The investigation as to water-quality included not only the surface-waters of these streams and their tributaries but also water imported by the Los Angeles Aqueduct from Owens River and the ground-waters of the Basin that are extensively developed for irrigation purposes as well as for domestic and industrial purposes. The results of this investigation were reported by the Division of Water Resources, Department of Public Works, State of California in Bulletin No. 40, South Coastal Basin investigation, quality of irrigation-waters, 1933.

It may be observed in passing that this critical diagnosis of quality-of-water conditions in the South Coastal Basin as of 1931-32 served a useful but limited purpose. The conditions in that

Area are not static and are not in equilibrium. Changes are going on progressively as more land is developed, as towns and industries expand, and as additional water is imported from other drainage-basins. The survey of 1931-32 is useful chiefly as a frame of reference. It should be supplemented by a continuing program of observations at representative stream-stations and at representative wells to record the trend and rate of the changes that are occurring and further by another comprehensive survey of the Area in the not-too-distant future. Fortunately this need has been appreciated and arrangements have been made by University of California in cooperation with the State Engineer's office to carry on this continuing program of observations.

The Colorado River

The Colorado River is one of our more important streams from the standpoint of irrigation. Its drainage-basin occupies nearly a quarter of a million square miles, having an annual runoff of some 16 million acre-feet of water. With much of its basin in the arid region it requires no great stretch of the imagination to conceive that all of this water might be appropriated for irrigation. There is even now some concern as to whether there is enough water in the Basin to serve all of the irrigable land. This situation makes it pertinent to consider the question of water quality. If all of the dissolved salt, amounting to some 13 million tons annually, carried by the stream is diverted to contiguous irrigated lands instead of into the Gulf of California, its ultimate destination may have to be considered. In any event it has seemed justifiable to get the facts in the case and make them available for engineering consideration.

As early as 1900 Professor R. H. Forbes, of the University of Arizona, made a study of salinity-conditions at the Yuma Gaging station based on daily samples for a year. His findings were published in Bulletin 44 of the Arizona Experiment Station. In 1905 another year's study was made at Yuma by the United States Reclamation Service under the direction of Thos. H. Means. This was reported by Herman Stabler in Water-Supply Paper 274, of the United States Geological Survey. In 1925 the United States Geological Survey began another investigation, this time at the Grand Canyon gaging station. Daily samples have been taken at this Station since 1925. Samples have also been taken from other stations on the main stream and some of its tributaries. Progress-reports on this investigation have been published by the United States Geological Survey in Water-Supply Papers 596, 636, and 638 covering the period 1925 to 1930, inclusive. It is my understanding that the United States Geological Survey now has in preparation a comprehensive report to include all available analyses of the waters of the Colorado Drainage-Basin.

Since 1928 the United States Bureau of Plant Industry in cooperation with the Bureau of Reclamation has been analyzing samples of Colorado River water as obtained at the Yuma gaging station each week, and also as diverted from the stream at Laguna Dam above the junction of Gila River. Also for approximately the same period weekly samples have been taken of the drainage-water discharged by pumps from the drainage-system of the Valley Division of the Yuma Reclamation Project. These samples of the irrigation-supply and of the drainage-discharge of the Valley Division together with data as to the weekly volumes in each case have made it possible to compute the annual quantities of water and of dissolved solids delivered to and discharged from the irrigated areas of the Valley Division. These computations show, among other things, how much water is consumed by or dissipated from the irrigated area and the difference between the input and the output of dissolved solids. This latter relationship has been designated the salt-balance of the area.

During the period 1930 to 1933 a similar investigation of salt-balance conditions was made for the Imperial Valley in cooperation with the Imperial Irrigation District. For this investigation sampling stations were established on New River and Alamo drains just above Salton Sea, at which weekly samples were taken. The quantity of salt-input was computed from the samples taken at the Yuma gaging station together with weekly reports by the Irrigation District on the quantities of water crossing the International Boundary into Imperial Valley. The findings of these two salt-balance investigations have been reported in mimeographed form as letters to the Bureau of Reclamation and the Imperial Irrigation District.

The construction of a dam in Black Canyon to impound the flood-waters of the Colorado has changed the regimen of the stream below the Dam. These new conditions are now under investigation both in the Reservoir itself and along the stream below the Reservoir. This investigation includes not only the concentration and composition of the dissolved solids but also the quantity and composition of the sediments carried, moved, or deposited by the Stream. These investigations are being sponsored by several agencies and have not, as yet, been coordinated although action to that end is under consideration.

The Rio Grande investigation

The Rio Grande like the Colorado is an important irrigation-stream though smaller. From its drainage-basin of 175,000 square miles in the United States and Mexico it collects some five million acre-feet of water and enough dissolved salts to cause concern along the middle and lower sections of its course. The burden of sediments carried into the middle section of the Stream in New Mexico is also potentially troublesome. The Rio Grande is an international stream but unlike the Colorado its channel marks the international boundary for a long distance and a substantial part of its water-supply is contributed from the Mexican portion of its drainage-basin.

In its upper basin, that is, above its international section, irrigation-development has already reached the point of utilizing most if not all of the available water-supply. In fact, this development has gone so far that the water available for use on the lower lands of the upper basin is composed largely of drainage returned to the stream from the irrigated lands lying farther upstream. This condition has given added interest to the quality of the water and this subject has been under intensive investigation for a number of years past by various agencies.

Certain questions in respect to the allocation of the waters of the Rio Grande among the three States of the upper basin have recently become acute. With a view to meeting the needs of this situation the National Resources Committee in cooperation with the three States has set up and implemented the Rio Grande Joint Investigation of 1936. One part of this investigation has had to do with the quality of the waters of the Basin. The scope of this part of the work included not only the investigations of 1936 but the bringing together for a report of all the available and acceptable water-analyses not hitherto published. The report on this phase of the investigation consists of two parts. One part includes the analytical data now in course of publication by the United States Geological Survey as a Water-Supply Paper, and the other an interpretive report now in course of publication as Part IV of the report of the Rio Grande Joint Investigation.

The Big Horn Basin of Wyoming and Montana

This stream is a tributary of Yellowstone River and thus a part of the Missouri River System. It drains the Eastern slopes of the Rocky Mountains in Wyoming and flows northward to its junction with the Yellowstone in Montana. Its drainage-area of some 20,000 square miles above its lowest gaging station contributes some three million acre-feet of water annually. Under natural conditions its water is not saline, being derived chiefly from the snow of the high mountains. However, the irrigable lands contiguous to the tributary streams include numerous outcroppings of laminated salt-bearing shales. Where these shales are cut into by irrigation-canals the seepage-losses are large and extensive artificial drainage becomes necessary. The drainage-waters returned to the main stream are often highly saline and their subsequent diversion for irrigation use is potentially detrimental to the lower lands.

The continuing development of irrigated areas in the Big Horn Basin involves more extensive use of the contaminated water-supply and in turn still further contamination. The situation has already become sufficiently acute to warrant the inauguration of an investigation of the quality of the water of the main stream at its four gaging stations. This investigation, which is cooperative between the States of Wyoming and Montana and the United States Geological Survey, has not yet progressed to the stage of yielding reports. Nor has it progressed, as it should to include stations on several of the important tributaries of the Stream above and below the irrigated areas. It may become necessary to localize the sources of salinity-contamination and to devise ways of diminishing or preventing it in order to utilize more completely the limited water-resources of the Basin.

The Central Valleys of California

The great central trough of California lying between the Coast Range and the Sierra Nevada is not supplied with rain uniformly throughout its length. The north end, drained by the Sacramento River, has a surplus of water while the south end is deficient. One of the objectives of the Central Valleys Project is to improve upon this natural condition by supplementing the water-supply to the southward from the available surplus from the north. The most practicable method of achieving this end involves extensive and complex readjustments of the existing systems for distributing irrigation-water and the substitution of new supplies for those currently in use. One of the questions brought up in this connection relates to the quality of the water to be substituted as compared with that heretofore used.

Interest in this subject is not limited to surface-waters because throughout the southern part of the area ground-waters are extensively utilized for irrigation and these ground-waters are replenished from surface-supplies. What is here implied is that as the Central Valleys Project progresses it may involve extensive and detailed hydrological investigations of which the quality of the irrigation-waters may be an important part. Unfortunately there has not been made heretofore any adequate investigation of existing conditions. In 1905 and 1906 some of the stream-waters were analysed by the United States Geological Survey and reported in Water-Supply Paper 274. In 1916, Mendenhall, Dole, and Stabler reported on the ground-waters of the San Joaquin Valley in Water-Supply Paper 398. Again in 1935 Dr. Eaton reported a number of analyses of surface and ground-waters in Technical Bulletin No. 448 of the United States Department of Agriculture.

It is my understanding that the Water-Project Authority of the State of California has begun recently to assemble all of the available data on quality of water for the Central Valley area and to obtain additional analyses where essential information is lacking. It is to be hoped that a way may be found to publish these analytical data with adequate location-descriptions and to publish also an interpretive summary or digest. Such reports have value not only in dealing with immediate and current problems but even greater value for the future as bases of reference from which to measure trends and rates of change with the passage of time.

U. S. Bureau of Plant Industry,
Riverside, California

SOUTH CONTINENTAL DIVIDE REGIONAL MEETING, DENVER, COLORADO, JUNE 21-24, 1937
(ADDENDUM TO PART II, TRANSACTIONS OF 1937)

EUROPEAN HYDROLOGIC ACTIVITIES

Samuel Shulits

Introduction--In a five-year period prior to 1933, the author visited many hydrologic bureaus in Central Europe, studying their aims and methods. This paper is a brief summary of the activities of these organizations. Considerable use has been made of an excellent article by Ing. A. Rundo, entitled "Die hydrographischen Institutionen Europas, deren Organisation und Taetigkeit" (in the Wasserkraft-Jahrbuch of 1928-29), to supplement information obtained by the author.

Common activities, such as the collection and recording of precipitation, stream-gaging, and evaporation-data, are not mentioned, for most countries procure this kind of hydrologic data. A quantitative tabulation of the extent to which such statistics are gathered in Europe is contained in "Deficiencies in basic hydrologic data" of the National Resources Commission.

General statement--The following countries engage in no unusual hydrologic activities: Spain; Yugoslavia; Roumania; Lettonia; Holland; Belgium; England (not Great Britain); Austria; Czechoslovakia; Hungary; Finland; Norway; Poland; Estonia; and Latvia. They will be treated cursorily here, if mentioned at all.

The more active countries hydrologically are France, Germany, Switzerland, Sweden, Italy, and Russia.

Description of activities

England--The hydrologic activities of Great Britain are almost entirely in India, Egypt, and the Dominions, little being done in England itself. A committee of the London Geographical Society makes hydrologic studies of England's rivers, published periodically in the Geographical Journal.

Austria--The Hydrographic Central Bureau in Vienna and local hydrographic organizations are in charge of the work, which includes a flood-alarm service.

Czechoslovakia--The Federal Hydrographic Bureau in Prague carries out systematic bed-load measurements in rivers.

Finland--The Hydrographic Bureau collects data on tides, water-power, and lake-evaporation. It also studies the long-range forecasting of flood and low stages by correlation-methods.

Norway--In this land of "white coal," hydrology is important primarily for the evaluation of water-powers. Consequently, hydrologic investigations are made mainly by the Bureau of Water Power and Electricity.

Poland--The Polish Central Hydrographic Bureau observes water-temperatures, ground-water conditions, winter flow (effect of ice-cover on discharge), and swamp-evaporation. It conducts limnologic investigations, surveys of developed and undeveloped water-powers, and a river-stage alarm-service.

France--The mother of the science of hydrology, France, has a Bureau of Hydrology and Flood Warnings for each of the river drainage-basins (Seine, Loire, Garonne, Saône, Meuse, and the Rhone tributaries). The most important duty of this Bureau is the investigation of flood-conditions and the prediction of crest and time of culmination of floods. Reports of stage are distributed telegraphically, by mail, and by radio to various officials and bureaus. The organizations is under a permanent commission composed of high officials of the ministries of Public Works, Interior, Agriculture, and Commerce, and of representatives of navigation-interests. There is also a bureau of Large Water Powers to study rivers, mountain streams, lakes, and glaciers in Southern France from the hydrologic, orographic, and meteorologic points of view for the utilization of water-power. A provisional commission exists to investigate the cause of Seine floods and to determine preventive measures.

Germany--The provinces of Germany, Baden, Prussia, Bavaria, Hesse, Saxony, Thuringia, and Wurttemberg each have a Bureau of Hydrology. There seems to be no coordinating or central bureau.

To save space, the activities of these provincial bureaus will be enumerated together, though they may not be common to all. These cover: Ground-water-table observations by means of extensive systems of wells; flood-studies and flood-warning services; measurement of temperature of water and soil; evaporation-investigations; ice-run observations; studies of seepage, ground-water flow and springs; measurements of bed and suspended load.

The Bavarian Bureau of Water Supply acts as consultant to communities in the design and installation of water-supply systems. There are hydraulic laboratories for model-studies, in particular, The Research Institute for Hydraulic Engineering and Water Power near Munich. Extensive scientific hydrologic studies are planned here on the drainage-basin of the Walchensee (Walchen Lake) for this area contains all kinds of terrain.

Though not part of the hydrologic institutions the following should be mentioned because they cooperate--particularly in industrial regions: Research Institute for Water Supply and Sewage Disposal and the Prussian Institute of Water, Soil and Air Hygiene.

Switzerland--The growth of the Swiss Bureau of Hydrology is closely linked with the development of the Swiss water-powers. Accordingly it issues publications in four categories for nine rivers: Drainage-areas; river gage-stations and stage-relations; river-profiles; and minimum flow and minimum water-power. There are also bulletins on developed and undeveloped water-powers. Some unusual activities of this Bureau are studies of the titration and moving-screen methods of measuring flow, studies of velocity-formulas and roughness-coefficients, and bed-load investigations.

Sweden--The Swedish Meteorologic-Hydrographic Bureau observes water-temperatures, the chemical composition of water, and makes ice-crust measurements. The tides in the Baltic Sea are also studied. Other important investigations are the periodicity of lake-level fluctuations, long-range forecasting of river-stages, precipitation and runoff by correlation-methods, influence of reforestation and swamps, ground-water observation by the Swedish Geological Institute, and water-power surveys by the Water Power Commission.

Italy--The Italian Hydrographic Service consists of river, sea, meteorological, geodetic, and geologic divisions. The duties include: Stage-warning service; measurements of tides and ocean-currents; surveys of coast and gulf; compilation of water-power statistics; permeability-tests of rocks; water-temperature observations; lagoon-research; bed-load studies; ground-water observations; determination of profiles of bed and water-surface of rivers; surveys of natural and artificial reservoirs; surveys of surface- and ground-waters available for irrigation. A hydraulic research laboratory is part of the Italian Hydrographic Service.

Union of Soviet Socialist Republics--The Russian Hydrologic Institute has ten main divisions: Applied mathematics; hydrophysics; hydrochemistry; hydrobiology; stream-gaging; hydrodynamics; rivers and lakes; limnology; ground-water; talassology. In addition there are the following auxiliary departments: Computations; ice-investigations; hydrologic forecasting; geodetic service; bed-load studies; sweet- and mineral-water investigations; moor- and swamp-studies; laboratories; and a hydrologic bibliographical center. The Russian Hydrologic Institute is the coordinating organization. Nevertheless the hydrologic activities are mainly decentralized, due perhaps to the individualistic, though very scientific nature of the Russian character. The size of the country is also a factor.

River-investigations are conducted by the regional offices of the Commissariat of Transportation and by the Electrotechnical Bureau (through its regional offices) for the development and design of hydro-electric projects.

Limnologic investigations are made sporadically by various bureaus and societies.

Hydraulic laboratories are part of the Hydrologic Institute, the Reclamation Institute, the Transportation Institute, and the Central Aero- and Hydrodynamic Institute.

Some special activities are: Ice-research which has apparently refuted Barnes' theories; water-power statistics; flood-investigations; studies of headwaters and estuaries; and oceanography.

Colorado School of Mines,
Golden, Colorado

NATIONAL RESEARCH COUNCIL

TRANSACTIONS OF THE AMERICAN GEOPHYSICAL UNION

NINETEENTH ANNUAL MEETING
APRIL 27 TO 30, 1938
WASHINGTON, D. C.

REGIONAL MEETINGS
DECEMBER 28 TO 29, 1937, SPOKANE, WASHINGTON
JANUARY 7 TO 8, 1938, DAVIS, CALIFORNIA
JUNE 21, 1937, DENVER, COLORADO (ADDENDUM)

REPORTS AND PAPERS
WESTERN INTERSTATE SNOW-SURVEY CONFERENCE
JANUARY 8, 1938
DAVIS, CALIFORNIA

PUBLISHED BY THE NATIONAL RESEARCH COUNCIL
OF
THE NATIONAL ACADEMY OF SCIENCES
WASHINGTON, D. C.
AUGUST 1938

WESTERN INTERSTATE SNOW-SURVEY CONFERENCE

JOINT REGIONAL MEETING WITH

SECTION OF HYDROLOGY

Davis, California
January 8, 1938

Reports and papers

(James E. Jones, Local Chairman, presiding)

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James E. Jones, Chairman

THE EXPANDED PROGRAM OF THE UNITED STATES WEATHER BUREAU IN SNOW-WORK

Merrill Bernard

The Weather Bureau is entering its fortieth year of mountain-snowfall reporting for the benefit of the water-users in the Rocky Mountain States. Publication of the reports of Climatological Data was begun in the winter of 1898-99; and in the winter of 1899-1900 separate snowfall-bulletins were published at the close of each winter month.

According to the files of the Engineering News-Record, the Monthly Weather Review, and other national periodicals, J. Cecil Alter, now Official in Charge of the Weather Bureau Office at Salt Lake City, first suggested the mountain snow-survey for the benefit of irrigators in 1909-10, and made the first systematic mountain-watershed snow-surveys in the country, on Maple Creek Watershed, Utah, beginning in March 1911. This work was done under the supervision of A. H. Thiessen, then the Section Director, at Salt Lake City. Detailed reports of these surveys were made in 1911, 1912, and 1913. From that time, snow-surveys were made annually by the Bureau, on an increasing number of watersheds, the service being done cooperatively with the Utah State Experiment Station in the last several years.

Elevation has long been recognized as one of the most important factors involved in the measurement of snowfall, the reasons being that increasing elevation influences markedly the depth of snow, and at the same time multiplies the difficulties of snow-measurement. Many attempts have been made to evaluate the influence of elevation on snowfall. Among the more successful, is the work of Raymond Price and Robert B. Evans, of the Forest Service, who in a paper entitled "Climate of the west front of the Wasatch Plateau in central Utah," [Mon. Wea. Rev., August 1937] present an analysis of hydrologic data which has been summarized in the chart shown in Figure 1.

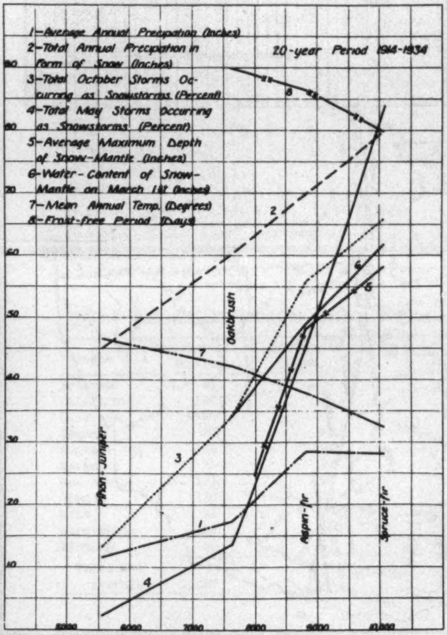


FIG. 1—ANALYSIS HYDROLOGIC DATA, WEST FRONT WASATCH PLATEAU, UTAH

A principal criticism of the Weather Bureau's records of snowfall in the past has been an apparent disregard of the elevation-factor. Because of the necessity to use resident attendants (usually on the basis of unpaid cooperation), the higher elevations predominating throughout the Western States have been poorly represented in the network of Weather Bureau stations. Figure 2 shows graphically this deficiency. Both Profile A-A' crossing the Northwestern States and Profile B-B' crossing the central Western States show a conspicuous absence of snowfall-stations at higher elevations.

Table 1 on which Figure 3 is based indicates also a poor representation of higher elevations throughout the net of Weather Bureau stations reporting snowfall in the Western States. While the average maximum elevation is about 8,000 feet, the average of the 1,287 stations reporting snowfall is only 4,174 feet.

On the other hand, the far-flung network of meteorological and hydrological stations of the Weather Bureau offers the promise of overcoming promptly the difficulty of the inadequate distribution of snowfall-stations. Figure 4 shows the Nation-wide pattern of meteorological stations of all classes. The first-order stations (approximately 225 in number) measure precipitation in all of its forms, as well as other meteorological data. The Bureau's aerological stations, serving aviation, maintain a 24-hour service and prepare weather-maps at six-hour intervals. The network of aerological stations is expanding rapidly to include points of high elevations, and will offer many opportunities to establish mountain snowfall-stations under continuous attendance of Bureau employees. All of the Weather Bureau cooperative precipitation-stations making up the greater number of those comprising the network, are potential snowfall-stations. Those located advantageously will be equipped with storage snow-gage

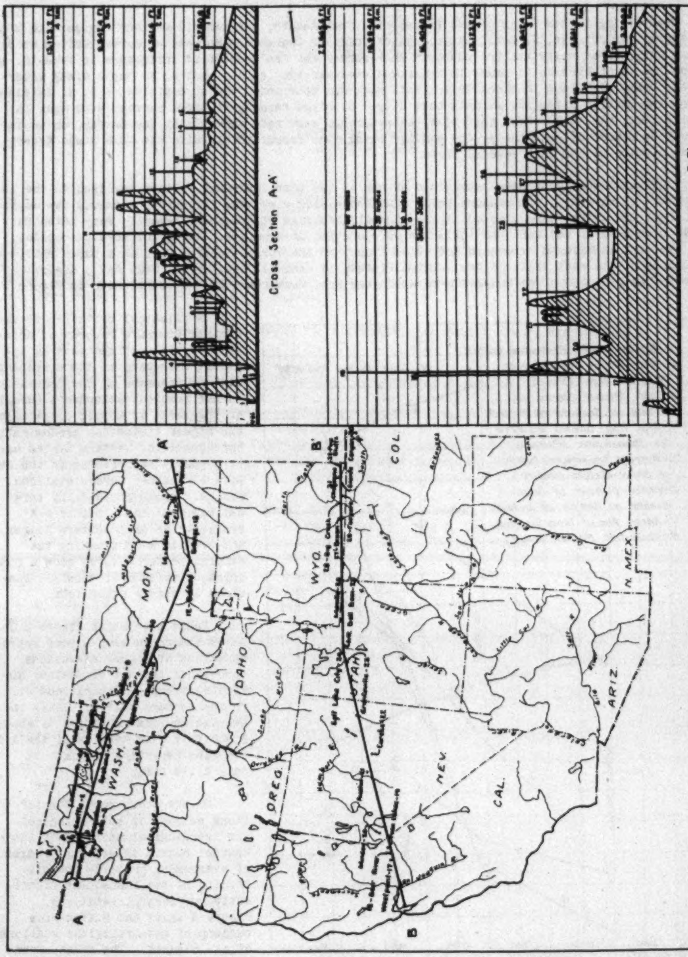
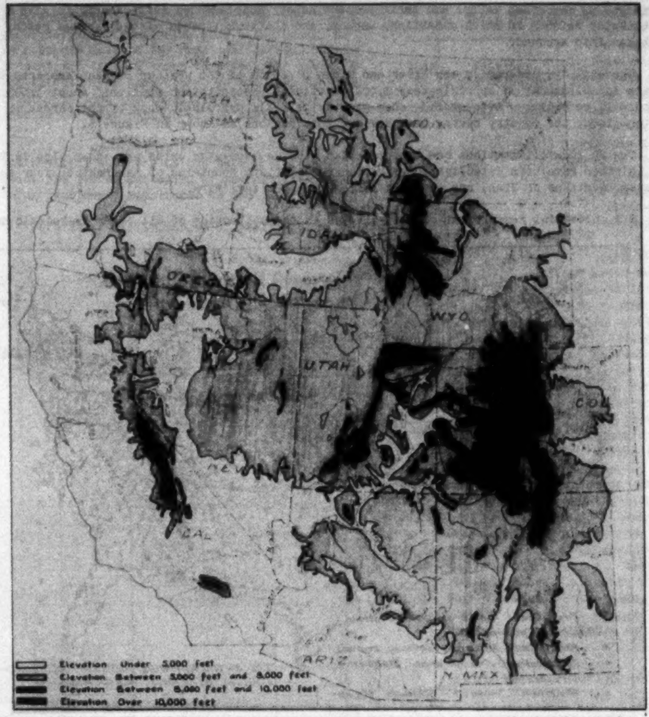


FIG. 2—SHOWING DEFICIENCY WEATHER BUREAU SNOWFALL-RECORDS AS REGARDS ELEVATION


WASH
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UTAH
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N. MEX
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Elevation Under 5,000 feet
Elevation Between 5,000 feet and 8,000 feet
Elevation Between 8,000 feet and 10,000 feet
Elevation Over 10,000 feet

FIG. 3—CHART TO SHOW ROUGHLY REGIONS OF DIFFERENT ELEVATION, WESTERN STATES (SEE TABLE I IN CONNECTION THIS FIGURE)

Table 1--Number of United States Weather Bureau stations reporting snowfall

State	Number of stations	Average length of record (years)	Elevation in feet		
			Maximum	Minimum	Average
Arizona	84	28	8,400	350	3,912
California	143	34	9,600	-20	2,831
Colorado	98	31	10,265	3,386	6,352
Idaho	97	26	6,900	738	3,938
Montana	124	24	6,600	1,900	3,561
Nevada	54	34	7,250	1,400	4,997
New Mexico	171	27	9,000	3,120	5,774
Oregon	130	26	6,475	10	2,116
Utah	94	29	8,700	2,880	5,389
Washington	193	25	5,550	7	1,249
Wyoming	99	23	9,015	3,680	5,791
Total and average	1,287	28	7,978	1,586	4,174

units (to be described later) and their present number supplemented sufficiently to give a well-distributed network in which elevation, aspect, and individual needs of the various basins will be taken into account.

The expanded program of the River and Flood Division of the Weather Bureau embraces the ultimate establishment of approximately 3,400 additional precipitation-stations, about 1,000 of which will be equipped with storage snow-gage units. All hydrologic work of the Bureau is being regionalized, the country having been divided into regions as shown in Figure 5.

The 20 snowfall-stations now equipped with storage snow-gage units are shown also in Figure 5. Thirteen have been established in the Sangre de Cristo Mountains of New Mexico, four in the Wasatch Mountains of Utah, and three in Yellowstone National Park.

The successful measurement of snowfall (as in the collection of all other hydrologic data)

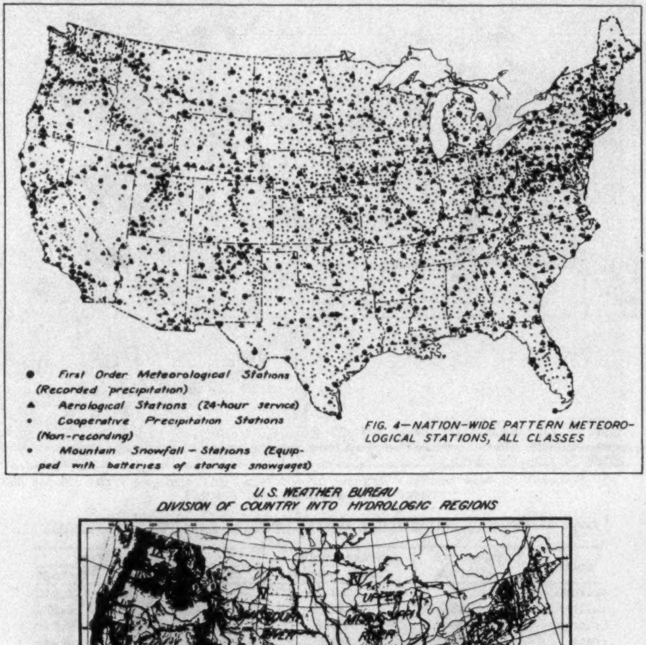

● First Order Meteorological Stations (Recorded precipitation)
▲ Aerological Stations (24-hour service)
• Cooperative Precipitation Stations (Non-recording)
• Mountain Snowfall - Stations (Equipped with batteries of storage snowgages)

FIG. 4—NATION-WIDE PATTERN METEOROLOGICAL STATIONS, ALL CLASSES

U. S. WEATHER BUREAU
DIVISION OF COUNTRY INTO HYDROLOGIC REGIONS

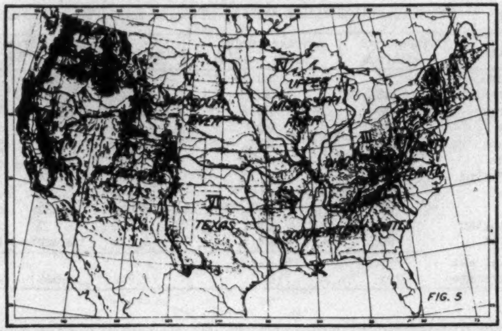
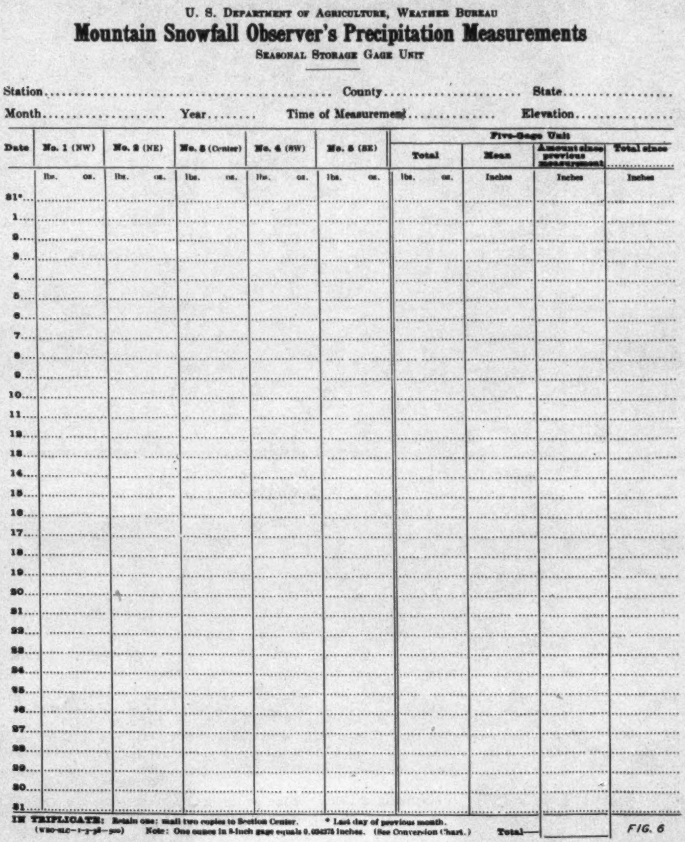

IV
UPPER
MISSISSIPPI
RIVER
V
MISSOURI
RIVER
STATES
VI
TEXAS
III
ATLANTIC
SOUTHEASTERN STATES

FIG. 5

must adhere to a sound theory of sampling. As a carload of sand must be quartered down to a laboratory-sample weighing only a few grams, so must the snow falling over a basin be evaluated by a sample that is the result of careful quartering and elimination and which has taken into account all factors known to affect the amount and distribution of snowfall, such as elevation and aspect. The snow-course is expected to give relative value to these factors throughout its length, so that the composite record of the sampling points along the course can be taken as the average for that portion of the basin the course is to represent.

As the snow-course gives areal significance to its record by virtue of its length, so will the snowfall-stations of the Bureau utilize sampling areas, carefully selected as typical, upon which are placed batteries of storage snow-gages, the average record of the individual gages being considered applicable to the portion of the basin represented by the snowfall-station.

Professor Marvin, about 30 years ago, then Chief of the Weather Bureau Instrument Division,



U. S. DEPARTMENT OF AGRICULTURE, WEATHER BUREAU

Mountain Snowfall Observer's Precipitation Measurements

SEASONAL STORAGE GAGE UNIT

Station.. County...................... State.................

Month.................... Year........ Time of Measurement.............. Elevation...............

Date	No. 1 (NW)		No. 2 (NE)		No. 3 (Center)		No. 4 (SW)		No. 5 (SE)		Five-Gage Unit				
											Total		Mean	Amount since previous measurement	Total since ....
	lbs.	oz.	lbs.	oz.	lbs.	oz.	lbs.	oz.	lbs.	oz.	lbs.	oz.	Inches	Inches	Inches
31*...															
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													Total—		

IN TRIPLICATE: Retain one; mail two copies to Section Center. * Last day of previous month.

(WBO-SLC-1-3-38—900) Note: One ounce in 8-inch gage equals 0.034375 inches. (See Conversion Chart.)

FIG. 6

Table 2--Experimental shielded storage snow-gages at Terminal seven miles west of Salt Lake City

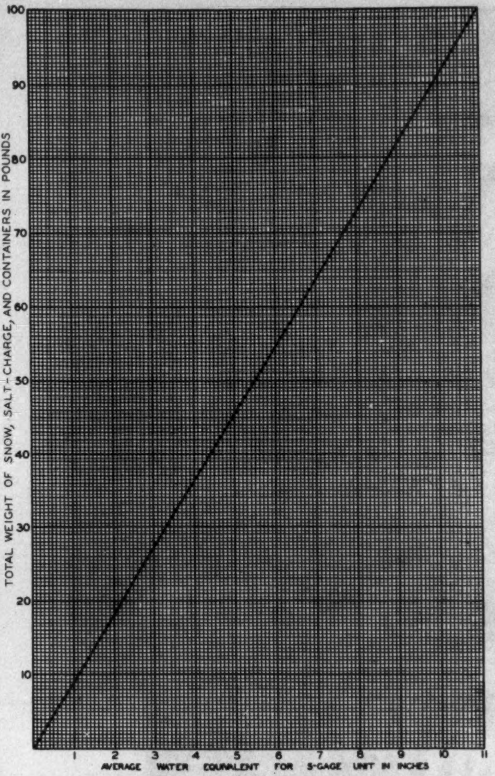
Gage No.	Shields		Dec. 1935	Per cent	Jan. 1936	Per cent	Feb. 1936	Per cent	Oct. 28 and Dec. 1, 1936	Per cent	Dec. 1, 1936, and Mar. 27, 1937	Per cent	Mar. 27, and May 1, 1937	Per cent	Average variation per cent	Per cent of group-average
	Diameter	Height														
	inches	inches														
									Unshielded check-gages							
1	..	..	0.70	89	1.63	75	1.25	76	1.24	78	4.93	90	1.29	99	8.2	84.5
9	..	..	0.66	84	1.73	79	1.38	84	1.25	79	4.86	89	1.26	97	5.1	85.3
									Canvas-shields							
2	32	6	0.77	97	1.92	88	1.66	101	....	..	....	..	....	..	4.9	95.3
3	40	6	0.74	94	1.86	85	1.55	95	1.37	86	4.97	91	1.24	95	3.7	91.0
5	32	3-1/2	0.79	100	2.05	94	1.74	106	1.39	87	5.07	93	1.24	95	4.8	95.8
6	34.4	1	0.81	102	2.08	95	1.62	99	1.40	88	5.36	98	1.27	98	3.4	96.7
									Metal-shields							
4	16	1/2	0.81	102	2.08	95	1.73	106	1.48	93	5.53	101	1.29	99	3.7	99.3
7	24	3/4	0.80	101	2.05	94	1.74	106	1.47	92	5.30	97	1.29	99	3.8	98.2
8	34.4	1	0.83	105	2.14	98	1.77	108	1.49	94	5.64	103	1.29	99	4.2	101.2
									Lath-fence shields							
10	34.4	1	0.84	106	2.47	113	1.86	113	1.50	94	5.79	106	1.29	99	5.8	105.2
11	34.4	1	0.79	100	2.16	99	1.64	100	1.53	96	5.46	100	1.28	98	1.1	98.8
12	56	1-1/2	0.78	99	2.24	103	1.53	93	1.83	115	5.98	110	1.38	106	6.0	104.3
13	92	2-1/2	0.79	100	2.27	104	1.55	95	1.87	118	5.88	108	1.36	105	5.3	105.0
14	152	4-1/2	0.78	100	2.34	107	1.58	96	1.91	120	5.96	109	1.31	101	6.5	105.5
									Ground-baffles							
15	Check-gage.....		0.74	94	2.06	94	1.52	93	1.72	108	....	...	1.31	101	5.2	98.0
16	Baffles........		0.85	108	2.59	119	1.78	109	1.84	116	....	...	1.38	106	4.7	111.6
17	Baffles........		0.85	108	2.81	129	1.80	110	1.85	116	....	...	1.36	105	7.1	113.6
18	Netting........		0.86	109	2.67	122	1.75	107	1.88	118	....	...	1.38	106	6.1	112.4
	Average variability........		....	4.7	....	11.2	....	7.5	....	12.2	....	6.1	....	3.2	...	.....
19	Gage-diameter, 10.488 inches.		....	..	....	..	....	..	....	..	5.64	103	1.24	95	...	.....
20	Gage-diameter, 14.833 inches.		....	..	....	..	....	..	....	..	....	...	6.82	..	...	.....
	Means..........		0.79	..	2.18	..	1.64	..	1.59	..	5.46	...	1.30	..	...	.....
	Average wind miles/hour on stormy days...		5.7	..	7.0	..	8.1	..	6.4	..	7.9	...	9.9	..	...	.....

Table 3--Typical records of snow-courses and storage snow-gage units on experimental watersheds

Description (1)	Date (2)	Depth snow (3)	Density (4)	Water-content (5)	Water-depth snow-gage units (6)	Difference (5) and (6) (7)	(7) as percentage of (5)
	1937	inch	per cent	inch	inch	inch	
Beaver dams	Dec. 21	9.1	18.1	1.65	1.87	0.22	+13
Lupine Creek	Dec. 20	20.6	16.2	3.33	2.86	0.47	-14
Blacktail Deer Creek	Dec. 28	17.4	18.8	3.28	3.66	0.38	+12
Beaver dams	Dec. 29	12.8	19.5	2.50	2.49	0.01	- 0.4

designed a shielded storage snow-gage which was theoretically correct for collecting snowfall in windy weather. It was found, however, in relatively quite weather, when much snow falls, that the rigid, shelf-like metal shields with which Marvin's gage was equipped sometimes collected considerable quantities of snow. Some of the snow lodging on the shields later blew into the gage, and the residue, becoming solidified, was found to obstruct the gage-receiver.

Experiments with flexible, or swinging shields, which would not collect snow, were begun in 1909 by Mr. Alter, then an assistant observer at the Weather Bureau Office at Salt Lake City. Ac-



For gross weights in excess of 100 lbs. multiply the number of hundreds by 11 and add the result obtained from the chart, thus: 249 lbs. 8 ozs. = (2 × 11) + 5.42 = 27.42 inches. One ounce of snow in an eight-inch gage 0.034375 inch

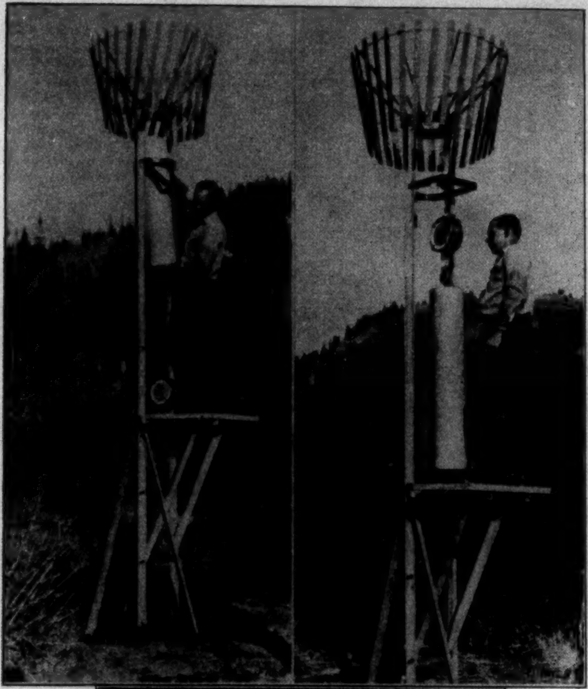
FIG. 7—SNOWFALL CONVERSION-CHART

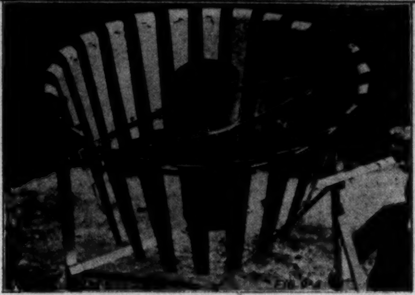
cording to the Monthly Weather Review, Mr. Alter had previously, in 1907, made a seasonal test of an unshielded storage-gage, using an oil-film to prevent evaporation, a six months' test accounting for 97 per cent of the daily measurements, as recorded in the official gage nearby. As a consequence, several seasonal gages were exposed in Utah by Section Director Thiessen during the summer of 1910.

In order to carry these storage precipitation-measurements through the winter and obtain year-long records, Mr. Alter's tests were continued with anti-freeze solutions and gages with reservoirs buried below the frost-line. In the early 1920's, further experiments were made with gages coated on the inside with acid-proof paint, and using common salt to prevent freezing. The method proved so satisfactory that in 1925 a few of the Marvin shielded gages (unmanned in winter) were charged with brine and oil, and monthly measurements were provided for. These seasonal measurements have been continued to date, giving year-long records where only summer measurements were previously made.

In 1935 Mr. Alter was authorized to make further experiments with the flexible, or swinging shield, and the use of salt and oil, with a view to developing a gage suitable for use at isolated mountain outposts. Accordingly, 20 experimental gages were erected at Terminal, near Salt Lake City, with a variety of shields [see Mon. Wea. Rev., July 1937]; also five additional shielded storage-gages were installed near the official gages at four mountain snowfall-stations where the snowfall was particularly heavy. At the end of the second winter, the following results were noted:

Copper gages with folded seams are practically invulnerable to corrosion. Even galvanized sheet-iron gages, heavily galvanized, well soldered at folded seams and heavily coated inside with Triple A acid-proof paint, have shown no leaks or corrosion-stains. At the beginning of this, the third winter of the experiments at Terminal, six of the most efficient shields have been retained, but placed in different positions in the group. Ten new shields, or modifications of old ones, have been added to the group. Six improved shields, identical in their chief dimensions with the old gage, No. 11, are also being studied, one of which is on a ten-foot support. The remaining five comprise an areal unit, four of the gages marking the corners of a rectangle, with the fifth gage being located in the middle of the group.





FIGS. 8 AND 9—SNOW-GAGES AT SILVER LAKE, UTAH
FIG. 9-A—SHOWING IMPROVED SUB-STRUCTURE AND METAL LEAVES REPLACING WOODEN STRIPS IN SHIELD OF SNOW-GAGE

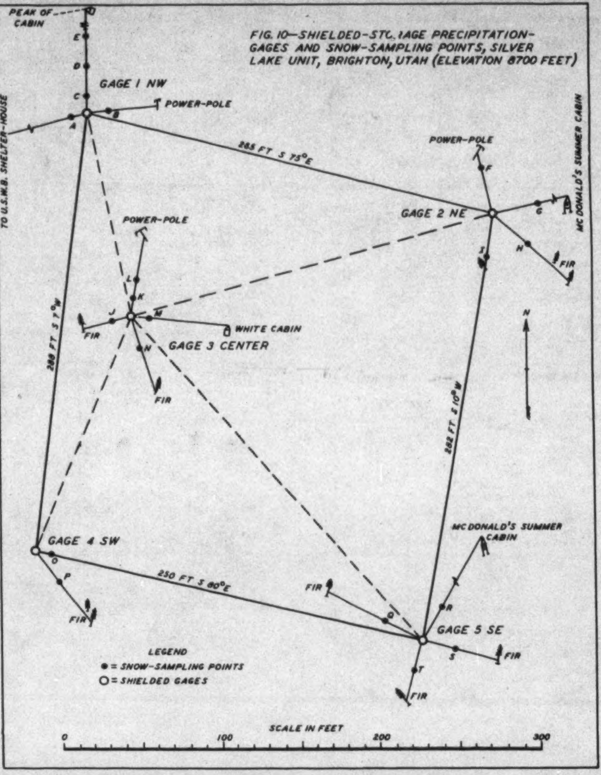

PEAK OF CABIN
E
D
C
GAGE 1 NW
POWER-POLE
A
B
TO U.S.W.B. SHELTER-HOUSE
285 FT S 75°E
POWER-POLE
F
GAGE 2 NE
G
McDONALD'S SUMMER CABIN
I
H
FIR
POWER-POLE
L
K
J
M
FIR
WHITE CABIN
GAGE 3 CENTER
N
FIR
N
288 FT S 7°W
282 FT S 10°W
McDONALD'S SUMMER CABIN
GAGE 4 SW
O
P
FIR
250 FT S 80°E
FIR
Q
R
GAGE 5 SE
S
FIR
T
FIR
LEGEND
● = SNOW-SAMPLING POINTS
○ = SHIELDED GAGES
SCALE IN FEET
0
100
200
300

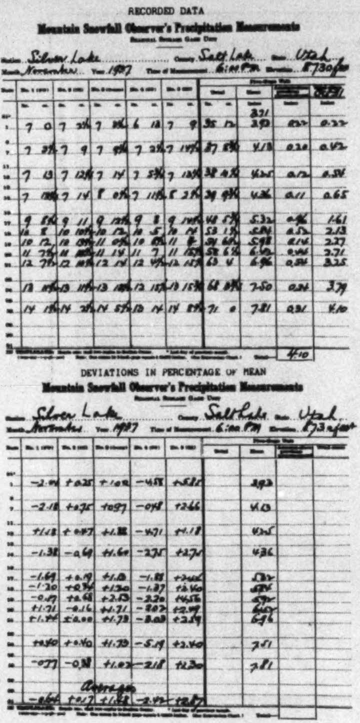
FIG. 10—SHIELDED-STORAGE PRECIPITATION-GAGES AND SNOW-SAMPLING POINTS, SILVER LAKE UNIT, BRIGHTON, UTAH (ELEVATION 8700 FEET)

Gages fitted with swinging shields, from one to three feet in diameter, made of canvas, metal-strips and lath-fencing, have given gratifying results. A comprehensive account of the results appear in the Monthly Weather Review of July 1937, which are summarized in Table 2.

As a basis of comparison, the 20-gage mean for the two years' record has been taken as 100 per cent. Two unshielded check-gages (No. 1 and No. 9) caught only 85 per cent of the 20-gage mean. The average variability of the several monthly measurements from the two year mean was 8.2 per cent for gage No. 1, that being the most unstable gage of the group.

Two gages with canvas-shields (No. 3 and No. 6) and three with metal-leaf shields (Nos. 4, 7, and 8), the catches of which were from 90 per cent to 100 per cent of the group-mean, had variations from the two-year mean of three to four per cent. Gage No. 11, with a common lath-fence screen, or shield, had a two-year average of 98.8 per cent of the 20-gage group-mean and an average variability, or deviation, of only 1.1 per cent. While the experimental tests are not completed, the shield on gage No. 11 has been adopted tentatively, as worthy of more general use.

The operation of a storage snow-gage unit is based upon the simple expedient of weighing the gages, under which procedure the increment increase in weight between weighings becomes the snow-



RECORDED DATA

Mountain Snowfall Observer's Precipitation Measurements

Silver Lake Salt Lake Utah
November 1937 6:00 P.M. 8730 feet

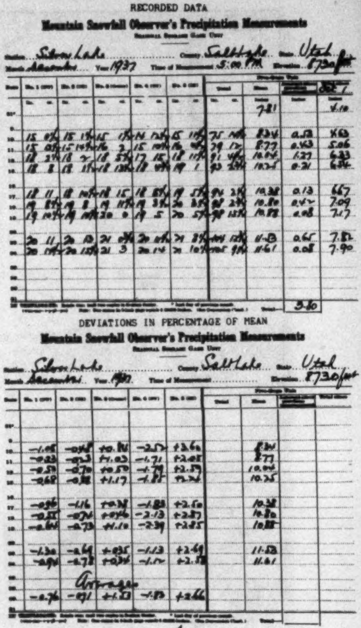
											3.71		
7	0	7	2½	7	3½	6	13	7	9	35 12	3.92	0.22	0.22
7	2½	7	9	7	9½	7	2½	7	14½	37 8½	4.13	0.20	0.42
7	13	7	12½	7	14	7	5½	7	13½	38 4½	4.25	0.12	0.54
7	13½	7	14	8	0½	7	11½	8	2½	39 9½	4.36	0.11	0.65
9	8½	9	11	9	12½	9	8	9	14½	48 5½	5.32	0.96	1.61
10	8	10	10½	10	12	10	5	10	14	53 1½	5.84	0.52	2.13
10	12	10	13½	11	0½	10	6½	11	8	54 6½	5.98	0.14	2.27
11	7½	11	[illegible]	11	14	11	7	11	12½	58 6½	6.42	0.44	2.71
12	7½	12	10½	12	14	12	4½	12	15½	63 4	6.96	0.54	3.25
13	11½	13	11½	13	12½	12	15½	13	15½	68 8½	7.50	0.54	3.79
14	1½	14	2½	14	5½	13	14	14	8½	71 0	7.81	0.31	4.10
												4.10	

DEVIATIONS IN PERCENTAGE OF MEAN

Mountain Snowfall Observer's Precipitation Measurements

Silver Lake Salt Lake Utah
November 1937 6:00 P.M. 8730 feet

-2.94	+0.25	+1.02	-4.58	+5.85	3.93
-2.18	+0.75	+0.97	-0.48	+2.66	4.13
+1.18	+0.47	+1.88	-4.71	+1.18	4.25
-1.38	-0.69	+1.60	-2.75	+2.70	4.36
-1.69	+0.19	+1.10	-1.88	+2.26	5.32
-1.20	+0.94	+1.30	-1.37	+2.40	5.84
-0.17	+0.68	+2.53	-2.20	+4.56	5.98
+1.71	-0.16	+1.71	-3.07	+2.49	6.42
+1.44	±0.00	+1.73	-3.03	+2.29	6.96
+0.40	+0.40	+1.73	-5.19	+2.40	7.51
-0.77	-0.38	+1.07	-2.18	+2.30	7.81
		Average			
-0.66	+0.17	+1.48	-2.42	+2.87	



RECORDED DATA

Mountain Snowfall Observer's Precipitation Measurements

Silver Lake Salt Lake Utah
December 1937 5:00 P.M. 8730 feet

											7.81		4.10
15	0½	15	1½	15	1½	14	12½	15	11½	75 14½	8.34	0.53	4.63
15	0½	15	14½	16	2	15	10½	16	4½	79 12	8.77	0.43	5.06
18	2½	18	2	18	5½	17	15	18	11½	91 4½	10.04	1.27	6.33
18	8	18	11½	18	13½	18	4½	19	1	93 2½	10.25	0.21	6.54
18	11	18	10½	18	15	18	8½	19	5½	94 2½	10.38	0.13	6.67
19	8½	19	8	19	11½	19	3½	20	3½	98 2½	10.80	0.42	7.09
19	10½	19	10½	20	0	19	5	20	5½	98 15½	10.88	0.08	7.17
20	11	20	10	21	0½	20	10½	21	8½	104 12½	11.53	0.65	7.82
20	14½	20	15½	21	3	20	14	20	10½	105 9½	11.61	0.08	7.90
												3.80	

DEVIATIONS IN PERCENTAGE OF MEAN

Mountain Snowfall Observer's Precipitation Measurements

Silver Lake Salt Lake Utah
December 1937 8730 feet

-1.05	-0.48	+0.84	-2.57	+2.60	8.34
-0.23	-0.23	+1.03	-1.71	+2.05	8.77
-0.58	-0.70	+0.50	-1.79	+2.57	10.04
-0.68	-0.88	+1.17	-1.88	+2.26	10.25
-0.96	-1.16	+0.38	-1.83	+2.50	10.38
-0.55	-0.76	+0.96	-2.13	+2.87	10.80
-0.64	-0.73	+1.10	-2.39	+2.85	10.88
-1.30	-0.64	+0.35	-1.13	+2.69	11.53
-0.96	-0.78	+0.34	-1.12	+2.52	11.61
		Average			
-0.76	-0.71	+1.23	-1.83	+2.66	

FIG. 12

FIG. 11

fall for the period expressed in inches of water.

The weights, as taken, are recorded on the form shown in Figure 6. The chart shown in Figure 7 facilitates the reduction of the total weight of the five gages to average depth of water for the unit. The weighing procedure is illustrated in Figures 8 and 9. These views show the snow-gages at Silver Lake, a snowfall-station at the headwaters of Big Cottonwood Creek, the source of water-supply for the city of Salt Lake. [The design has since been improved materially; the sub-structure has been strengthened and the wooden leaves have been replaced by metal ones in the shield (see Fig. 9-A); other details of design have also been improved.] A sketch showing the orientation of the gages in the unit is given in Figure 10. Similar storage snow-gage batteries have been installed at Clear Creek, a snowfall-station in the headwaters of the Price River in Utah, and at Kimberly, Utah, in the Sevier River Basin. Also 13 mountain snowfall-units have been located at the headwaters of the Rio Grande and Pecos River in New Mexico.

Of the mountain stations now equipped with storage gage-units, Silver Lake offers so far the most complete record for the current season. Figure 11 presents the basic data for November 1937, together with differences between the gage-readings expressed as a percentage of the mean for the unit. Figure 12 gives comparable data for December.

Aside from the extremely small departures from the mean, the consistency with which each gage adheres to its relative position to the mean of the group is to be noted. No. 1 gage hovers within two per cent of the mean, either plus or minus; No. 2 gage stays within a fraction of minus one per cent; No. 3 gage is, with the exception of one observation, within two per cent of the mean, all being plus readings; No. 4 gage, except in two instances, registers within three per cent of the group-mean and is consistently less than the mean; No. 5 gage departs farther from the mean than the other gages but is consistent in recording two or three per cent above the mean for all observations.

This consistency, evidenced in all the early reports of the 20 snowfall-units established, points to two distinct advantages of the storage snow-gage unit as a sampling medium. First, it permits an immediate opportunity to correct for faulty gage-location without waiting for the passing of several years which is usually necessary in establishing trends; and secondly, after several years of record, during which one particular gage has demonstrated its ability to register consistently the mean of the group, the other gages might reasonably be dispensed with, perhaps retaining a second gage having a consistent record as a check-gage.

Stream-flow forecasting must be conducted ultimately on a quantitative basis and in terms of a hydrograph of flow which relates time to discharge or stage. This must be the objective of the River and Flood Service of the Weather Bureau, which has the responsibility of forecasting river-and-flood-stages throughout the country. Where heavy snowfall is not involved, appreciable gains have been made in this direction, modern methods projecting the forecasts ahead to the time of falling rain, and with some success into the causes of flood-producing rainfall.

In regions of heavy snowfall, a distinct advantage to the forecaster, it would seem, lies in the fact that the mantle, or "snow-cloud" obligingly builds up and remains in place until released by rising temperatures and concurrent rains, the source of runoff thereby becoming a constant factor.

While the many difficulties of forecasting runoff from snow are fully appreciated, it is believed that a considerable number of them will be eliminated when adequate data are made available. Other phases of development must await the results of hydrologic research for more than empirical treatment.

Recommendations of the Water Resources Committee of the National Resources Board for overcoming the deficiencies in hydrologic research suggest the division of the problem embracing snow into the following parts: (1) Improvement of snowfall-measurement; (2) improvement of snow-depth measurement; (3) determination of relations between factors causing snow to melt; and (4) evaluation of surface-conditions controlling the losses to runoff from melting snow.

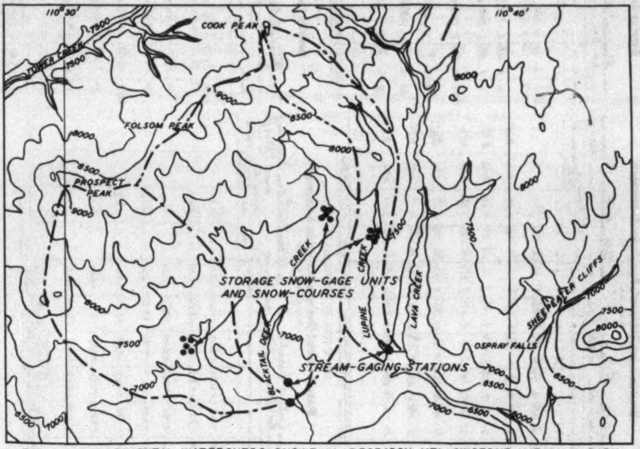
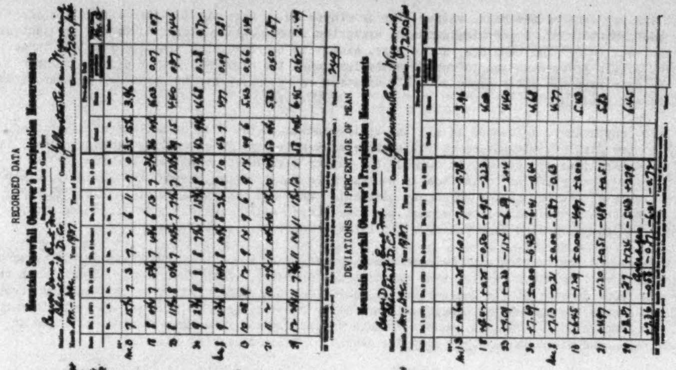


FIG. 13—EXPERIMENTAL WATERSHEDS, SNOWFALL RESEARCH, YELLOWSTONE NATIONAL PARK, UNITED STATES WEATHER BUREAU



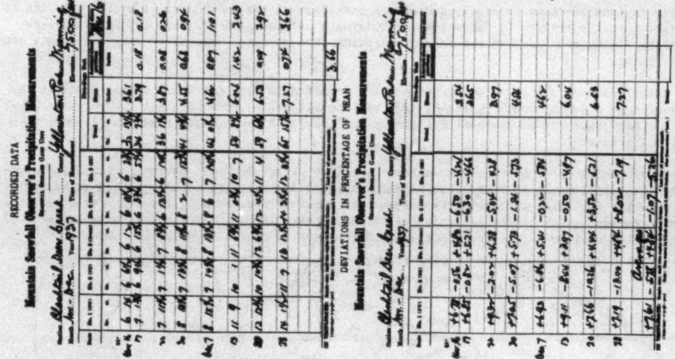
RECORDED DATA

Mountain Snowfall Observer's Precipitation Measurements

DEVIATIONS IN PERCENTAGE OF MEAN

Mountain Snowfall Observer's Precipitation Measurements

FIG. 14



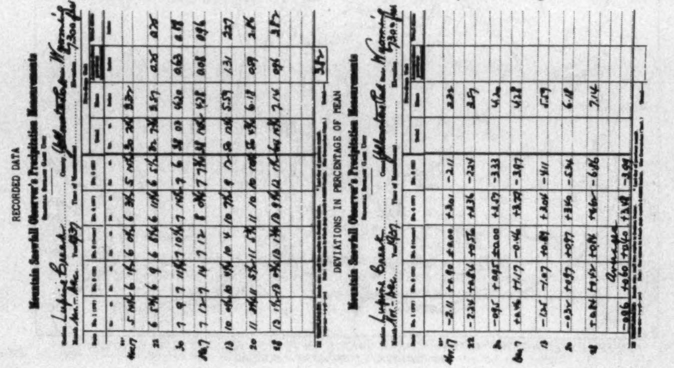
RECORDED DATA

Mountain Snowfall Observer's Precipitation Measurements

DEVIATIONS IN PERCENTAGE OF MEAN

Mountain Snowfall Observer's Precipitation Measurements

FIG. 15



RECORDED DATA

Mountain Snowfall Observer's Precipitation Measurements

DEVIATIONS IN PERCENTAGE OF MEAN

Mountain Snowfall Observer's Precipitation Measurements

FIG. 16

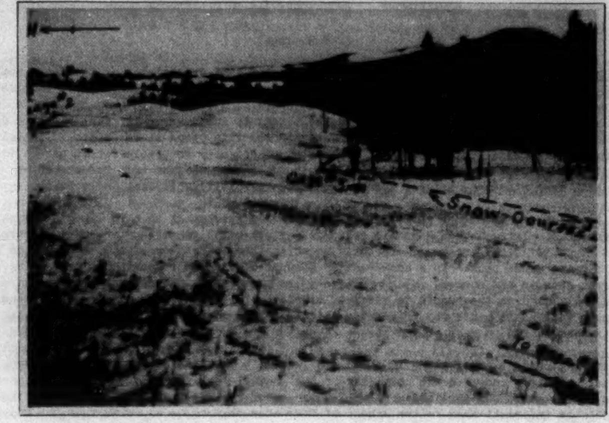


FIG. 17—TYPICAL INSTALLATION FOR SNOW-COURSE

To make a beginning in its research in snow-hydrology, the Weather Bureau has established three experimental watersheds in Yellowstone National Park (Figure 13). Each has been equipped with snow-gage units of the type installed in Utah and New Mexico, having the lath-shield developed by gage No. 11 of the Terminal experiments.

Each watershed has upon it a snow-course, established by the Bureau of Agricultural Engineers (Fig. 18). Runoff from each watershed is measured in cooperation with the United States Geological Survey, the stations being equipped with automatic stage-recorders. The National Park Service is cooperating in the work, constructing the stream-gage houses and assisting in making the snow-runs.

Early results are interesting in that they confirm the consistency with which the storage snow-gage catches and retains snowfall. Figure 14 presents the observed data on Beaver Dams Watershed, together with departures expressed as percentages of the mean. Figures 15 and 16 give comparable data for Blacktail Deer Creek and Lupine Creek, respectively.

An interesting comparison, shown in Table 3, is found in the records of the snow-courses and the storage snow-gage units on the experimental watersheds; a typical installation is shown in Figure 17. It is believed the differences will be less for greater quantities of snow.

In conclusion it seems appropriate to review the advantages of the storage snow-gage units as basin snowfall-samplers:

(1) They measure snow as it falls and before it is subjected to prolonged wind-action.
(2) They secure the record against loss through poor attendance or failure to reach the station before a critical period of melting.
(3) They function at all elevations including the transition-zone from snowfall to rainfall.
(4) They are inexpensive to install and require a minimum attendance.
(5) They are not dependent upon tree-shelter for stability of record.
(6) Precipitation in the form of snow can be recorded as daily precipitation through the simple procedure of daily weighing.

U. S. Weather Bureau,
Washington, D. C.

DEVELOPMENT OF SNOW-SURVEYING IN CALIFORNIA

Fred H. Paget

Introduction--California, in common with all other semi-arid states of the west, sets great store upon its water-supply. In this State with a population that has doubled in the last 20 years and trebled in the last 30 and promises to increase still more in the near future, the ultimate expansion is definitely tied up with the extent to which our water-supply can be developed. In California it has been truly said that "Water is the life-blood of the State." In California, from the first of June until the end of September, practically no precipitation occurs in the areas where it is most needed, and all water used in these areas must come from the preceding winter's supply held over either by natural agencies or by artificial development and control.

A beneficient Providence anticipating California's need seems to have especially designed the magnificent Sierra Nevada Range to serve as a store-house for the water-supply of the State. Here at high elevations are vast watersheds on which the winter's precipitation falls as snow, producing a deep, dense pack. This snow-pack on account of the low temperatures prevailing in these high areas even in the summer, melts very slowly and by so doing, without any artificial regulation, automatically retards the runoff to the levels below until the middle of July, and in the case of the higher watersheds radiating from Mount Whitney, for a month longer. With the regulating reservoirs constructed on many of the rivers, a year round dependable supply is obtained.

With the present development in the use of water in California for irrigation, power, and municipal purposes, and with the certainty of the gradual realization of the State Water Plan for the conservation and regulation of all of the water-resources of the State, it is important that reliable information as to the extent of the water-crop to be expected from the mountain snowpack should be available early each spring. With this advance information a definite program for reservoir-regulation, harmonizing to the best advantage the interests of the various users, can be intelligently worked out in advance of the runoff from the mountains.

History--It is found on looking back through the record that the first snow-surveys made in California occurred very early in its history. The honor of being the first snow-gager in California must go the almost legendary figure of the pioneer ski mail-carrier, Snowshoe Thompson. In the "Daily Union," a Sacramento newspaper, the following article appeared under date of January 10, 1857: "Mr. Thompson left Carson Valley on Monday, January 5 and arrived at this City yesterday morning, the 9th. At Big Canyon the snow was four feet deep; at Hope Valley, five feet; at Luther's Pass, six feet; at Lake Valley, five feet; and in the pass on Johnson's Summit he sounded a depth of ten feet without reaching bottom. He estimates the depth of snow for eight miles this side of Slippery Ford at 12 feet."

It is interesting to note that Thompson is credited with carrying a pack on each trip that weighed between 60 and 80 pounds. Snowshoe died at the comparatively youthful age of 49. Perhaps it was with a view to increasing the longevity of present day snow-surveyors that the Mount Rose snow-measuring equipment was designed with a weight of only 25 pounds and the Federal Government further reduced this weight to 16 pounds.

The first snow-surveys made by the State of California for the purpose of accurately forecasting runoff were begun in 1918 at the time when the late Major Norboe was Chief Assistant State Engineer. In cooperation with the State of Nevada snow-surveys were carried on for several years in the watersheds of Yuba, Tahoe, Carson, and Walker rivers. This work languished in 1923 because of lack of funds.

The present program of snow-surveys was initiated in 1929 through the efforts of the California Economic Research Council of the State Chamber of Commerce. The Legislature from 1929 to 1933 set aside $15,000 each year to organize and carry on this work. The function was allocated to the State Engineer's organization, the Division of Water Resources. In 1933 the Legislature in the interests of stringent State economy was unable to provide any funds for snow-surveying and during the following two years the only work of this nature that was done was by those interested cooperators who carried on alone without outside help in their individual areas. In 1935 this work was re-established as a State function with an appropriation one-half of that formerly provided but, with a little more aid from the cooperating agencies, the program put into effect during 1929-33 has been essentially continued.

Help to maintain the California snow-survey program in its established form was also received from the Division of Irrigation, Bureau of Agricultural Engineering, United States Department of Agriculture. Its financial assistance during the past two years has been very material and its aid in interesting the United States Forest Service in this work to the extent of active cooperation promises to have a stabilizing effect on the future of this work in California.

Present set-up--As the title--"California cooperative snow-surveys"--indicates, this work is essentially of a cooperative nature. Although the State exercises supervision and handles practically all arrangements with respect to field procedure to insure correlation and standardization, and cooperates in building, maintaining, and stocking the mountain shelter-cabins, a large proportion of the actual surveys is made by the personnel of the various interested organizations.

At the present time in California these actively cooperating agencies include municipalities, utility-districts, irrigation-districts, power-companies, and private land-companies in addition to the two Governmental agencies, State and Federal. In all a total of 17 agencies actively engage in this work and if each of the National Forests and National Parks and the widely separated divisions of the larger power-companies, each operating under different field-management, is considered separately, the number of cooperators increases from 17 to 33.

The total number of active snow-courses in California is 175 and at the time of the main snow-survey of April 1 it is necessary to send 150 men out in the field to get the work done within the necessary time-limits. Of these 150 men only about 17 are paid through the State and the Federal Bureau of Agricultural Engineering, the balance being carried directly on the pay-rolls of the different cooperators. The annual cost of this work is about $18,500, the State expenditure being about 40 per cent of the total, the cooperating agencies providing the remaining 60 per cent.

With the surveys so made 24 watersheds are covered, beginning with the Shasta River on the north and ending on the south with the Kern. For each main stream we forecast the runoff at the point where the river leaves the mountains and on most of them at several points on the higher tributaries as well. Our present plans call for ultimately forecasting at 58 points.

A bulletin of snow-conditions is issued four times each year, beginning in February and then monthly until May. The bulletin in April carries our forecasts of runoff for the ensuing April-July period. Last April our bulletin included forecasts on 16 streams at a total of 30 points, and on those that we have the figures to check on to date our maximum error has been twelve per cent. Out of a similar set of forecasts made of the runoff of 1936, 80 per cent had an error of less than ten per cent.

Direct method--As a rule in California most of the snow-pack is in the mountains by April 1 so that accurate forecasts can generally be made from the results of snow-surveys conducted at that time. These forecasts are necessarily based upon the assumption that average conditions will prevail during the following months. On those few years when precipitation during April is unusually low or unusually high the original forecasts of April 1 can be modified and the changes published in the bulletin issued at the first of May. However, the departure from normal precipitation in the spring months must be quite large to change the forecasts of April 1 materially.

In working up a method to afford a basis for accurately modifying the original estimates because of abnormal precipitation during the early runoff-period, the State of California has somewhat modified the basic method of working from a percentage of normal and has developed so-called "Direct-method curves." As this method affords a means of putting a definite tag on the influence of precipitation during the runoff-period and since it also has received favorable comment from several of our larger cooperators, we believe that there is sufficient merit in it to justify a brief outline of the procedure.

To illustrate this the watershed of the Upper San Joaquin River has been chosen. Here the supporting surveys are made by The Inyo National Forest, The Mono National Forest, The City of Los Angeles, The San Joaquin Light and Power Corporation, and the Southern California Edison Company.

Illustration of method--The steps in working up the curve are as follows:

(1) Compute the mean weighted water-content for the watershed in inches. This is done by:
  (a) Find mean water-content in inches for each elevation-zone by averaging the results from each snow-course in that zone.
  (b) Multiply mean water-content for each zone as found under (a) by the area of the corresponding zone; these zone-areas are to be expressed as a percentage of the total area of the watershed considered (generally not lower than 5000-foot contour).

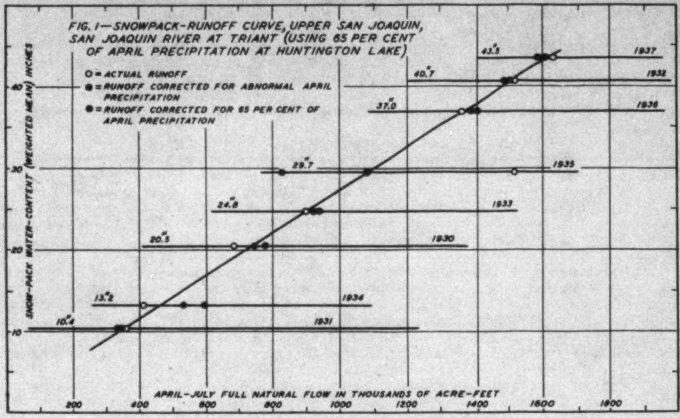


FIG. 1—SNOWPACK-RUNOFF CURVE, UPPER SAN JOAQUIN, SAN JOAQUIN RIVER AT TRIANT (USING 65 PER CENT OF APRIL PRECIPITATION AT HUNTINGTON LAKE)

Table 1--Computation-sheet for forecast by direct method, California Cooperative Snow-Surveys (specimen Form SS-4)

Basin: Upper San Joaquin
Area: Above Friant = 1,632 square miles
Period for which forecast is made: April 1 to July 31
Precipitation-factor for watershed = area in square miles × (640/12) = 1,632 × (640/12) = 87,000
Percentage of precipitation-departure used as correction: 65 per cent
Precipitation-station: Huntington Lake

Year (1)	Water-content, weighted mean (2)	Full natural flow, thousands acre-feet (3)	Precipitation-departure (mean 2.34 inches) (4)	Acre-feet at 100 o/o = (4) × 87000, thousands acre-feet (5)	Correction 65 per cent (5) × 0.65, thousands acre-feet (6)	Full natural flow: Basic (7)	Full natural flow: From curve (8)	Difference: Thousands acre-feet (9)	Difference: Per cent (10)
1930	20.5	687	-1.01	-88	-57	744	740	4	1
1931	10.4	352	+0.09	+ 8	+ 5	347	350	3	1
1932	40.7	1519	+0.27	+23	+15	1504	1510	6	0
1933	24.8	905	-0.38	-33	-22	927	900	27	3
1934	13.2	412	-2.11	-184	-120	532	460	72	17
1935	29.5	1522	+7.91	+690	+450	1072	1080	8	1
1936	37.0	1366	-0.36	-31	-20	1386	1370	16	1
1937	43.5	1625	+0.46	+40	+26	1599	1620	21	1

(c) An addition of the figures for each zone as found under (b) represents the mean weighted water-content of the watershed in inches of water. (Note: This is a parallel computation to that used in the "standard percentage-of-normal" method, except that the actual water-content in inches is used directly instead of reducing it to a percentage of normal.)

(2) The mean weighted water-content in inches for the watershed, computed in this way for each year of record, is plotted against the full natural flow from the watershed for the corresponding year, with water-content in inches as the ordinate, and runoff in thousands of acre-feet as the abscissa; so much water-content = so much runoff--an empirical relation. Comment: At this stage, however, the empirical relation, although exhibiting a marked tendency,

is generally characterized by several markedly non-conforming years. Figure 1--data as in Table 1--for the San Joaquin illustrates this point. Here the open circles indicate the points as plotted by the method discussed. The point for 1935 is far out of line with the rest and some of the other points show variations. A casual inspection indicates that the disturbing factor is mainly the precipitation during the runoff-period. The great divergence of 1935 is explained when we examine the record of rainfall at Huntington Lake and we find that 10.25 inches of rain fell that year during April. This amount is 34 per cent of the average yearly total and 435 per cent of the average precipitation for April.

(3) Correct annual full natural flows for the surplus or deficit of precipitation during April so that these natural-flow figures approximate the runoff that would have occurred had April been an average month. This is done by:
  (a) The precipitation at the controlling station used (in this case Huntington Lake) during the month of April of each year of record is compared to that normally occurring during that month and the deficit or surplus is noted.
  (b) This surplus or deficit in inches is multiplied by one-twelfth of the area in acres of the watershed above the point of forecast and so reduced to an amount in acre-feet.
  (c) The historical full natural flow for each year is then modified by the full amount so obtained, increased during years of deficient rainfall and reduced during years when a surplus occurred during April. These modified figures can now be regarded as the full natural flow that would have occurred had the deficiency or surplus of April precipitation at the controlling station been reflected 100 per cent in the runoff from the entire watershed.

The crossed circles of Figure 1 are the points so corrected. It is noted that now the nonconformity is in the opposite direction--1935 especially now being far to the left where before it was far to the right. It is evident that an over-correction has been made and this would signify that 100 per cent of the rainfall-departure is not reflected in the runoff from the watershed.

By inspection a percentage of this rainfall-departure is arrived at which when corrected for, best satisfies all the years and leads to a more-or-less smooth curve with all points falling within a reasonable limit of error. In this instance the correction is 65 per cent. Using this correction of 65 per cent the full black circles of Figure 1 are plotted and a curve is drawn through the points.

With this curve at the first of April the forecast of runoff in acre-feet for the April-July period can be taken directly from the curve as soon as the results of the snow-surveys have been received, with a minimum of computation, since only the mean weighted water-content for the watershed has to be computed and a curve so established--provided it is backed by enough years of record to make it dependable--should need very little revision.

At the end of the month as soon as the record of rainfall at the precipitation-station is received, a quick computation determines the correction to be applied which, if material, can be published immediately in the May 1 bulletin.

In presenting this procedure it is realized that no new principles are involved. It has the advantage that in cases where a constant flow occurs from the watershed, which does not fluctuate with the snow-pack, this is automatically taken care of by the curve. Also the curve sets up the units just as they are measured in the field. We find that by this method with its accompanying curve it is extremely easy to explain the system to the busy engineers of most of our cooperators. With the curve in their possession as soon as the results are brought into their offices by their own snow-surveyors they can quickly arrive at an accurate estimate of runoff-expectations in their area without waiting for the time-lag necessary for the returns from all over the State to be analyzed and published.

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ESTABLISHING FIRST-YEAR NORMALS ON THE BASIS OF ACCUMULATED SNOWFALL AND SOME FORECASTS FROM RUNOFF-RATIOS

Richard C. Farrow

Introductory-- This paper deals with methods and their application in a particular case, which follow logically from a paper by the author, "Snowfall and runoff in the Canadian Columbia Basin: A study to determine the ideal locations for snow-survey stations" [Trans. Amer. Geophys. Union, 18th annual meeting, p. 632, 1937].

It is necessary, therefore, to outline some of the deductions of that paper, which form the background for this one. They may be enumerated as follows:

(1) That the snowfall-data of certain selected precipitation-stations showed a remarkable relationship to runoff.

(2) That precipitation from May to August was a negligible factor in affecting runoff.

(3) That both the Kootenay and Columbia rivers showed a remarkable uniformity of flow at widely-separated points when expressed in per cent of normal.

(4) That the snowfall-data for those precipitation-stations showing a reasonable relationship to runoff can be used to produce a preliminary normal of water-content for new snow-courses in their vicinity, so that a start can be made on the mechanics of forecasting as soon as the first year's sampling is obtained.

Snow-courses have now been established in close proximity to the various precipitation-stations whose data were found to be relatable to runoff, but at higher elevations. Three were laid out last year and sampled for the first time in March, 1937. Seven more were laid out in 1938, and will be sampled for the first time next March. This paper will discuss the sampling data from the former three courses, the method used in obtaining a tentative first-year normal, the mechanics of forecasting used, and the application of these forecasts to points further downstream by the use of runoff-ratio factors. The three courses in question are located at Glacier and Field, on the main line of the Canadian Pacific Railway in the Selkirks and Rockies, respectively, and at Fernie in the Crows Nest Pass, Southern Canadian Rockies.

First-year normals--Establishing tentative snow-course normals from records of precipitation-data has been in use for some time. Percentages of normal were thus established in the initial Lake Tahoe snow-surveys under the direction of Dr. Church.

The method used is as follows: The records of accumulated snowfall are tabulated and an average, or normal, is obtained. The accumulated snowfall for the year in question is then expressed as percentage of this normal and the water-content of the snow-course is assumed to be of the same percentage. Therefore, the water-content divided by this percentage will give the tentative "normal" of water-content in inches for the course. I shall call this the "percentage-method," merely to distinguish it from the one described and used later on.

It has the advantage of being applicable to any case where precipitation-data of snowfall are assumed to be in relationship to snow-course data; and as long as the percentage of normal at the station approximates that at the snow-course, it is usable, regardless of the relative depth of snow at the two places, which may actually be some distance apart. It has, however, the inherent weakness that the normal obtained must reflect any inaccuracy or discordance in the percentage of normal of accumulated snowfall for the single year in question.

With the idea of avoiding this weakness the writer developed what may be called the "density-method," for want of a better name. It is frankly more limited in application than the "percentage-method," as it can only be used where the precipitation-station and snow-course are close enough together to assure approximately equal depths of snowfall. Such conditions are assumed to exist at all the new Columbia Basin courses located in 1937 and 1938. Of the new courses, those at Glacier and Field will be used as an example. This method should not be used if premature melting has occurred on the snow-course or if the density found at sampling appears to be abnormal. Both these limitations are usually assessable.

The "density-method" attempts to set up a tentative normal for the course by using the normal of accumulated snowfall and correcting this by the ratio between the density of newly-fallen snow and that at sampling. It is evident that other factors enter into it which the density-ratio appears to represent, but as this is still experimental and hypothetical--it is not ripe for formal presentation. Sufficient to say that I used it experimentally for the Kootenay-Columbia forecasts, though the "percentage-method" yielded equally good results. I am much indebted to Carl Elges, Assistant Meteorologist of the Nevada Agricultural Experiment Station for useful and constructive criticisms and discussions of this method, which are still taking place.

Whichever method is used, it is plainly very worth while to study the relevant precipitation-data, with the idea of using them, if possible, as there are several advantages to be gained in obtaining a tentative snow-course normal in this way, namely:

(1) It enables the mechanics of forecasting to be gone through, using the data from a new course the first year it is sampled, instead of having to wait a year when the normal is obtained by comparison with runoff.

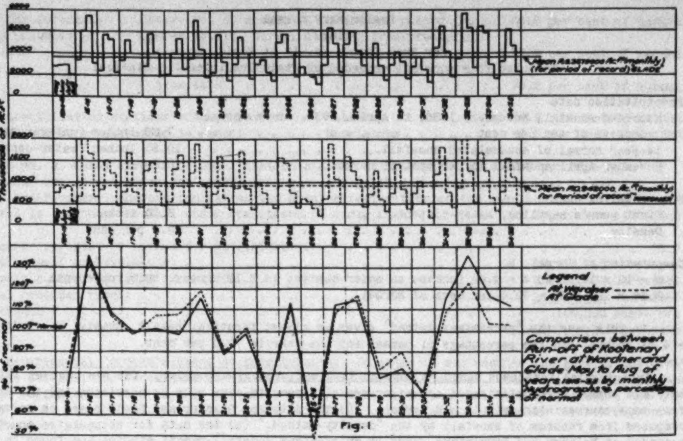

Fig. 1

(2) If the data from a new course are to be combined with those from several others before being used for forecasting, there is a risk that the individual course-data may not be very well related to runoff in many years, although when combined with the data of other courses, the combination of all data does relate to runoff. In this case a normal for an individual course computed from runoff might be seriously in error, and the error would not become apparent until several years of record were built up. The normal computed from precipitation-data avoids this possibility of error.

(3) Where no previous record of runoff exists, the above method eliminates the long wait of years while an independent snow-course normal is being built up.

In order to start forecasting in the first year of sampling, this method was employed to produce a normal for the courses at Field, Glacier, and Fernie, and with good results.

The computation of tentative snow-course normals for Glacier and Field are given below as an example:

Preliminary normal
(1937)
For Snow-Course No. 11 at Glacier
(computed from snowfall recorded at Glacier Precipitation Station)

Precipitation-data

Recorded snowfall November, 1936, to April, 1937, at the assumed density of ten per cent	28.80 inches (water-content)
23-year normal of accumulated snowfall.	36.63 inches (water-content)
November-April snowfall in per cent of normal	78.6

Snow-sampling data, March, 1937

First year's sampling, water-content.	13.49 inches
Density	23.3 per cent

Computation of normal, N

N = 10 × 36.63/23.3 = 15.72 inches; as water-content is 13.49 inches, this represents 13.49 × 100/15.72 = 85.8 per cent of normal.

It will be noted that if the "percentage-method" is used N = 13.49/0.786 = 17.16 inches, and of course the percentage of normal of water-content for the year is the same as for accumulated snowfall, 78.6 per cent.

Preliminary normal
(1937)
For Snow-Course No. 12 at Field
(Computed from snowfall recorded at Field Precipitation Station)

Precipitation data

Recorded snowfall November, 1936, to April, 1937, at an assumed density of ten per cent	7.36 inches (water-content)
14-year normal of accumulated snowfall	10.51 inches (water-content)
November-April snowfall in per cent of normal	70.0

Snow-sampling data

First year's sampling, water-content	3.16 inches
Density	23.4 per cent

Computation of normal, N

$N = 10 \times 10.51/23.4 = 4.49$ inches; as water-content is 3.16 inches, this represents $3.16 \times 100/4.49 = 70.4$ per cent of normal.

In this case the "percentage-method" gives an almost identical result, namely, $N = 3.16/0.70 = 4.51$ inches, and the percentage of normal for the year is 70.0 per cent.

Experimental forecasts based on snow-surveys and precipitation-data--The new courses laid out this summer were anticipated and experimental forecasts were made by combining (a) the data from snow-courses with established normals, (b) the data for Glacier and Field based on normals computed from records of snowfall by the "density-method," (c) the data for accumulated snowfall recorded at Malakwa and Revelstoke, used in my previous snowfall-runoff studies and found to be closely relatable to runoff, and (d) the average flow-ratios which were developed in the paper mentioned above [Trans. Amer. Geophys. Union, 18th annual meeting, p. 639, 1937].

The paper by the author, referred to before, showed that unweighted averages of the percentages of normal of the various courses should yield the best forecasts. The forecasts for the Columbia River at Golden and at Revelstoke were, therefore, computed as follows:

Columbia River at Golden (Nicholson)
Forecast 1937

Courses: No. 8 at Sinclair Pass; No. 11 at Glacier; No. 12 at Field

Course No. 8, normal of three-year record	66.5
Course No. 11, normal from precipitation-data	85.8
Course No. 12, normal from precipitation-data	70.2
Average per cent of normal	74.1

Normal runoff 2,093,000 acre-feet

Forecast: 74.1 per cent of 2,093,000	1,551,000 acre-feet
Actual runoff	1,529,000 acre-feet
Difference	22,000 acre-feet
Error	$22 \times 100/1529 = 1.4$ per cent

Using percentages of normal based on the "percentage-method," the forecast would be 1,500,000 acre-feet, giving an error of 1.9 per cent; but as the two errors have opposite signs, the actual divergence is $1.4 + 1.9 = 3.3$ per cent.

Columbia River at Revelstoke
Forecast 1937 (experimental)

This is built up by using the forecast for Golden, which is based on snow-courses, two of which (Nos. 11 and 12) have a preliminary normal of water-content computed from precipitation-records of snowfall.

The data for Golden are then combined with those for certain precipitation-stations, which in the Columbia snowfall-runoff studies showed best relationship to the runoff of the Columbia at Revelstoke. The data from Course No. 9, Canoe River, were also brought into the combination.

Used for forecasting Columbia at Golden	Course No. 8, normal of three-year record	66.5 per cent of normal
	Course No. 11, normal computed from precipitation data	85.8 per cent of normal
	Course No. 12, normal computed from precipitation data	70.2 per cent of normal

From precipitation stations which when combined showed relationship to runoff	Malakwa	78.1 per cent of normal
	Revelstoke	90.0 per cent of normal

Course No. 9, Canoe River (the precipitation-station at this point gave 77.1 per cent of normal, but it was known that there had been considerable thawing in this area in January and February, thereby indicating the greater reliability of the snow-course as compared to recorded snowfall under conditions of early thaw) . . . . . 63.5 per cent of normal.

Indicated per cent of normal for Revelstoke	75.7
Normal runoff at Revelstoke	17,486,000 acre-feet
Forecast runoff = 0.757 × 17,486,000	13,237,000 acre-feet
Actual recorded runoff	13,077,000 acre-feet
Difference	160,000 acre-feet
Error	160 × 100/13077 = 1.2 per cent

Experimental forecasts based on runoff-ratios--In order to see whether the general uniformity between snowfall and runoff was maintained for the precipitation-year 1936-37, [see (1), (2), and (3) at beginning of this paper], forecasts were attempted for the runoff of the Columbia River at Trail and of the Kootenay at Glade. These points will ultimately be served by the new snow-courses located this year.

In previous studies [see Trans. Amer. Geophys. Union, 18th annual meeting, p. 639, 1937], it was found that the May-August flow of both the Kootenay and Columbia showed a remarkable uniformity at widely separated points, which became apparent when the runoff was expressed in per cent of normal. On the Columbia River the variation in per cent of normal between Golden and Revelstoke was under ten for 17 years, and between ten and fifteen for the remaining four years of the period of record; while between Revelstoke and Trail the variation was under ten for 16 years, and between ten and fifteen for the remaining five years. On the Kootenay River between Wardner and Glade the variation was even less; being under ten for 19 years, and under 14 for the remaining two years of record.

On the Columbia, Revelstoke is about 200 miles below Golden and the average May-August increase of runoff is 8.44 times. Trail is about 175 miles below Revelstoke and the average runoff increases 2.10 times, while on the Kootenay, Glade is about 300 miles below, and the average increase of runoff is 3.91 times that at Wardner. In both cases during the 21-year period of record the average increase-factor used would be within seven per cent of the actual in the year of maximum departure from average.

The flow-relationship between Wardner and Glade on the Kootenay is shown below by the comparative hydrographs. It will be noted how clearly the percentage hydrograph shows this relationship, as compared to the ordinary hydrographs. These increase factors were used for experimentally forecasting the Columbia at Trail and the Kootenay at Glade; the former is given as an example.

Columbia River at Trail
Forecast, 1937 (experimental)

Based on the forecast for Revelstoke adjusted for the runoff-ratio between Revelstoke and Trail which was developed in the Columbia snowfall-runoff studies. The runoff at Trail is 2.1 times that at Revelstoke as per hydrological studies of 25 years of record; it is 17.73 times that at Golden (see also experimental forecast for Revelstoke).

Forecast of Columbia at Revelstoke (based on snow-courses Nos. 8, 9, 11, and 12 and snowfall-data of precipitation-stations at Malakwa and Revelstoke	13,237,000 acre-feet

Forecast for Trail based on above ratio = 2.1 × 13,237,000	27,798,000 acre-feet
Actual	28,414,000 acre-feet
Difference	616,000 acre-feet
Error in forecast	616 × 100/28414 = 2.2 per cent

Table 1--Forecasts, Canadian Columbia Basin, 1937

River	Station	Snow-course	Per cent of normal	Runoff in acre-feet		Error in per cent of actual	Per cent error in 1936 forecast
				Forecast	Actual		
Columbia	Golden	8, 11, 12	74.1	1,551,000	1,529,000	1.4	11.5
Columbia	Revelstoke	8, 9, 11, 12	75.7	13,237,000	13,077,000	1.2	(a)
Columbia	Trail	8, 9, 11, 12	77.8	27,798,000	28,414,000	2.2	(a)
Kootenay	Wardner	6, 10	79.9	2,997,800	2,804,700	6.9	5.8
Kootenay	Glade	7, 8, 10	79.6	11,721,400	11,781,000	0.5	(a)
Elk	Elko	8, 10	79.6	873,300	816,300	7.0	(a)
Lardeau	Gerrard	7	74.9	394,000	379,000	4.0	7.9
Duncan	Howser	7	74.3	1,311,000	1,404,000	6.6	6.8

(a) = No forecast in 1936.

Notes: Courses 7, 8, and 9 had three years of record and normals computed from runoff. Course 10 had two years of record and a normal computed from precipitation-records of snowfall. Courses 11 and 12 were sampled for the first time in 1938, and normals were computed from precipitation-records of snowfall. The forecasts for the Columbia at Golden, for the Kootenay at Wardner, and for the Elk, Duncan, and Lardeau rivers were all based on snow-course data alone. The Columbia at Revelstoke was forecast using the data from four snow-courses and two precipitation-stations. The Columbia at Trail and the Kootenay at Glade were forecast experimentally using the runoff-ratio factors.

The complete results of forecasts in the Canadian Columbia Basin for 1937 are shown in Table 1.

Conclusion--The foregoing appears to substantiate the general relationships previously disclosed and once again precipitation during runoff has produced no noticeable effect on runoff. This is important, as, before these studies were made, it was thought that it might seriously affect forecasts in many years.

Finally, the thought occurs that the snowfall-runoff relationship which has been found to exist in the Canadian Columbia Basin, can hardly be a "law unto itself." There must be many other areas on this Continent--particularly in the West--where these conditions are duplicated in greater or lesser degree, if some one will but undertake to find them.

Water Rights Branch, Department of Lands,
Province of British Columbia,
Victoria, British Columbia, Canada

DISCUSSION

CARL ELGES (University of Nevada, Reno, Nevada)--Mr. Farrow and I had considerable correspondence regarding the contents of this paper and there remains a difference of opinion on one subject, the so-called "density-method" of computing a snow-survey-course normal. I shall endeavor to point out briefly my objections to this method.

As was stated in the paper, the "density-method" attempts to set up a tentative normal for the course by using the normal of accumulated snowfall and correcting this by the ratio between the density of newly fallen snow and that at sampling. From this statement and from the examples given, the equation for the tentative snow-course normal is: Normal water-content = density of freshly fallen snow × accumulated snowfall normal water-content divided by density of snow as surveyed on course. This equation would intimate: (1) That there is a relationship between density of the snow on the snow-course and the amount of snow that fell during the winter; (2) that the density would be the same year after year at the time of surveying; (3) that the more dense the snow, the less would be the water-content of the snowpack. All of these conditions are, of course, untenable.

As a practical test, the Summit Snow-Course, elevation 6,900 feet, and Soda Springs Precipitation-Station, elevation 6,752 feet, were selected for application of this density-method. These stations are located within a short distance of each other in the Sierra Nevada Mountains. The snow-course record at Summit for the past eight years is as follows:

Year	Water-content	Density in per cent	Year	Water-content	Density in per cent
1930	24.3	46.7	1934	17.8	50.0
1931	17.9	40.5	1935	53.7	43.2
1932	49.7	55.2	1936	48.5	49.5
1933	35.3	40.6	1937	43.0	39.3

The mean precipitation during November to March for the past 67 years of record at the Soda Springs Station is 33.03 inches water-content.

Using the equation stated above, the following would result: Normal water-content = 10 × 33.03/density at sampling. Using the 1937 density, the result would be: normal water-content = 10 × 33.03/39.3 = 8.4 inches. Using the 1932 density, the normal water-content = 10 × 33.03/55.2 = 6.0 inches. By merely glancing at the record above for the past lean years it is obvious that the equation is not producing normals that are at all within reason. The normal used by the Nevada Cooperative Snow-Surveys for the Summit Course is 47.8 inches.

Since I find no logic in the equation and since I am unable to get reasonable results with it, I am of the opinion that the "density-method" is basically wrong. Even though the "density-method" were logical, it would still be undesirable because of the variability of the snow-cover density. In the eight years of record cited above for the Summit Snow-Course, the density ranged from 39.3 to 55.2 per cent. Thus, for that course, a normal computed from the lower density would be about 40 per cent greater than that obtained from the higher density.

It appears that in Mr. Farrow's region there is a loss of about half of the snow water-content between the time of precipitation and the date the snow-survey is made. This condition seems to have made the "density-method" results approach somewhere near a true normal. Equally good normals and results are obtainable by using the percentage method, which is known to give satisfactory results for all regions.

WINTER PRECIPITATION VERSUS SNOW-SURVEY FOR FORECASTING ON SKAGIT RIVER, WASHINGTON

Robert Leaver

This article is a discussion of an able paper prepared by George Holmes Moore, Chief Hydrographer for the Seattle Light Department, in which he proposes to forecast the month-by-month flow of the Skagit River at Diablo Dam. Mr. Moore uses certain comparisons of the monthly precipitation and the monthly mean inflow over a period of 25 years. The correspondence between Dr. J. E. Church and Mr. Moore during the period of July 9, 1937, to September 9, 1937, forms the basis for the preparation of the paper.

The figures pertain to the 1,100 square miles of drainage into the Diablo Reservoir. It is shown by Mr. Moore that in an average year 74 inches of rain falls and the measurement of the runoff entering the Reservoir appears to be 68.7 per cent of this. By using the water-year, beginning the calculations with October of one year and ending with September of the next, he showed that almost half of the annual rainfall occurs in the first three months and during this period the runoff is under one-sixth, from which he concludes that the difference is a surplus to be drawn upon during the nine months to follow.

Summing up the maximum and minimum figures for each month as obtained from the 25-year record, the flow of the River for each particular period, it is claimed, can be predicted within a mean average of five per cent.

The storage in the form of snow, using the accepted rule that ten inches of the snow as it falls makes one inch of water, sums up roughly that the 1,100 square miles of watershed tributary to Diablo Dam would need be covered with 22 feet of unpacked snow at the end of March. No effort has been made to check the snow-depth by actual measurements on the ground because it is stated that occasional precise measurements would not provide a reliable basis for estimates. A detailed survey covering such a large area would be expensive and from the power-company's standpoint is not considered of enough importance. The advantage of the stream-flow forecast from past rainfall-runoff data is that it is easy to obtain and is claimed to be sufficiently accurate to indicate the actual flow to be expected for any particular period, provided considerations are given for unusual conditions such as excessive or deficient rains during the runoff-period. In addition sunshine and temperature are of grave importance.

In order to get a better mathematical viewpoint, we have tabulated the monthly precipitation-

Table 1--Monthly precipitation in inches at Diablo Dam, 1909-34

Month	Year												
	09-10	10-11	11-12	12-13	13-14	14-15	15-16	16-17	17-18	18-19	19-20	20-21	21-22
October	6.45	14.97	1.35	7.07	8.29	6.20	13.00	5.72	4.59	12.35	3.77	12.27	13.49
November	27.68	17.54	19.15	12.37	13.31	11.60	11.53	11.84	7.71	9.84	11.39	10.62	14.14
December	7.41	9.56	7.72	16.21	3.10	2.80	10.80	8.38	29.75	16.66	10.38	13.08	12.83
January	13.64	9.26	11.84	11.57	17.42	4.77	5.53	8.00	16.45	15.26	17.75	18.54	5.20
February	9.03	2.69	8.32	3.31	4.53	5.69	12.51	5.80	11.19	8.68	2.24	12.08	5.19
March	6.40	4.80	1.71	8.76	8.10	3.59	8.34	9.22	10.36	7.85	9.71	6.33	7.68
April	5.00	1.92	2.75	2.46	4.61	4.77	6.69	6.28	1.59	5.56	4.00	3.87	5.43
May	4.20	3.58	1.51	2.62	1.40	3.56	2.63	0.81	3.58	3.60	3.55	3.33	3.58
June	1.89	1.67	1.96	3.19	2.80	0.90	1.82	4.71	1.09	1.17	2.18	3.89	0.23
July	0.13	0.68	2.16	2.30	0.30	2.09	2.67	1.20	1.46	0.55	0.38	0.35	T
August	0.71	1.63	3.09	1.77	0.40	0.16	0.23	0.13	3.20	0.76	2.00	1.39	2.60
September	1.90	6.69	1.30	7.03	7.90	2.00	3.21	3.13	0.23	2.22	10.59	7.84	6.33
Totals													
Oct.-Mar.	70.61	58.82	50.09	59.29	54.75	34.65	61.71	48.96	80.05	70.64	55.24	72.92	58.53
Apr.-Sep.	13.83	16.17	12.77	19.37	17.41	13.48	17.25	16.26	11.15	13.86	22.70	20.67	18.17
Per cent normal													
Oct.-Mar.	118.3	98.5	83.9	99.4	91.8	58.1	103.4	82.0	134.1	118.3	92.6	122.1	97.2
Apr.-Sep.	91.3	106.7	84.2	127.7	114.6	88.9	113.8	107.1	71.9	91.3	149.4	136.4	119.8

Month	Year												Mean
	22-23	23-24	24-25	25-26	26-27	27-28	28-29	29-30	30-31	31-32	32-33	33-34	
October	7.72	3.82	12.24	6.39	9.59	9.43	11.70	4.80	6.59	6.38	10.50	14.57	8.53
November	3.21	7.62	9.61	6.48	8.43	17.53	6.55	1.29	5.33	11.47	20.01	7.76	11.36
December	14.88	19.31	15.78	13.40	10.11	11.13	7.46	14.73	4.64	9.85	13.53	29.24	12.51
January	19.18	11.64	19.39	9.22	9.65	10.93	4.19	5.35	13.01	9.82	15.77	15.57	11.96
February	2.55	16.34	13.03	8.95	9.73	1.78	3.11	14.69	8.13	18.28	8.40	2.90	7.97
March	8.46	1.96	6.22	2.21	6.46	10.56	6.92	6.89	10.08	12.79	8.22	10.57	7.37
April	3.05	4.45	2.90	2.29	2.99	5.24	3.67	3.98	3.63	3.40	1.38	3.01	3.80
May	2.52	0.73	1.62	4.79	3.07	1.69	3.07	2.46	3.88	0.79	3.04	4.11	2.79
June	2.33	0.88	0.49	0.28	0.80	1.94	3.38	2.44	5.31	1.08	2.42	0.96	1.99
July	1.45	0.43	0.09	0.01	0.47	0.95	0.78	0.39	0.80	2.50	0.80	2.43	1.01
August	1.00	0.79	1.56	2.62	2.94	0.45	1.13	0.26	0.14	0.95	1.38	0.53	1.27
September	2.85	5.92	0.23	3.43	6.63	1.52	1.00	3.05	7.18	2.11	9.47	2.02	4.23
Totals													
Oct.-Mar.	56.00	60.69	76.27	46.65	53.97	61.36	39.93	47.75	47.78	68.59	76.43	80.61	59.69
Apr.-Sep.	13.20	13.20	6.89	13.42	16.90	11.79	13.03	12.58	20.94	10.83	18.49	13.06	15.09
Per cent normal													
Oct.-Mar.	93.8	102.1	127.8	78.2	90.4	103.3	66.9	80.0	80.0	114.9	128.0	135.1	100.00
Apr.-Sep.	86.9	86.9	58.5	88.5	111.5	77.9	85.8	83.0	137.9	71.5	121.7	86.2	100.00

figures (Table No. 1) recorded in inches at Diablo Dam for the 25-year period from 1909 to 1934. The data of precipitation at Diablo Dam and inflow into the Reservoir, or past its site before construction, were kindly furnished to Dr. Church by Mr. Moore.

It is noted that while the precipitation in any particular month may vary more than 100 per cent over a period of years, yet the mean for a long period represents the expected rainfall and the few exceptions can be classed as years of drought or flood.

If we consider the mean precipitation occurring in the Diablo Watershed in two periods of six months each, the first period, October to March, will show 59.69 inches of water as the mean precipitation, which is equal to 80 per cent of the yearly total based upon normal conditions. In Table No. 2, which gives the monthly mean inflow at Diablo Dam, the reverse runoff-condition is readily apparent, that is, the months of October to March show the lowest runoff. During the month of April the streams double and June is the high-water month. July is six-tenths of June and in August the flow is only one-half of July and by September it is back to low, practically the same as the average for October to March.

The normal figures are determined by monthly averages in each of the two six-month periods during the 25 years that the records have been kept. The use of normal figures covering the last

Table 2--Monthly mean inflow in second-feet at Diablo Dam, 1909-34

Month	Year												
	09-10	10-11	11-12	12-13	13-14	14-15	15-16	16-17	17-18	18-19	19-20	20-21	21-22
October	1940	5360	1130	1110	2580	1910	1830	1230	1580	2370	1020	4950	4070
November	6910	5790	1650	1940	2690	3840	2030	1390	1840	1960	2780	2270	3510
December	3660	2540	1400	1320	1830	1730	1870	930	5090	3330	2690	1850	6700
January	1780	1470	1360	950	3590	900	860	910	7220	2100	2810	2240	1420
February	1510	930	2060	1380	1270	770	1170	1250	2530	1410	2710	3210	875
March	4280	1720	1110	1390	2800	1560	1720	925	1980	1220	1570	2760	785
April	7260	3280	2600	3640	5350	5200	3020	2000	5270	5150	1670	3220	2000
May	14000	7400	8760	9310	9180	4520	8210	8180	8380	10300	5480	10400	8150
June	10100	13500	10600	15400	8490	3920	15600	12300	12700	12000	8750	16700	14500
July	7730	7380	5280	8360	6010	3590	10400	8990	6620	9320	8470	7650	5700
August	3670	3660	3370	4230	3200	3480	4760	3800	3700	4510	3690	4030	3490
September	2020	2700	1570	3200	1960	1500	2740	2220	2420	2340	3100	2430	2530
Totals													
Oct.-Mar.	20080	17810	8710	8090	14760	10710	9480	6635	20240	12390	13580	17280	17360
Apr.-Sep.	44780	37920	32180	44140	34190	22210	44730	37490	39090	43620	31160	44430	36370
Per cent normal inflow													
Oct.-Mar.	146.2	129.7	63.4	58.9	107.5	78.0	69.0	48.3	147.4	90.2	98.9	125.8	126.5
Apr.-Sep.	124.0	105.0	89.1	122.2	94.7	61.5	123.8	103.8	108.2	120.8	86.3	123.0	100.7
Per cent normal precip.													
Oct.-Mar.	118.3	98.5	83.9	99.4	91.8	58.1	103.4	82.0	134.1	118.3	92.6	122.1	97.2
Apr.-Sep.	91.3	106.7	84.2	127.7	114.6	88.9	113.8	107.1	71.9	91.3	149.4	136.4	119.8

Month	Year												Mean
	22-23	23-24	24-25	25-26	26-27	27-28	28-29	29-30	30-31	31-32	32-33	33-34	
October	1975	1395	2535	850	2930	4155	2915	1145	1465	1265	1980	5485	2367
November	1390	935	2420	760	1555	3850	1400	605	1255	2260	5860	5315	2648
December	2015	1785	5155	2670	2055	3285	1135	695	860	1595	3575	5245	2600
January	2460	1185	1890	1510	1535	4690	705	600	1850	1600	1885	4660	2087
February	1135	5685	3075	1470	1080	1725	565	2755	2220	4305	1085	3440	1985
March	1350	1925	1995	1740	1240	2370	1050	2140	2475	4095	1450	5460	2044
April	4985	2195	5420	4585	2890	2820	1950	7035	3325	5175	3975	11710	4229
May	9455	11835	13415	4565	6895	12005	8185	6400	9530	9490	7400	11045	8900
June	11490	7260	10275	4095	13330	7420	8855	7780	7425	11625	14645	8205	10679
July	7740	4655	6200	3460	6005	5230	4470	5090	4025	5345	11480	5285	6579
August	3585	2820	3120	2525	3575	2645	2795	2780	2350	3180	5305	3370	3506
September	2185	2035	1750	1360	3390	1835	1550	1945	2330	1605	2870	2010	2224
Totals													
Oct.-Mar.	10325	12910	17070	9000	10395	20075	7770	7940	10125	15120	15835	29605	13732
Apr.-Sep.	39440	30800	40180	20590	36085	31955	27805	31030	28985	36420	45675	41625	36116
Per cent normal inflow													
Oct.-Mar.	75.2	94.0	124.3	65.6	75.7	146.2	56.6	57.8	73.7	110.1	115.3	215.6	100.00
Apr.-Sep.	109.2	85.3	111.3	57.0	99.9	88.5	77.0	85.9	80.3	100.8	126.5	115.3	100.00
Per cent normal precip.[a]													
Oct.-Mar.	93.8	102.1	127.8	78.2	90.4	103.3	66.9	80.0	80.0	114.9	128.0	135.1	100.00
Apr.-Sep.	86.9	86.9	58.5	88.5	111.5	77.9	85.8	83.0	137.9	71.5	121.7	86.2	100.00

[a] From Table 1.

quarter of a century adds much weight as to the probable conditions to be expected in the next few years of the future.

Now compare the percentages of normal inflow, April to September (Tables 2 and 2-A), with the percentages of normal precipitation during October to March of each seasonal year as abstracted from Table 1 and recorded in Table 2. By considering each year during a number of years, Table 2-A--Summary of percentages of normal from Table 2

Year	Precipitation Oct.-Mar.	Runoff Apr.-Sep.	Divergence of runoff compared with precipitation	Precipitation Apr.-Sep.
1909-10	118.3	124.0	+5.7	91.3
1910-11	98.5	105.0	+6.5	106.7
1911-12	83.9	89.1	+5.2	84.2
1912-13	99.4	122.2	+22.8	127.7
1913-14	91.8	94.7	+2.9	114.6
1914-15	58.1	61.5	+3.4	88.9
1915-16	103.4	123.8	+20.4	113.8
1916-17	82.0	103.8	+21.8	107.1
1917-18	134.1	108.2	-25.9	71.9
1918-19	118.3	120.8	+2.5	91.3
1919-20	92.6	86.3	-6.3	149.4
1920-21	122.1	123.0	+0.9	136.4
1921-22	97.2	100.7	+3.5	119.8
1922-23	93.8	109.2	+15.4	86.9
1923-24	102.1	85.3	-16.8	86.9
1924-25	127.8	111.3	-16.5	58.5
1925-26	78.2	57.0	-21.2	88.5
1926-27	90.4	99.9	+9.5	111.5
1927-28	103.3	88.5	-14.8	77.9
1928-29	66.9	77.0	+10.1	85.8
1929-30	80.0	85.9	+5.9	83.0
1930-31	80.0	80.3	+0.3	137.9
1931-32	114.9	100.8	-14.1	71.5
1932-33	128.0	126.5	-1.5	121.7
1933-34	135.1	115.3	-19.8	86.2

you will note how surprisingly closely the runoff approaches the precipitation. There are a few years that do not check closely, but if you consider the particular year when there is a considerable difference between the precipitation during October to March and inflow for April to September, in most cases the explanation appears to be due to non-normal precipitation during the summer or runoff-season. The break is not exact and may average the difference in the two adjoining seasons, but occasionally this explanation does not hold. It appears from these figures that the rainfall and snow deposited during the early months of the seasonal year furnishes an excess over the runoff at that period, which provides a surplus which about equals the losses of the nine months that are to follow. The tables check Mr. Moore's contention that although nearly one-half of the annual precipitation occurs in the first quarter of the seasonal twelve months, less than one-sixth of the annual runoff takes place in the same three months. Practically 80 per cent of the annual precipitation takes place in the six months through March and only 27-1/2 per cent of the annual runoff.

Let us now consider the figures covering the outflow at the power-plant below the Diablo Dam. These figures were obtained from United States Geological Survey Water-Supply Papers. The gaging station is located in SE 1/4 sec. 21 T. 37 N., R. 12 E in Whatcom County, one mile above Goodell Creek, 6-1/2 miles below Stetattle Creek, and 16 miles above Marblemount. Some of the water is held back by the Dam since its construction, but a correction is made for this in the figures as recorded in the Water-Supply Papers and assembled in Table 3. The percentages of normal for each of the six-month periods of the flow below the Dam do not vary, in most cases, more than one to two per cent from the figures showing the inflow to the Reservoir which were furnished by Mr. Moore. The records for the outflow below the Dam cover only 18 years compared with 25 years for the inflow. In my figures the percentages of the same 18 years for inflow were averaged to obtain the percentage of the total in order to make a direct comparison. This gave 103.27 per cent of the 25-year period for October to March and 100.06 per cent for runoff during April to September.

Mr. Moore's plan appears practical if close seasonal conditions are not important. Storms may vary in intensity at different sections of the watershed and not accurately correspond with the measurements at the one inflow-station. Seepage-fractures and movement caused by earthquakes may in the future detour some water past the measuring point and thus change the conditions of long standing.

Greater accuracy could probably be obtained by taking more samples at extended stations, but it may not be of enough importance to the Power-Company using the outflow.

Table 3--Monthly mean runoff in second-feet, power-plant below Diablo Dam, 1909-13 and 1920-34

Month	Year									
	09-10	10-11	11-12	12-13	20-21	21-22	22-23	23-24	24-25	25-26
October	2150	5950	1250	1230	5740	4680	2150	1440	2900	860
November	7880	6600	1880	2210	2660	4170	1460	1090	2660	850
December	4250	2940	1620	1530	2310	7680	2510	2180	5660	3170
January	2050	1690	1560	1090	2810	1530	2840	1440	2150	1680
February	1660	1020	2270	1520	3970	940	1190	6270	3480	1670
March	4660	1880	1210	1520	3350	810	1480	2010	2240	1920
April	8060	3640	2890	4040	3810	2290	5530	2340	5850	4810
May	14800	7850	9290	9870	11400	8850	5590	12400	13500	4890
June	10400	13900	10900	15900	17600	14600	11700	7700	10500	4270
July	8270	7890	5650	8950	8560	5960	8230	4930	6520	3500
August	3890	3880	3570	4480	4160	3680	3800	2930	3200	2640
September	2160	2890	1680	3430	2720	2690	2250	2090	1720	1460
Total										
Oct.-Mar.	22650	20080	9790	9100	20840	19810	11630	14430	19090	10150
Apr.-Sep.	47580	40050	33980	46670	48250	38070	37100	32390	41290	21570
Per cent normal runoff										
Oct.-Mar.	146.9	130.2	63.5	59.0	135.1	128.5	75.4	93.6	123.8	65.8
Apr.-Sep.	126.8	106.7	90.5	124.4	128.6	101.4	98.9	86.3	110.0	57.5
Per cent normal precip.[a]										
Oct.-Mar.	118.3	98.5	83.9	99.4	122.1	97.2	93.8	102.1	127.8	79.2
Apr.-Sep.	91.3	106.7	84.2	127.7	136.4	119.8	86.9	86.9	58.5	88.5

Month	Year								Mean	25-year normal
	26-27	27-28	28-29	29-30	30-31	31-32	32-33	33-34		
October	3220	4550	2930	1170	1580	1310	2200	6030		
November	1850	4330	1590	630	1320	2550	6490	5700		
December	2410	3550	1300	840	960	1680	3790	5820		
January	1830	5210	770	670	2360	1690	2080	5050		
February	1260	1940	600	3220	2470	4680	1050	3810		
March	1430	2670	1210	2500	2860	4390	1590	6000		
April	3270	3070	2160	7360	3730	5750	4240	12260		
May	7360	12400	8580	6620	9760	9740	7600	11510		
June	13700	7670	9190	7860	7710	11800	14700	8370		
July	6460	5340	4460	5190	4280	5640	11500	5310		
August	3690	2590	2720	2860	2310	3330	5350	3360		
September	3630	1790	1470	2050	2420	1730	3110	1990		
Totals										
Oct.-Mar.	12000	22250	8400	9030	11550	16300	17200	32410	15928	15420
Apr.-Sep.	38110	32860	28580	31940	30210	37990	46500	42800	37552	37530
Per cent normal runoff										
Oct.-Mar.	77.8	144.3	54.5	58.6	74.9	105.7	111.5	210.2	103.27	100.00
Apr.-Sep.	101.5	87.6	76.2	85.1	80.7	101.2	123.9	114.0	100.06	100.00
Per cent normal precip[a]										
Oct.-Mar.	90.4	103.3	66.9	80.0	80.0	114.9	128.0	135.1		
Apr.-Sep.	111.5	77.9	85.8	83.0	137.9	71.5	121.7	86.2		

[a] From Table 1.

The knowledge of the conditions of the snow-reservoir for each month of the year would aid in predicting the time of increased runoff, but this is considerably affected by temperature.

The value the Power-Company would receive from using the snow-survey seems questionable in view of the extra cost and but small increase in accuracy.

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DISCUSSION

H. P. BOARDMAN (University of Nevada, Reno, Nevada)--Mr. Moore of Seattle, in his paper which gave rise to the studies under discussion and in his correspondence with Dr. Church, seemed to evidence a misapprehension of snow-surveying and its use for forecasting runoff. He apparently thinks of snow-surveying as an attempt to determine quantitatively the water-content of the snow on a watershed instead of its use to make a comparison of different years by the percentage of normal relationship. Probably the percentage of normal relationship was first used for this forecasting purpose by Dr. Church.

The main advantage I would claim for the snow-survey method over the single-station precipitation-method used by Mr. Moore is that several snow-courses, each comprising a number of points of measurement, the same each year, should be expected to give a better indication of watershed-conditions than a single point of observation even if actual water-content of each snow-storm were determined instead of assuming, as does Mr. Moore, that ten inches of snow equals one inch of water.

Using Mr. Moore's tables of precipitation at Diablo Dam Site and stream-flow into the Reservoir-site, Mr. Leaver has reduced them to the percentage of normal form, for the two semiannual periods, termed summer and winter.

The results have been plotted on the graphs shown as Figure 1. We can see at a glance that the upper graph shows a very fair agreement between the winter precipitation and the following summer runoff for a majority of the years. The agreement is within less than ten per cent of normal for 13 years and 10.1 per cent one other year--1928-29.

Let us study some of the years when the agreement is not so close and see if we can in part account for the discrepancies. In 1912-13 the summer runoff exceeded the winter precipitation by 23 per cent of normal and the apparent explanation is that the summer precipitation was 128 per cent of normal and the winter runoff was only 59 per cent of normal--the latter indicating that the winter precipitation was mostly snow and the weather cold enough to prevent much melting until April. A snow-survey about April 1 should have checked this last condition but nothing could have foretold that the summer precipitation would exceed normal by 28 per cent.

In 1915-16 and 1916-17, the summer runoff exceeded the winter precipitation by more than 20 per cent of normal and while the summer precipitation was somewhat above normal, the winter runoff was far below normal again indicating cool weather holding the snow for summer runoff.

In 1917-18, however, the winter precipitation was 134 per cent of normal but the summer runoff only 108 per cent of normal. The winter runoff was very high, over 147 per cent of normal and an April 1 snow-survey would no doubt have shown this loss by indicating much less than 134 per cent of normal water-content remaining in the snow. However, with storage developed, this winter runoff would be saved and, even by Mr. Moore's method, an allowance for high winter runoff would no doubt lead to a decreased estimate of summer runoff. Also in that year the April-September, or summer precipitation, was only 72 per cent of normal thus helping to explain the drop in summer runoff.

For 1919-20 an apparent discrepancy may be accounted for as follows: The summer runoff was six per cent under the winter precipitation in per cent of normal, while the summer precipitation was 149 per cent of normal. However, an examination of the monthly records in the Tables shows that the high summer precipitation occurred mostly in September--10.6 inches, where the normal for September is only 4.2 inches, and that the October runoff following was more than twice normal. Probably the high precipitation of that summer came so late in September as to contribute more to the next water-year in runoff than to the month in which it fell as the ground was likely dry enough to absorb much of the early part of the storm. I have not had access to the daily records.

In 1924-25 the drop in summer runoff of 16.5 per cent below the winter precipitation is consistent with the high winter runoff and the very low summer precipitation of 58.5 per cent.

In the last year shown, 1933-34, the extraordinarily high winter runoff of 215.6 per cent coupled with a somewhat low summer precipitation, 86 per cent, might easily account for the drop in summer runoff of nearly 20 per cent below the winter precipitation. The December precipitation, more than 29 inches, was 234 per cent of normal and probably rain, thus accounting for the heavy winter runoff and decrease in snow-holdover.

I congratulate Mr. Moore on having a watershed to deal with where there is so satisfactory a relationship between precipitation and runoff.

If I were making the forecasts, I would very much prefer to rely on snow-surveys of two or three well-selected snow-courses rather than one precipitation-station such as he uses. However, I can see how he, without snow-survey experience and with such results as he can show for his method, can readily convince those in authority that the added expense of snow-surveys may not be warranted.

GEORGE H. MOORE (Department of Lighting, Seattle, Washington, by letter to H. P. Boardman)--Replying to your kind letter of February 21, I am glad to say that I am in substantial agreement with the paper by Mr. Leaver, and with the discussion by yourself. There are, however, a few points that I wish to stress.

The first of these is that I am not opposed to snow-surveying, but I am dubious about "occasional" or "spot" measurements. And I am especially skeptical where "spot" measurements, or even measurements at a single portion-of-a-mile snow-course, are used as a basis for forecasting runoff from a watershed which is many square miles in area. On the other hand, if stream-flow forecasting from snow-course surveys has been proved valuable, over a span of years, in any locality, I have no wish to do other than applaud the forecaster.

A second point for stress is that snow-surveys usually have, as their first objective, the forecasting of the stream-flow in the summer months which follow. In our particular case, we have long known that, until later stages of the Skagit development, we will be wasting water during an average of five of these summer months. In the future development of the Skagit Project, when we can use the tremendous storage which will be created by Ruby Dam, now under first construction, it is possible that snow-surveys may become of prime importance in the Skagit hydrography.

As a third and final point, I urge that, as a preliminary to any and all forecasts, the runoff during the months of increasing snow-storage should be converted into average inches over the watershed-area, and these runoff-inches should be subtracted from some percentage of the inches of water-precipitation in those same months. Too many forecasts seem to disregard this vital point.

Or, putting it differently, on the rule-of-thumb basis that one inch of water may equal ten inches of snow, it is ten times as important to know the "per cent of normal" for runoff-inches as to know it for snowfall-inches.

H. P. BOARDMAN--Referring to Mr. Moore's discussion by letter, I still think he fails to appreciate the main differences between snow-surveys and precipitation-records for forecasting runoff.

His problem involves only water for power-development and of course, as he says discussing his "second point," with incomplete storage-development they must waste water during about five months. Where water is used for irrigation, some of it is actually "used up" by applying it to the land instead of merely letting all which is not caught in reservoirs go by, either through water-wheels or over spillways. In either case, it is important to know early in the spring approximately what can be counted on for current use and for storage and future use.

In his "third point" Mr. Moore urges the importance of converting the runoff during the winter months into equivalent inches depth over the whole watershed-area and subtracting it from "some percentage of the inches of water-precipitation in those same months." That shows the quantitative point of view and is of course important for his method of forecasting from precipitation-records. However, it is not at all necessary or even useful with the snow-survey method, for the basis of our forecasts from snow-surveys is the residue, in the form of snow, still on the ground at the end of the snowfall-season. What ran off during the winter is gone or caught in reservoirs, measured, of course, but belonging to past history and not useful for forecasting summer runoff.

The residue of water in the snow-blanket measured at the various snow-courses does not even need to be applied to the drainage-area in a quantitative way, but by the percentage-of-normal method is compared with other years, thus enabling an estimate to be made of runoff by a similar comparison of years.

Referring to the graph (Fig. 1), in my former discussion let us examine a few points which were not discussed before. In 1923-24 the summer runoff dropped to 85.3 per cent of normal when the winter precipitation had been two per cent above normal and the winter runoff and summer precipitation had not been sufficiently abnormal to account for 17 per cent discrepancy. Probably

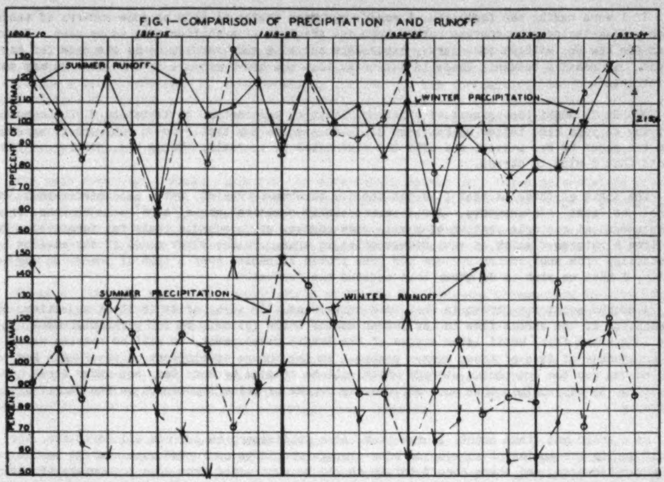


FIG. I—COMPARISON OF PRECIPITATION AND RUNOFF

this was a case where the winter precipitation at Diablo Dam was not representative of the whole tributary watershed.

In 1925-26 and in 1930-31 the winter precipitation was about the same in total, namely, 78.2 per cent and 80 per cent of normal, and the winter runoff still lower in proportion, namely, 65.6 per cent and 73.7 per cent of normal, indicating a large portion of the precipitation retained in the form of snow. But the summer runoff dropped off to 57 per cent of normal in 1925-26 while it held up to 80.3 per cent of normal in 1930-31 almost the same as the winter precipitation. The summer precipitation in 1925-26 was a little low while in 1930-31 it was 137.9 per cent of normal and this no doubt explains why the summer runoff kept up in 1930-31.

Naturally in a low precipitation-year the runoff should be still lower in proportion since the absorption-losses, consisting of percolation, evaporation, transpiration, etc., being approximately constant or probably even greater in a dry year, would leave less in proportion for surface-runoff. If this would not account for the drop of 21.2 per cent from winter precipitation to summer runoff in 1925-26, then it would indicate another discrepancy perhaps due to the single precipitation-station not being representative of the whole drainage-basin.

However, we have some discrepancies hard to explain with our snow-surveying even when we have numerous snow-courses pretty well distributed over the watershed and at various elevations.

R. C. FARROW (Water Rights Branch, Department of Lands, Victoria, British Columbia, by letter to H. P. Boardman)--I was much interested in the paper of Mr. Leaver and would have liked very much to have studied it and offered something serious in the way of criticism. But at this late hour it is useless to attempt the constructive criticism which the paper deserves.

Mr. Moore's use of available data is ingenious and logical, and one can well see that with the results obtained there is little incentive to launch out into snow-surveys. At the same time I am of the opinion that he was very fortunate in finding precipitation- and runoff-data which would relate so well, especially as there was only one precipitation-station for 1,100 square miles. In the Coastal Belt I have been unable to use precipitation-station data with any success as the stations are rarely located where they are representative of the upper watersheds.

I am all in favor, however, of making use of precipitation-data where possible and, of course, have done so extensively in the Columbia Basin in order to select strategic snow-course sites and study the possibility of discordant factors. It would be interesting to see Mr. Moore's method tried out in other areas.

CATCH-CAN MEASUREMENTS MEET POWER-FORECASTING REQUIREMENTS IN BISHOP-MONO BASINS

George Wills

I feel like a "black sheep" in the family here, and I have been made to feel that way in the past for the reason that, after participation in the snow-survey for a period of ten years, we have decided to stop the work. I do not want to give any impression, however, that we stopped the work because we did not think it was effective--the method was effective.

I will give you the figures of the watersheds we are interested in--of the snow-survey of 1935 and the record for the season of 1935-36. When the estimate was made, the estimate for Bishop Creek was 73,800 acre-feet and we measured 65,600; the estimate for Rush Creek was 46,000 acre-feet and we measured 40,100; the estimate for Leevining was 48,200 acre-feet and we measured 51,600; for Mill Creek we estimated 20,860 acre-feet and measured 24,300. These snow-surveys all compare very favorably with those in other areas.

I have also prepared a graph (Fig. 1) showing the total runoff on all of these watersheds compared with precipitation as measured at the catch-stations. Our method of making our forecasts is from catch-stations. We have catch-stations on Bishop Creek, Rush Creek, Leevining Creek, and Mill Creek. The elevations of the reservoirs are: Bishop Creek, 9,000 and 9,500 feet; Rush Creek, 9,800 feet; Leevining Creek, 9,400 feet; and Mill Creek, 7,700 feet. From this you will see that our reservoir-tenders are in a position to measure their catch-cans, or from the measurement of their catch-cans can tell very well what precipitation they have at that elevation. We have found from experience and observation that we can make a pretty fair forecast of what water we are going to have and, from that, what kilowatt-hours we can make. This is the extent of our interest in that water; and the points on the graph are equated to the relation of the water on the watershed. Thus it is a complicated figure and one probably of unusual character.

Our catch-station cans are cans about five feet in depth and have a cross-sectional area of exactly one square foot. One cubic foot of water weighs 62-1/2 pounds; therefore, if we have in our catch-can snow that weighs 5.2 pounds, we know that we have the equivalent of one inch of water for our records. We make our precipitation-report every two weeks starting in September. Reports of unusual storms are made without waiting for the regular two-weeks' period.

The following is a typical report:

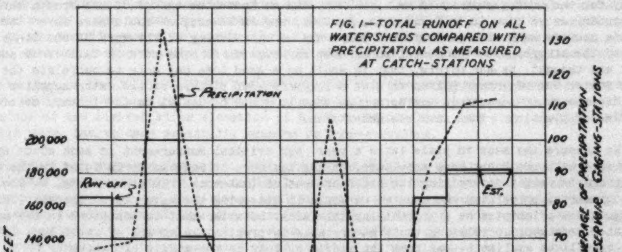


FIG. 1—TOTAL RUNOFF ON ALL WATERSHEDS COMPARED WITH PRECIPITATION AS MEASURED AT CATCH-STATIONS

WATERSHED	AREA IN ACRES	VALUE OF ONE ACRE-FOOT IN K. W. HOURS
Bishop Creek	37,320	2200
Rush Creek	14,150	1000
Leevining Creek	11,255	1264
Mill Creek	12,500	450

Precipitation-report as of 5:00 p.m. December 12, 1937

Item	Bishop Creek	Rush Creek	Mill Creek	Leevining Creek
	pounds	pounds	pounds	pounds
Storm of December 9-12 (all rain)	37.69	22.06	35.19	37.00
To date this season..............	40.45	28.57	42.25	54.33
Last season to December 15.......	28.32	22.02	24.45	23.07
Normal season to December 15.....	18.23	41.86	20.90	40.43
Weighted averages of total:				
To date this season....................		40.75, 173 per cent of normal		
To December 15, last season............		26.88		
To December 15, normal season..........		23.56		
Weighted averages as of December 31:				
December 31, of total this season.......		42.75		
December 31, for last season............		49.62		
December 31, of total for normal season.		32.97		
Total for season 129.7 per cent of normal				

On Bishop Creek for this storm we got 37.69 pounds. That makes 40.45 pounds on Bishop Creek to that date, but last season to December 15 had only 28.32 pounds at that point; so you see we had a whale of a storm. The weighted average as of that date this season was 26.88, while the normal of the period was 23.56.

Our estimate in kilowatt-hours was close; the year before--1935-36--we predicted 300,000,000 and we realized 302,000,000.

Nevada-California Electric Corporation,
Riverside, California

FROST IN THE SOIL A SUGGESTED FACTOR AFFECTING FORECASTS

W. W. McLaughlin

Four or five years ago I became interested in trying to determine what the probable water-supply for the coming year would be. In 1934, had we known the extent of the drouth throughout the country as we knew it in California, in Utah, and in Nevada, we could have saved the farmers in this country much time and effort. There was no correlating of the work of one State with that of the others, with the exception of some correlation of some work in California and Nevada; so it was thought, by our Bureau, that it would be a good idea not only to correlate the work of a few States but of enough States to give a comprehensive picture of the water-supplies of the West far enough in advance in the spring so that it would be useful to the farmers before and at the time of planting. That work was undertaken.

We measure the snow in place twice a year; our critical measurement is made about April 1. It is made with the Mount Rose snow-tube, taking the core of snow as it is found in the mountain areas, and by weight determining the water-content of that snow. In other words, we are applying engineering principles the same as we would in measuring the water in a reservoir, and through those principles we are obtaining the amount of water that is contained in the snow on the watersheds and, correlating that, we are able to predict the amount of water that is available, or will be available--say from the middle of July to the middle of September.

In Utah, if I remember correctly, the snow-courses are established at about 8,000 feet or higher, and the area in Utah above 8,000 feet is about ten per cent of the State. Thus this area furnishes the water-supply for 100 per cent of the State.

We have established altogether, with the cooperators, about 700 snow-courses. Our ultimate aim is possibly 900 snow-courses. We have, we believe, all of the key-stations established and many of the secondary, and many of the third or fourth streams or watersheds. This work was initiated originally by Dr. Church and later taken up in Utah by Professor Clyde. These two have pioneered the way for measuring the snowfall at the close of the season, converting it into inches of water and thereby obtaining an estimate of runoff for the ensuing year.

There are some factors that have to do with the runoff from snow of which we do not know very much. One of these is the frost contained in the ground beneath the snow-cover. This is a subject of great importance and value in predicting the runoff that will occur in the coming summer. I discussed the matter just recently in Washington with the Forest Service and that Service, like ourselves, is very anxious to determine whether the snow brings the frost out of the ground or whether the frost still remains at the time of the melting of the snow.

Another item of importance is the amount of moisture in the soil at the time of freezing in the fall, for the moisture-content in the soil-mantle beneath the snow-mantle is important as governing the amount of priming necessary before appreciable runoff is possible, and as affecting the accuracy of forecasts of runoff.

We consider a forecast of ten per cent as out of line and of five per cent as fairly good. When we have these other factors and a command of them, I think we will get down to five per cent--five as our maximum.

I think we could give some time at this meeting to the question of how to tackle frost at the time of melting of the snows. Whether thermometers will give us the answer, no one knows; we never have tried it. I hope to see Dr. Smith of this institution here at Davis, who perhaps knows more of soil-temperatures. I told the people at Washington that by a full line of stations, a hundred or more, we might get somewhere--maybe so, maybe not.

U. S. Bureau of Agricultural Engineering,
Berkeley, California

SYMPOSIUM ON ECONOMIC ASPECTS OF SNOW-SURVEYING

VALUE OF SNOW-SURVEYING IN THE TAHOE-TRUCKEE BASIN

George G. Devore

The economic aspects of snow-surveying to a commercial organization deal directly or indirectly in dollars and service for the final answer.

The snow-surveys on the eastern slope of the Sierra Nevada Mountains, where the winter precipitation is primarily in the form of snow, have as a rule not only permitted reasonably accurate estimates of the stream-runoff, but have also made possible fairly accurate estimates as to the time of the runoff. The importance of these two factors--that is, the rate of the runoff and the time of the runoff--cannot be overemphasized when considering the economic aspects of snow-surveying, especially when the use of the water is for power, domestic, and irrigation purposes.

In the case of the Sierra Pacific Power Company we are particularly interested in the runoff of the Truckee River and the Lake Tahoe drainage-basins. These basins are located on the eastern slope of the Sierras at an elevation of 5,000 to 10,000 feet, where the precipitation is principally snow, making them especially adapted to snow-surveying.

Key-courses are surveyed January 1, February 1, and March 1. A detail survey is made April 1 and a further key-course survey is made May 1 to confirm the detail survey. Based upon these snow-surveys and other related conditions, preliminary estimates of monthly natural river-flows and lake-levels for the current year are made February 1 and March 1, with basic estimates on April 1 and confirming estimates on May 1. From these estimates of monthly natural river-flow and Lake Tahoe levels, estimates are made as to the rate of regulated river-flow that can be maintained by the release of Tahoe waters to supplement the natural flow for the following 12 months.

During the past 15 years these regulated river-flows have been insufficient at times to permit the generation of enough power by the Sierra Pacific Power Company to meet its power-requirements and an interconnecting line with the Pacific Gas and Electric Company on the Yuba Watershed was constructed. Maintenance-work on this line is planned and arranged annually so as to be completed prior to the time when it is necessary to secure supplemental power from the Pacific Gas and Electric Company. This time is dependent upon the stream-flow which is estimated upon the basis of the snow-survey.

The time for a large part of the construction and maintenance-work on the power-plants and waterways is regulated, as far as possible, by the stream-flow as anticipated from the snow-surveys, and this work is planned and programmed to secure the most economic results. At times it is more economical from a power-purchase viewpoint to shut down a plant when a full water-supply is available for the other plants, while at other times it is more economical to delay the work if you can anticipate a river-flow where such a plant would necessarily be shut down for lack of water. The economic time to do such work depends upon what the water-flow will be at certain dates, and this flow must be anticipated in advance in order to secure the best results.

Other uses of the stream-flow data secured by the snow-survey are the determination, during years of water-shortage, of the time when the gravity domestic water-supply must be supplemented from other sources, and the time when the irrigation-water supply will become inadequate.

There are numerous advantages in being able to foretell the time when such shortages will occur, not only so as to be in a position to provide service, but also to arrange for it in the most economical manner. In anticipation of certain recent shortages it has been necessary to purchase water for domestic use and for irrigation from other sources--develop wells and pumping plants and expand the electric distribution-system in order to provide electric service for pumping plants. Numerous other items might be mentioned where benefits were secured, but they all resulted from ability to anticipate in advance when and to what extent the water-shortage would occur.

Our discussion has been limited more or less to the benefits resulting from the anticipation of water-shortages. However, recently a new use of the snow-surveys has been proposed, in regulating the storage in Lake Tahoe during periods of abnormal runoff. You are all more or less familiar with Lake Tahoe as a resort. As a reservoir it has a capacity of 720,000 acre-feet with a six-foot fluctuation above the rim at its outlet. However, if the water rises above this six-foot height, damage will occur to the abutting property, and to control this level it is proposed to utilize numerous snow-surveys during the winter and spring months in order not only to anticipate normal rises, but also to provide for abnormal and possible sudden rises in the Lake-level.

When the surveys indicate that there is more than sufficient water-content available in the snow to raise the water in the Lake to the six-foot level, water will be released from the Lake in advance of the spring runoff and before the Lake reaches the six-foot level. This is necessary in order to prevent the water from rising above this level as the rate of inflow less evaporation, under certain conditions, exceeds the maximum possible rate of discharge.

Studies have been made with actual water-conditions covering the period of 1907 to 1937 of such a control under various Lake-levels and with rapid rises such as occurred during 1907, 1928, and 1937, and it appears that with proper operation and frequent snow-surveys, the Lake-level can be controlled within the six-foot fluctuation. Under certain conditions this control will also assist in regulating river-flow during flood-conditions.

In addition to the benefits of better service and reduction of operating and maintenance costs secured for the Power Company by anticipating the river-flows in advance, is also the ability to estimate the amount of power that can be generated and the amount of power that must be purchased, and when such purchase will occur. These estimates are made for a period of 12 months in advance and are important items in determining the probable operating costs and dividends that will be available for the stockholders.

Sierra Pacific Power Company,
Reno, Nevada

ECONOMIC VALUE OF SNOW-SURVEYING FOR THE LOS ANGELES AQUEDUCT

J. E. Phillips

The evaluation of snow-surveying into terms of exact dollars and cents is difficult of accomplishment. In rare cases it may be possible to set a financial measure, but usually the best that can be stated is the general situation as viewed by the organization doing the work.

The Department of Water and Power of the City of Los Angeles appreciates the value of knowing early in the season just what to reasonably expect as the runoff from the snow-fields feeding the streams from which its Aqueduct water-supply is drawn. This supply is drawn from the surface-streams and springs to a large extent, but in subnormal years the shortage must be made up by pumping from deep wells. All three sources, of course, derive their supply and replenishment from precipitation on the watershed, which in this case is principally in the form of snow on the high Sierra Nevada Mountains.

To know in advance that pumping is to be expected permits the planning of the season's operations as regards stream-diversions so as to conserve the surface-flows; to handle the reservoir-drafts in such a manner as to have space for storage of the natural runoff when it shall occur; to avoid waste in anticipated irrigation-uses; also, to organize the operative force for efficient handling of the pumps and diversional works.

The Owens Valley deep-well pumping equipment is maintained in operating condition at all times. The work that may be required to deliver the water directly from the well to the main stream or aqueduct is the principal item to be considered, as preparatory work possible of accomplishment because of knowledge based upon the snow-survey.

The delivery of the water from the wells to the main streams is by means of short ditches and pipe-lines and a relatively small amount of work, usually performed as maintenance, is required to keep these in condition.

The introduction by artificial means of excess waters, not immediately required for diversion to municipal uses, into the underground areas is an operation whose possibility can be forecast from the knowledge gained through the snow-surveys. This putting of waters underground is referred to as "spreading" in the departmental records.

The value of the snow-surveys in spreading operations is the providing of a definite idea before the runoff-season as to how much water will be available, and this, when taken into consideration with the estimated drafts for municipal uses, indicates the limit for local or Owens Valley consumption. This local consumption can be set up as spreading, irrigation, and waste to Owens Lake. With the underground reservoir-areas in a satisfactory condition the irrigation of the lands owned by the Department can be given serious consideration. Satisfying the municipal demand always comes first.

The leasing of departmentally-owned lands yields a sum of money that varies with the character of the lease. Lands leased without water and actually receiving no water, will return only a nominal sum, while lands upon which leases can be made with a fair assurance of irrigation-water, rent for several dollars per acre.

The leasing of lands with water is reaching the stage where knowledge supplied by the snow-surveys is essential at the time of executing the irrigation-leases. No water needed for municipal uses within the City is used for the irrigation of leased lands, and the tenant must have a reasonable expectation of sufficient water in prospect to justify him in planting the annual crops for which he will pay the higher rental scales.

Experience has shown that in average, or greater-than-average years, sufficient water will be available to keep the Aqueduct running to working capacity, but in years of less-than-average flows thought must be given to the provision of reservoir-capacity at the times and places at which the runoff from the snow-fields will occur. Lack of storage-capacity in a subnormal year may make necessary an additional expensive pumping program.

Years of experience in handling the Owens River Drainage-Basin waters, the close estimates of the City's coming water-needs, and the final knowledge obtained from the snow-surveys as to the expectation of the coming season's water-supply demonstrate a very real economic value for the snow-surveys, though it is not possible to state this value in dollars and cents.

Department of Water and Power,
City of Los Angeles,
Los Angeles, California

COSTS AND BENEFITS OF SNOW-SURVEYING IN UTAH

George D. Clyde

Irrigation-agriculture depends for its water-supply upon the runoff from the high watersheds of the State. The fountain head for these waters is mainly snow which accumulates on the high watersheds and furnishes the storage from which most of the water-supplies in the West are drawn.

Precipitation varies over a wide range, and runoff which results from precipitation varies over a wider range. In the West it is not unusual to have a variation of from 20 to 300 per cent in seasonal runoff. This wide variation in stream-flow makes it extremely important to know in advance the probable seasonal flow if the water-resources are to be utilized to the greatest extent.

Irrigation-agriculture is extremely flexible. The cropping system can be adjusted in a large measure to the expected water-supply; for example, crops can be planned in advance of the planting season if the probable water-supply is known and only those crops that can be reasonably assured of a water-supply are planted. Power and municipal demands are not so flexible, but even for these uses, if the characteristics of a pending water-shortage are known, supplemental supplies can be developed or the least important uses curtailed.

The storage of water in the form of snow on the high watersheds makes possible a determination of the potential water-supply in advance of the occurrence of runoff. Such a determination is made by means of seasonal snow-surveys over these watersheds, and the use of supplemental hydrological, meteorological, and physical data pertaining to each drainage-area.

Water-shortages and drought are characteristic of an arid region, and forewarning of the occurrence of these conditions is about the only protection water-users have against the ravages of a deficient water-supply. Droughts of great magnitude have occurred in the past, and they can reasonably be expected to occur again. In the light of such expectancy, people living in a semi-arid or arid region should endeavor to become familiar with the characteristics of such situations and to develop practices and methods of agriculture and water-use to meet such periods of low water-supply. This is possible only if a knowledge of the probable water-supply is available before the planting season or period of maximum use begins.

The value of snow-surveying as a basis for predicting water-supplies has been amply demonstrated in Utah, Nevada, and California, and as records of snow-cover become available in other States, it will be demonstrated there also.

Snow-surveys and stream-flow forecasts were begun in Utah in 1923, first by the Utah Agricultural Experiment Station and later with the cooperation of the United States Forest Service, Weather Bureau, National Park Service, the Utah State Engineer, and the United States Bureau of Agricultural Engineering. These State and Federal agencies now support the Utah Cooperative Snow-Surveys and Stream-flow Forecasts under the direction of the Utah Agricultural Experiment Station.

The Utah Agricultural Experiment Station supervises all the work and makes the seasonal surveys on approximately ten per cent of the courses, and furnishes about 50 per cent of the equipment and the cash budget. The Utah State Engineer makes the seasonal surveys on approximately 15 per cent of the courses and provides approximately 50 per cent of the cash budget. The United States Forest Service furnishes no equipment or cash budget, but makes the surveys on 56 per cent of the courses. The National Park Service surveys 15 per cent of the courses and approximately four per cent are surveyed by municipal and private agencies. The Weather Bureau publishes the snow-cover data in the Monthly Climatological Data Sheets and has in the past furnished some survey-equipment which, however, is now obsolete. The Bureau of Agricultural Engineering, the most recent addition to our cooperative work, has furnished both equipment and cash budget.

The Utah Cooperative Snow-Surveys directed by the Utah Agricultural Experiment Station, from which the stream-flow forecasts originate, and which cooperates closely with the Bureau of Agricultural Engineering, the coordinator of all snow-survey and stream-flow forecasting in the Western States, has proven entirely adequate for the conditions found in the State of Utah.

The Utah Cooperative Network now consists of 85 snow-courses. The total expenditures to date by all agencies in cash and time equals approximately $37,500, or an average of $2,500 per year. Spread over the irrigated acreage this amounts to $0.0018 per acre per year. The estimated total cost for the year 1937-38 is $4,000 or $0.003 per acre. The total costs to date cover both actual field-costs and cost of research.

Snow-surveys and stream-flow forecasts were begun in Utah in 1923. In 1926 the first water-shortage occurred. No quantitative measurements of probable shortage were available, but the deficient snow-cover indicated a water-shortage and a general curtailment of late-season crops followed. Those who reduced their late-season crops matured what they planted. Those who planted regularly did not mature a full crop. Much of their labor and seed was wasted. Five years later, in 1931, another drought was evident before the beginning of the planting season. This year the sugar companies would not contract beets until the water-supply forecast was available, and the banks would not lend money on crops before the extent of the probable water-supply was known. As a result of these precautions most of the late-season crops that were planted were fully matured and the effects of the drought materially reduced.

The drought of 1934, which was the most severe in the history of the West, taught many lessons, the most important of which was the necessity of having a full knowledge of an expected drought prior to the beginning of the planting season as well as full cooperation among users of water. Any year may be a dry year, but the probable extent of the drought which follows a deficient precipitation can be ascertained by a measurement of the accumulated precipitation which is stored in the form of snow on the high watersheds.

It was known from mid-winter snow-surveys over the Utah Snow-Survey Network as early as February, 1934, that Utah was facing a serious drought. There was an extremely light accumulation of snow on the high watersheds and the earth-mantle under the snow was dry. The lower watersheds were completely devoid of snow and the valleys had experienced only light precipitation. This early warning was emphasized by deficient precipitation during the months of February and March, and on the first of April, as a result of measurements made under the State-wide network of snow-surveys, it was clearly indicated that Utah faced the worst drought in her history. At that time the potential water-supply did not exceed 35 per cent of normal. The conditions were brought to the attention of the far-sighted Governor of Utah on April 1 and he immediately set in motion machinery necessary to meet the situation. He called the first drought-conference in history to be held before the drought occurred. At this conference two lines of action were developed: First, to put into effect immediately a water-conservation program, and second, to begin immediately the development of supplementary water-supplies so that they might be available by the time the crops needed the water.

The water-conservation program involved an educational campaign to acquaint the water-users with the situation and with the essential modification of acreage to be planted, methods of irrigation, and administration of the water to secure maximum use of the water available. Unusual as it may seem, the water-users of the State, in the main, were unaware of the seriousness of the situation. They had not been in the habit of considering their water-supply above their headgate. They assumed that water would come because it had always come before, and they did not take into consideration whether or not there was any water on the watersheds to supply the streams.

As soon as the water-users were convinced that a real shortage of water existed, they began planning and executing a program to combat the drought. They did not wait until the drought was upon them before beginning operations, and as a result they matured most of the crops they planted. They saved their orchards and perennial plantings, they moved their livestock off the ranges and out of the State before they starved, and they developed supplementary water-supplies for irrigation and domestic purposes before the ordinary sources of supply dried up or got so low as to be insufficient to meet the minimum demands.

The water-development program which was inaugurated upon the issuance of the water-supply forecast enabled the State to develop approximately 400,000 acre-feet of water in time for use in saving crops. Estimates based on measurements made during the season of 1934 indicate that a saving in crops alone due to the program of water-conservation and development of supplemental water-supplies was over $5,000,000 in Utah. On the Bear River, which supplies lands in Utah and Idaho, a total of 150,225 acres were supplied with supplemental water by this program. It resulted in a saving in crops of $3,149,000 in Utah and $684,000 in Idaho, or a total saving of $3,833,000 in the two States. In addition to this, a tremendous saving in livestock was effected through the Federal purchase plan which took the cattle off the ranges and out of the State before they starved or choked to death.

The water-supply forecasts enabled municipalities to early inaugurate a water-conservation program and a supplementary development program which gave them sufficient water in most instances to carry them through the drought. It enabled power-companies to provide supplemental steam-power in advance of its needs. Early warning of a water-shortage enabled Salt Lake City (population 125,000) to develop a supplemental water-supply of 40 to 50 second-feet from ground-water sources to avert a serious municipal water-supply shortage. Many small communities actually had to haul water for a short time until the supplemental supplies could be made available. These latter benefits cannot be evaluated in dollars and cents, but they rank high in prevention of human suffering and perhaps death.

The experience of Utah in 1934 was duplicated in many other Western States and clearly demonstrates the value and necessity of seasonal snow-surveys upon which stream-flow forecasts may be based.

We have considered thus far the value of snow-surveys and stream-flow forecasts only from the standpoint of water-shortage and the effect of such shortages upon irrigation, power, and municipal uses. Such surveys and forecasts are just as valuable from the standpoint of flood-protection and the operation of flood, power, and irrigation storage-reservoirs. Water under control is man's greatest asset. Out of control it may become his greatest enemy.

Potential flood-conditions are revealed by snow-surveys. In 1936 the benefits of snow-surveys and stream-flow forecasting, from a slightly different angle, have been made evident. The winter of 1935-36 was one of heavy snowfall on the watersheds of Northern and Central Utah. The snow-cover measurement on April 1 indicated an exceptionally large quantity of water in snow-storage. In addition to the heavy high snow-cover there was an unusually heavy low snow-cover of high density. These conditions constituted potential flood-flow on many of our streams. Warnings were issued prior to the runoff-season with the suggestion that stream-channels be cleared if the damage from high spring runoff was to be minimized. On many streams such precautions were taken, and the high water did very little damage; on other streams no precautions were taken and, as a result, considerable damage occurred.

On the Price River a peculiar condition existed. The Scofield Reservoir contained approximately 12,000 acre-feet of hold-over storage. The capacity of the Reservoir was limited to 30,000 acre-feet due to damage to the Dam which occurred in 1928, and to an inadequate spillway and outlet-tunnel capacity. The heavy snow-cover on the watershed on April 1 caused considerable concern among the people who lived below the Scofield Dam, and therefore, they requested the State Engineer to empty the Reservoir. On the other hand, the irrigators, who were dependent upon the water from this Reservoir for their irrigation-supply, were opposed to the emptying of the Reservoir because they felt that possibly the runoff from the watershed might not be sufficient to refill it. Snow-surveys had been made on this Watershed since 1925 and, on the basis of these snow-cover measurements and available stream-flow records, there appeared to be a potential runoff in snow-storage above the Scofield Reservoir on April 1 of between 60,000 and 70,000 acre-feet. This was approximately twice the capacity of the Reservoir. A conference of all interested parties was called at which all of the conditions pertaining to the water-supply and snow-storage, the runoff characteristics of the Watershed, and the conditions at the Reservoir and Dam were discussed. A plan of reservoir-operation based upon snow-cover measurements was decided upon at this meeting and accepted by the group. This plan of operation provided for the immediate emptying of the Reservoir and keeping the gates open until a total of 20,000 acre-feet of water had passed through the outlet tunnel, at which time the Water Commissioner was authorized to close the gates either partially or completely depending upon the snow-cover remaining on the watershed. By means of periodic snow-cover measurements it was possible to so regulate the storage that by the end of the runoff-season the Reservoir was filled to capacity. This plan was carried out, and in spite of the fact that practically no precipitation occurred during the period April 1 to May 1 the Reservoir was completely filled by the beginning of the irrigation-season. The Reservoir had been operated so as to assure minimum danger to the inhabitants below the Dam and at the same time assure a full and complete irrigation-supply to the water-users. Such a program would have been impossible without the information furnished by the Utah Cooperative Snow-Surveys.

In 1937 a similar condition existed on the Price River and on the Beaver River. The State Engineer regulated the storage on these streams according to the potential supply of water available on the Watershed as shown by the snow-surveys. Again he successfully passed the flood-flow without damage and yet stored an adequate irrigation-supply. Here, again, the benefits cannot be evaluated in dollars and cents, but when potential flood-conditions are known to exist, precautions can be taken to clear channels, protect bridges, and headworks, release accumulated storage, and thus greatly reduce the damage from the high-water flows which result from a rapidly melting heavy snow-cover.

From an actual money standpoint the savings in dollars to agriculture, during 1934 alone, which resulted directly from snow-surveys and stream-flow forecasts was 135 times the entire cost of snow-surveying in the State of Utah for 15 years, yet the benefits that can be reduced to a dollar value are but a small portion of the total.

The value of snow-surveying and stream-flow forecasting is both material and spiritual. It saves labor and seed when to plant would be useless because of no water to mature crops. It permits a maximum utilization of water-supplies, and creates confidence instead of suspicion between users of water. It gives water commissioners courage to allot water not yet in the reservoirs and makes it possible for the farmer to plan his crops to fit his water-supply. It has enabled power-companies to provide supplemental power in advance of its needs and municipalities to plan use-restrictions or develop supplementary supplies before a water-shortage actually occurs. In short, snow-surveys and stream-flow forecasts have reduced materially the uncertainty which has heretofore been attached to the West's greatest resource--its water-supply.

The people of the West are sold on snow-surveys and stream-flow forecasts. They are convinced that they need information with respect to their water-supply above their diversion-works. They are convinced that they must measure their water-resources on the watersheds, and that snow-surveys have been found to furnish the most practical means of such measurement.

Utah Agricultural Experiment Station,
Logan, Utah

IMPROVEMENT OF SNOW-SURVEY EQUIPMENT

IMPROVEMENT IN SKI, DURALUMIN SAMPLER, AND WEIGHING APPARATUS

Philip S. Cowgill

The most serious problem with which the snow-surveyor has to deal while in the field is that of equipment. By equipment I mean not only the instruments used in making the actual measurements, but also such items of personal equipment as clothing, knapsacks, and skis, and I believe that during recent years the greatest improvement has taken place in this latter group.

When I first started working with Dr. Church more than 25 years ago we used to start up Mount Rose looking like a pair of Egyptian mummies. In those days we wore heavy sheepskin-lined coats, blanket-lined trousers of heavy canvas, felt boots with rubber overshoes, fleece-lined leather mittens, and knitted helmets topped by plush-lined caps. In stormy weather we usually wore, in addition, canvas leggings reaching from ankles to hips. The wonder to me now is that we were able to carry our clothes, not to mention the necessary packs. My first knapsack was a barley sack with a rope tied to the two lower corners and looped around the top. True, owing to the thickness of clothing, the rope did not cut into the shoulders as badly as might be expected. We used webbed snowshoes most of the time as we found that we could not control the skis sufficiently with the old "toe-strap" bindings, and also the snow was too liable to clog on the bottom of the skis.

Now, most of our Nevada parties, at least in the Sierra Region, wear regular ski clothing of the popular type and carry some one of the modern style of packsack, frequently one of the framed type which puts the weight principally on the hips, thus giving greater freedom to the arms and shoulders. Skis, waxed according to the condition of the snow and equipped with some form of rigid bindings, are used almost entirely. Ski-waxes also have attained greater simplicity and adaptability to meeting diverse conditions of snow-surface.

The trip to the Granite Peak Course in the Truckee Basin offers a good example of the advantage of skis over webs. When webs were used the trip from the car to the course required a minimum of five hours' hard climbing, while the return took about three hours. During the last five years, with skis, the trip up has averaged less than four and one-half hours and the return has required from 30 to 45 minutes, depending on the skill of the individual.

If the new duralumin sampling tubes prove to be satisfactory, they will be the greatest improvement in sampling equipment which I have seen in many years. However, our experience last year was somewhat unsatisfactory and I do not like to express an opinion without further trial. We were informed that it was unnecessary to shellac the tube and consequently we did not do so. The tubes worked excellently in comparatively warm weather but on very cold days a great deal of difficulty, caused by the core freezing to the inside of the tube, was encountered. This was so serious at times that in an eight-foot hole a core of only ten to fifteen inches would be obtained and then as much as fifteen minutes would be consumed in removing it. After the core was removed, a layer of ice about an eighth of an inch thick would remain all around the tube. Whether or not shellacking the tube would eliminate this trouble I cannot say at present, but this year I hope to experiment along this line.

In this connection I believe that a series of experiments to determine, if possible, the effect of different temperatures and atmospheric conditions would be valuable. The coefficient of thermal conductivity of duralumin being considerably greater than that of steel, it may be that when the atmospheric temperature is much lower than the snow-temperature the duralumin cools the core and causes freezing within the tube. A careful series of temperature-measurements of the air, snow, and metal might reveal some interesting facts. It seemed at times, although none of our men were positive, that on very warm days the duralumin tubes drove more readily than the steel ones. This fact, if such it is, could also be explained by the difference in conductivity.

The tendency of the duralumin to seize has frequently been commented upon and was noticed here. However, we had little difficulty in that way since before sending out any of the new sets I had drilled them for spanner-wrenches and had all parties equipped with spanners. The new spanner with the 140°-bend and long handle, which Dr. Church is now advocating, is a great improvement over the old 90°-degree type and should be furnished to all parties. The hole for the spanner should be drilled completely through the metal and the pin in the spanner should be long enough to project slightly into the inside of the tube. In some cases I found that when a spanner- and a driving-wrench were used the joint turned on the tube without starting the threads. The use of two spanners with long pins, instead of a driving-wrench prevented this. I had some difficulty with the driving-wrench slipping out of the slot, although none of the tubes were seriously injured. I believe that the suggestion that the key be notched to fit the end of the slot is excellent.

Another weakness was developed, due perhaps to looseness at the couplings rather than to thinness of the tubing. In standing on the driving-wrench, the sections became somewhat kinked. By turning 180° and putting the same strain on the sampler, the kink could be straightened out again. The tubes are also badly finished at the joints, leaving a ridge which causes plugging and prevents the cores from coming out cleanly.

The 30-inch length of section is of great advantage for carrying but has the disadvantage of an increased number of joints, tending to make driving harder, particularly where layers of ice are encountered. An increase in length to 36 or even 42 inches would add but little to the difficulty of carrying and would correspondingly reduce both the resistance to driving and the cost of the set, not to mention the reduction in the number of joints to be coupled and uncoupled.

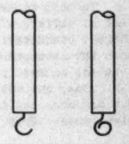
As to balances, the Mount Rose type still remains the most satisfactory for our locality where large variations in depth are encountered. For example, in the Independence Lake Region we have two courses on which depths will run from three to six feet and a third on which the depth varies from seven to 18 or 20 feet. Using the tubular balances, it would be necessary to carry two, whereas with the Mount Rose type only one is necessary. Furthermore, with a density of 40 per cent, which is not at all uncommon, the tubular balance has not sufficient capacity. The Mount Rose balance now made by Leopold and Volpel has a friction-clasp on the adjusting screw, which tends to prevent changes in adjustment while carrying the balance from point to point. The swivel-links at top and bottom are also an advantage. The balance has one weak point in construction, which lets the rack fall away from the pinion. This can readily be corrected by a pin. To reduce friction, R. L. Parshall has suggested using a magnet to hold the rack to the pinion.

The weighing staff seems to cause much concern in some quarters, but personally I regard it as a very minor point. In the early days of the work we carried a long stout pole, whether using skis or webs. Driven into the snow, this provided a very satisfactory weighing staff. Now, I simply use the regular ski-poles, two of which are carried by each man as a part of his regular equipment. By twisting the hand-straps together three poles form a very secure tripod from which the balance can be hung. The only disadvantage is that the tube must be run under the tripod from one end, which, in the case of long lengths, is somewhat difficult. By using only two poles they can be leaned forward so that the balance swings clear, but in this case one man must steady the poles. This can ordinarily be done by placing a ski on the button of the upper pole. Using the poles provides a steadier support than if the balance is held in a man's hand or suspended around the neck, and yet eliminates carrying an additional piece of apparatus.

In snow-surveying, even more than in most types of field-work the most essential characteristics of apparatus are portability, strength, and convenience of operation. Accuracy must be maintained, but the nature of the work does not require the high degree of precision so necessary in many kinds of instruments. Any change which will increase even one of these without sacrificing another will be welcomed, but we field-men are a rather cautious lot, and the advantages of any new device must be definitely proved before we are willing to discard something which has been tried and proved by long experience to be satisfactory.

(Note by J. E. Church--The United States Bureau of Agricultural Engineering cylindrical balance has an essential improvement which should be noted. A spiral hook has been substituted for the open hook previously used to support the cradle in which the snow-sampler tube is hung. This will ordinarily prevent the cradle of the balance from falling out during carrying. The advantage is shown in Figure 1.)

FIG. I



Sierra Pacific Power Company,
Reno, Nevada

FURTHER OBSERVATIONS AND TESTS ON DURALUMIN SAMPLER

J. E. Church

At the March snow-survey Ranger Haycock found the duralumin disappointing even in the comparatively shallow snow of the Marys River Basin of the Upper Humboldt, Nevada. In the deeper snow at Independence Lake in the Sierra Nevada, the party also could not get cores with it and unfortunately did not carry a steel sampler to make further comparisons of duralumin and steel. The day was "raw and cold." However, could not the air have been sufficiently warm to cause moisture on the sampler and consequent freezing of the sampler to the snow? This result had been observed by John T. Ryan in a series of experiments.

On March 20, the following observations were made by J. E. Church and an independent but inconclusive test was made by Philip S. Cowgill:

"March 20, Sunday--Blizzard at intervals here at Reno. Snow plastered against autos and trees. In early morning until 9 a.m. coating of ice on grass and on some of the sidewalks. At noon, walks dry. Then snow-pellets fell which melted promptly after rebounding from pavement and soil and causing vapor to rise, thus showing soil and objects opaque to heat rays to be warm although the air was still cold. Possibly this is 'sticky' weather for snow-samplers. Test it. At 3:30 p.m. ice is forming on the sidewalks again and the vapor noticed at noon is scarcely visible. Evidently the ground and concrete are cooling as the Sun sinks lower. (J.E.C.)"

"Test of samplers: (1) Snow-depth 94 inches; density 35.5 per cent. (a) Air-temperature 27° F and snow-temperature 34° F. With shellacked sampler, full depth, no sticking. (b) Air-temperature 27° F and snow-temperature 32°.5 F. With unshellacked sampler, core froze solidly to sampler.

"(2) Snow-depth 91 inches; density 29.3 per cent. (a) Air-temperature 27° F. With shellacked sampler, full depth. (b) Air-temperature 26°.5 F and snow-temperature 32° F. With unshellacked sampler, coat of ice around inside of sampler. Temperature of snow at depth of 44 inches was 32° F and of snow-sampler in snow at snow-surface was 38° F.

"(3) Snow-depth 86 inches; density 34 per cent. Shellacked sampler left in snow for 30 minutes, no sticking when removed. Air-temperature 26° F, snow-temperature at surface 32° F, at depth of 80 inches 32° F.

"(4) Snow-depth 91 inches, density 34 per cent. Temperature at 85-inch depth 32° F. (P.S.C.)"

The following detailed series of tests by Mr. Cowgill demonstrates the necessity of a heavy coating of shellac for duralumin samplers but still leaves the cause of adhesion uncertain:

"On March 27, I made a trip to Spooners Summit, Tahoe Basin, on the Clear Creek Highway to carry on further experiments with the duralumin snow-samplers. On this day the air-temperature in the shade averaged 45° F. The snow-temperature six inches below the surface was 28° F, and at about 90 inches was 32° F.

"The mean of 15 samples taken with a thoroughly shellacked sampler was as follows: Depth, 103 inches; core, 102 inches; water-content, 40 inches; density, 38.9 per cent. The snow was well packed and contained two layers of crust under the surface-layer. The surface was wet for a depth of about three inches. Driving was difficult, it being frequently necessary to stand on the driving wrench in order to reach ground. However, the cores were in all instances practically perfect. No difficulty whatsoever was experienced in removing the cores, which would slide out cleanly with just a light shake or jarring of the inverted tube.

"About a dozen attempts were made with an unshellacked tube but practically all were complete failures. The tube could be driven to a depth of from 40 to 46 inches. Beyond this depth it was utterly beyond the power of two persons working together to force the tube. Cores obtained averaged about 15 inches in length. It required from five to 15 minutes to remove these cores from the tube and a layer of ice approximately 1/4-inch thick always remained. The experiment of leaving the tube lying in the snow for approximately half an hour was tried. After several attempts it was found that by picking the tube up quickly and driving with great force and as rapidly as possible, the tube could be driven all the way down and a reasonably good core obtained. However, any hesitation in driving resulted in a complete stop and a frozen core.

"As a further experiment both tubes were driven to a depth of about 90 inches and thermometers were taped against the metal just above the surface of the snow, the bulbs being covered with several layers of adhesive tape. The thermometers were placed on the shady side of the tubes. With the air-temperature 44° F, the thermometer on the shellacked tube read 48° F and that on the unshellacked tube read 42° F.

"The snow-thermometer was the usual type inclosed in a glass tube, the ends of which were closed with cork sealed with paraffin. This was lowered into an 85-inch hole, which was then covered and left for about three-quarters of an hour. On attempting to withdraw it, it was found to be frozen down. All attempts to raise it were futile until the sampler was driven down beside it and used as a pry. This resulted in breaking off the glass tube about 1-1/2 inches above the bottom. This end and the cork remained in the bottom of the hole. Fortunately the thermometer was uninjured.

"On the March 1 surveys at Webber Lake, difficulty with freezing cores was encountered. Examination of the tube revealed the fact that a few spots had not been properly coated with shellac. After the tube was reshellacked no more difficulty was experienced even with weather- and snow-conditions apparently most adverse.

"At Independence Lake with comparatively warm weather on the low courses no difficulty was experienced, but on the high course with cold weather and snow-squalls sampling proved to be extremely difficult. Again careful examination revealed the fact that the shellacking had not been thorough.

"My conclusion is that if the duralumin tube is very heavily shellacked it will drive as readily as the steel and that the results will be equally good, but that the coating must be heavy and uniform. This appears to be more necessary than with the steel. Even one or two small spots that are not covered will cause sticking and preclude the possibility of obtaining good cores.

"Last year we had difficulty with the joints sticking and in one case, by using driving wrenches to unscrew the couplings, turned the coupling sleeve on the tube. This year we have been using two spanners with pins projecting through the tube and have had no difficulty. However, on April 1 at Webber Peak a 240-inch sampler was driven to a depth of 191 inches and stuck in ice. One man was unable to raise it, so two took hold of the driving wrench. This resulted in pulling the coupling sleeve entirely off the tube at the 180-inch joint. By digging a hole and placing the wrench at a lower point the rest of the tube was recovered. This particular set is one that was issued last year, but was not the set on which the joint was started in uncoupling. To date that joint has given no more trouble. However, from these experiences it is my conclusion that the joints should be riveted.

"Since the above was written the April 1 survey-party has returned from Independence Lake and reports that, with a properly shellacked sampler, no difficulty was experienced even under most adverse weather-conditions.

"It seems to me that the results of all our experiments and of our experience on actual surveys offer conclusive proof that it is fully as necessary to shellac the duralumin tubes as it is to shellac the steel ones."

A laboratory-study will next be made of the relative conductivity of shellack as compared with duralumin and steel and of the effect of heat-conductivity on the adhesion of snow and ice to duralumin and steel, with and without shellac. The problem seems to lie in this narrow field. Because of their durability, enamels such as Duco will also be tested as a possible substitute for shellac. Director Doten comments: "Steel and duralumin, like all metals, are far better conductors of heat than such heat-insulating substances as paper, shellac, et al."

Nevada Agricultural Experiment Station,
Reno, Nevada

ATTACHMENT OF CUTTERS AND COUPLINGS; SPANNER-WRENCHES

John T. Ryan

Cutter-joints--During the past year, at the request of Dr. Church and Mr. Elges, I have conducted a number of experiments and tests on two methods, other than the one now used, of joining the sections and cutters of the sampler-tubes. This work was carried out in the Mechanic Arts Shops and Civil Engineering Laboratory of the University of Nevada. The purpose of these experiments was to develop a joint between the aluminum tube and steel cutter that would make the replacement of the cutter possible in case of damage under conditions encountered in the field.

The first method tried was a threaded bushing. The bushing was made from steel tubing of the same inside diameter as the inside diameter of the cutter. The overall length of the bushing was slightly more than twice the length of the cutter-shank. One inch of this length was turned to make a pressed fit in the tube and the balance was made the same as the outside diameter of the tube. This larger portion was then threaded internally (24 threads per inch) to receive the threads chased on the cutter-shank.

The second method was a soldered joint, using aluminum solder (Alunize). The cutter-shank was ground to give a clearance of five to ten one-thousandths of an inch and the surface was prepared according to directions furnished by the makers of the solder, the essence of which is to tin thoroughly with common lead solder, wash off any flux and tin thoroughly with aluminum solder.

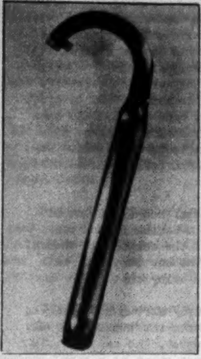


FIG. 1—NEW TYPE WRENCH FOR SAMPLER-TUBE

The part of the sampler-tube to be joined was then scraped clean and the surface tinned with aluminum solder using no flux. Both parts were then heated to the melting point of the aluminum solder (392° F) and brought together. The results showed consistently greater strength when allowed to cool slowly than when cooled by plunging in water. The average of the result of the tests on a number of the joints so made was approximately 4,500 pounds pressure to shear the solder and 2,000 pounds pressure to continue movement for one-fourth inch. In making these tests, compression instead of tension was used for the sake of simplifying holding devices. The joint was set with the cutter about three-eighths of an inch from its shoulder. Consequently the force necessary to continue the separation of the joint is probably less owing to the fact that no additional solder will oppose the movement. The same test applied to joints as originally supplied, or now in use, shows approximately 600 pounds pressure necessary to start movement.

The data on some of the tests are attached to this paper and the complete record is available in my office.

In comparing the results of these two methods we have, for the first, the advantage of a simple, positive, and reliable connection requiring no particular skill or ability to make, against the necessity of a special wrench for removal if set up tightly. Also the increased cost of manufacture (about 30 to 40 per cent of the first or cutter-section) might be regarded as unfavorable. For the second method, the advantage of greater strength, ease of replacement by the application of heat, which may or may not be readily available, and with very little if any additional expense of manufacture, may be obtained. The only disadvantage is the danger of destroying the temper of the cutter or tube by overheating, although no trouble was experienced in this respect in making and separating some 50 joints.

In making the soldered joints in the laboratory a Bunsen flame was used, and ten joints each were made using the fire in a small heating stove and over an open wood fire. When it was not necessary to tin the cutter-shank with lead solder, the wood fires gave good results, but the smoke and ashes caused trouble if complete preparation was required. However, the original lead tinning was found to last for an average of two joints. As there would usually be no object in replacing the damaged cutter, this fact need not be considered. The extra cutters would have been already prepared in the laboratory or shop. The difference in the results obtained and the method of application between the aluminum solder marketed by an American concern and that of an English concern is negligible.

Couplings--Inasmuch as soldering of the cutters has shown greater strength than the pressed joint, it would seem advisable to use this method also of attaching the coupling sleeves to the tubes.

The coupling, being pressed on the tube, is liable to loosen due to the strain of driving and also from the force necessary to separate the sections when tightened excessively. It might also be advisable to include rivets on this joint to hold the assembly together in the event that the coupling should shear from the tube under extremely difficult driving conditions.

Spanner-wrenches--To replace the 90° spanner-wrench with its four and one-half inch handle that has not proven satisfactory, a wrench (Fig. 1) was designed that would give increased leverage and eliminate the probability of the pin being forced out of the hole under pressure of use and at the same time tend to act perpendicularly to the axis of the tube, all of which features are lacking in the 90° wrench.

Two models of such a wrench were made up and found to be satisfactory in all the above points. To accomplish these results the arc of contact of the wrench was made between 140° and 150° and the handle extended from the point of tangency opposed to the pin. The overall length of the wrench is 8-1/2 inches. Applying pressure to the handle when the wrench has been placed in position on the joint causes the pin to seat in the hole and part of the force is taken by friction along the arc of contact, thus relieving some of the strain on the pin and hole. Another advantage gained by this design is the tendency of the wrench under pressure to inherently assume a position perpendicular to the axis of the tube. In other words, the pin will not pull out of the hole and the wrench will not slip off the joint when in use. All the effort applied by the person using it is utilized in separating the sections and not in holding the wrench in position.

To save as much weight as possible, aluminum alloy was suggested by Dr. Church as a material, and a sketch of the model was sent to the Aluminum Company of America with a letter asking if it would be practical to make such a wrench of that material. The answer was in the affirmative and a quotation was submitted on the cost of manufacture which was as follows (these quotations were made November 24, 1937, subject to acceptance within 30 days, so they are not effective now, but will probably not differ greatly): (1) As a forging (recommended) in lots of 500 to 1,000 units, $0.37 each; tool-charges, $392.00; individual set-up charges per run, $4.85. (2) As a casting (not recommended) in the same number of units, $0.41 each; permanent mold-cast, $186.00; no set-up charge. Reduced to unit-cost on a basis of 1,000 wrenches, this is $0.77 and $0.60, respectively. Additional wrenches will cost $0.37 each plus $4.85 per run forged and $0.41 each cast. The tool-cost is considered in the initial supply only.

The catalog retail price on the 90° commercial wrench is $0.53 unfinished and $0.66 finished. I understand the quotation submitted on the designed wrench as a medium carbon steel forging (0.3 to 0.6 per cent carbon) was four to five times the cost of the commercial wrench. With a discount of 50 or 60 per cent on the finished wrench, which is the one supplied, this figure does not seem reasonable in view of the estimate on the aluminum-alloy unit.

Both the forged and cast aluminum-alloy wrenches will be sufficiently strong according to tables of mechanical properties and specifications (see below) to separate the tubes under any condition even if it becomes necessary to strip the entire thread. However, the cast unit is liable to breakage from shock while the forged unit will withstand as much abuse as a steel product. As the strength of the aluminum-alloy wrench appears to be satisfactory and, of course, offers no corrosion difficulties, no inquiry was made concerning the cost of a wrench made of stainless steel, which would be somewhat greater.

The mechanical properties and specifications for aluminum-alloy wrenches are:

(1) Forging 14S-T: Yield strength 50,000 pounds per square inch; elongation 10 per cent in two inches; Brinell hardness 130 minimum (500-kg, 10-mm ball).
(2) Cast B-195-T6: Minimum tensile strength between 33,000 and 42,000 pounds per square inch; elongation 4-1/2 per cent in two inches; Brinell hardness between 70-110.
(3) 1035SAE steel forging: Yield-strength (no heat-treatment) 40,000-60,000 pounds per square inch.

Curvature of tubes--A defect to be remedied is the curvature of the sampler-tube caused by the rolling or stamping of the graduations. This expands or stretches the metal on the side graduated and causes the section to be curved. To correct this it will be necessary to stretch the opposite side accordingly. A method of accomplishing this has been proposed but not tried. Some sections have been straightened by hand in the laboratory with a formed punch, but this defect should be corrected by the manufacturer using a blank counter-roll if a roll-stamp is used. The cutting of the slots has no effect on the straightness of the tube.

Solder-tests and directions--Test No. 1--Joint soldered according to directions on clean surfaces, cooled by plunging in water. Results: 3,800 pounds to shear soldered joint; 2,000 pounds frictional resistance maintained; normal clearance 0.006 approximately.

Test No. 2--Joint resoldered without retinning cutter, cooled by plunging in water. Results: 2,200 pounds to shear resoldered joint; 1,700 pounds frictional resistance maintained.

Test No. 3--Joint soldered according to directions on recleaned surfaces, allowed to cool in atmosphere. Results: 4,600 pounds shearing; 2,300 pounds frictional.

Test No. 4--Joint resoldered with addition of aluminum solder only, cooled in atmosphere. Results: 4,600 pounds shearing; 2,400 pounds frictional.

In joining aluminum sampler-tubes and steel cutters by soldering, two brands of solder were used which gave practically the same results, using the same method of application.

The simplest method giving satisfactory results was as follows: Clean cutter-shank with emery-cloth or sand-paper and tin thoroughly with ordinary soft solder using any of the common fluxes. Clean the part of the sampler-tube that is to receive the cutter-shank with emery-cloth or sand-paper and heat over a fire until hot enough to cause aluminum solder to crumble and flow. Then with a steel scraper, or any steel object that can be used, work the solder around in the tube until it appears to adhere, keeping the tube just hot enough to keep the solder flowing. When the tube has been reasonably well tinned, the cutter can be started in the tube far enough to hold it and the assembly held over the fire until the two slip together easily. The joint should then be allowed to cool in the air. If plunged in water, the joint is only about half as strong as when cooled slowly. This method produces a joint requiring between 4,500 and 5,000 pounds pressure to separate, which is about 4,000 pounds more than that required to separate the shrunk-in cutter.

To remove the cutter after soldering, hold the tube over a fire, keeping the cutter out of the flame if possible until they can be pulled apart. If the cutter is to be replaced with another already tinned, it will only be necessary to start it in the tube and heat until it can be pushed home. The original preparation of the tube has been found to last from three to five replacements.

The cutter should not be heated to a higher temperature than necessary to accomplish the joining and separation as its temper can be easily destroyed. The temperature of flowing solder is 392° F or 100° below ill effects on the cutter. The two brands of solder tried were "Alcoa T" and "Alunize." The Alcoa T can be used with Alcoa A flux, but the results obtained were satisfactory without its use.

In these experiments no attempt was made to develop maximum strength of joints but merely to discover a product and method that would give uniformly satisfactory results under field-conditions.

Alcoa is a product of the Aluminum Company of America. Alunize can be obtained from the Coventry Aluminum Engineering Products Company, Coventry, England.

Superintendent of Shops,
University of Nevada,
Reno, Nevada

DISCUSSION

R. C. FARROW (Water Rights Branch, Department of Lands, Victoria, British Columbia, letter, May 28, 1938, re seizing)--You may remember my contention at both the Snow-Survey Conference at Denver and at Davis, California, that most of the seizing of the joints of snow-tubes occurred where the edges of the couplings came into contact rather than in the threads themselves. Two aluminum surfaces bearing on one another tend to score, and, as the scoring took place, my theory was that the seizing occurred.

I, therefore, had thin brass washers made to insert between the couplings so that the bearing surfaces were brass against aluminum. I used a seven-section set so equipped in deep, dense snow with heavy intermediate crusts, which necessitated standing on the driving wrench and constant twisting of the tube, all of which tends to tighten the joints. Despite this, we were able to unscrew the sections without difficulty with our bare hands, without resorting to the coupling wrenches. Anti-seize compound was also used.

As compared to this, using a slightly older set not equipped with driving wrenches and with no brass washers, but using anti-seize compound: After sampling soft, shallow snow using three sections, I was quite unable to unscrew them by hand, and in the end had to take the sampler to a machine shop and use a vise and pipe-wrench. I consider it would be well worth while trying these washers on the older sets not drilled for and equipped with coupling wrenches and which have shown a tendency to seize. It would be far cheaper than having them drilled and wrenches made.

SNOW-COURSE MARKERS

Ralph S. Parshall

(Letter of December 28, 1937, to W. W. McLaughlin, Chief of the Division of Irrigation, United States Bureau of Agricultural Engineering)

I am sending, under separate cover, two samples of Masonite cut from one of the snow-course marker-signs. These pieces are sent for inspection. One of these, as labeled, was submerged in water for 30 days and the other, a normal piece, was left dry. At the end of this period it was found that the thickness of the saturated piece had increased approximately 1/16 inch, but it may be found at the time of later inspection that this section will have become practically normal in thickness again because of the loss of moisture.

My conclusion is that the Masonite board, if painted with an enamel base and with a liberal coat of linseed oil on the reverse side, should provide ample protection for such a sign over a long period of years. I believe that under ordinary exposure these Masonite marker-signs should last more than 20 years.

(Note by J. E. Church--The value of Masonite markers is in their initial cheapness and portability and in the immunity of the signs to chipping from rifle-bullets or other impacts. The strips under test were only two by six inches in size. Would it not be well to test Masonite boards of the full size of one foot square or more to simulate field-use? The board could thus be tested for warping. Tests should be made from water to air to simulate wetting and freezing followed by drying.)

U. S. Bureau of Agricultural Engineering,
Colorado Agricultural College,
Fort Collins, Colorado

THE STEVENS SEASONAL SNOW-RAIN RECORDER

Lyman E. Rinker

Introduction--(At the second Western Interstate Snow-Survey Conference in 1934 [Trans. Amer. Geophys. Union, 15th Annual Meeting, pp. 632-633, 1934], a brief note was presented of the Stevens snow-and-rain recorder. The actual recording of winter precipitation on Mount Hood for the month January 14 to February 13, 1938, is sufficiently promising to merit further notice at this time, although the record was made after the close of the Davis Conference.)

Long-time records of cumulative precipitation in any form--snow, rain, sleet--are obtainable with this recorder. From the graph of cumulative precipitation the intensity for any period is readily determined.

Its salient features are: Continuous long-time record with one setting--a year if necessary; hermetically sealed; gravity-beam balance; friction reduced to a minimum; sensitive to 0.05 inch of precipitation; capacities up to 100 inches of equivalent water-precipitation.

General description--Precipitation passes through a standard eight-inch collector at the top of the housing, through an inverted cone-shaped conductor into a tank supported on a weighing mechanism. The weighing mechanism has been specially designed to eliminate friction--levers are suspended by flexible cables, thus avoiding any sliding parts or knife-edges. The balancing weight is positioned by a tiny battery-driven motor which automatically weighs the contents of the tank. The recorder-stylus is geared to the same motor so that it moves with every increment of weight, registering its motion in terms of inches of equivalent cumulative precipitation to any desired scale.

The chart is ten inches wide and the stylus automatically reverses at each margin, thereby permitting a large range at a very legible scale. The chart is sealed against dust, bugs, and changes of humidity. The graph is made by a brass stylus on a "metallic" paper chart that is legible, indelible, and never changes in character because there are no pencils to get dull or ink to fade out. The graph of cumulative precipitation is made on a Stevens Type A35 weight-driven recorder that will run for a year with one setting.

The entire mechanism is contained within a weather-proof metal housing with the door sealed by a rubber gasket. The recorder is to be mounted on a platform so as always to be above the deepest snow-drift. A 12-inch metal pipe closed at the lower end extends downward from the base of the housing to accommodate the driving weight of the clock and contain the battery deeply buried in the snow where it can not freeze.

In order to prevent snow sticking to the sides of the collector and reducing the standard area, a motor-driven swipe with knife-edge scrapes the sides of the collector. The power is supplied by a wind-generator which is installed at any convenient distance from the recorder.

The recorder itself is best installed in a clearing in the timber to break the force of the wind. Where such is not possible, it is permissible to use one of the new types of wind-shields with which the Weather Bureau has recently been experimenting.

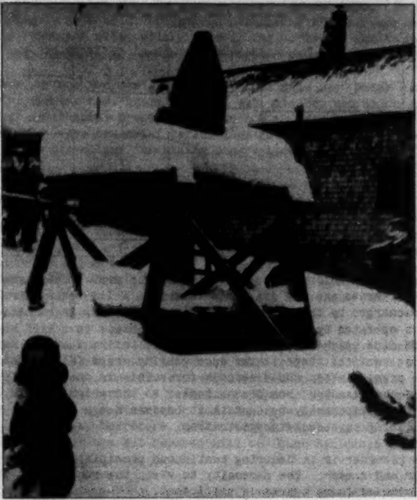


FIG. 1—MT. HOOD, FEBRUARY 13, 1938, SNOW-DEPTH ON GROUND ABOUT 6 FEET; PLATFORM 16 FEET ABOVE GROUND; RECORDER REGISTERED 9.4 INCHES OF CUMULATIVE PRECIPITATION BETWEEN JANUARY 14 AND FEBRUARY 13 (FROM GRAPH IT APPEARS MAXIMUM AVERAGE RATES WERE 0.145 INCH PER HOUR DURING 9^h45^m TO 15^h15^m, JANUARY 18, 0.167 INCH PER HOUR DURING 12^h TO 24^h, JANUARY 21, 1.9 INCHES PER DAY FOR 2 DAYS DURING NOON JANUARY 20 TO NOON JANUARY 22)

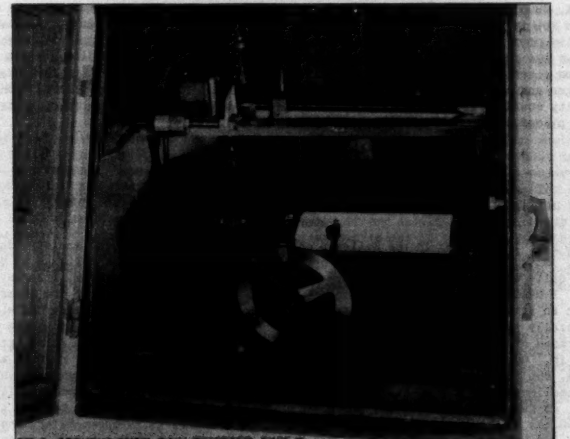


FIG. 2—STEVENS SNOW-RAIN RECORDER TYPE W

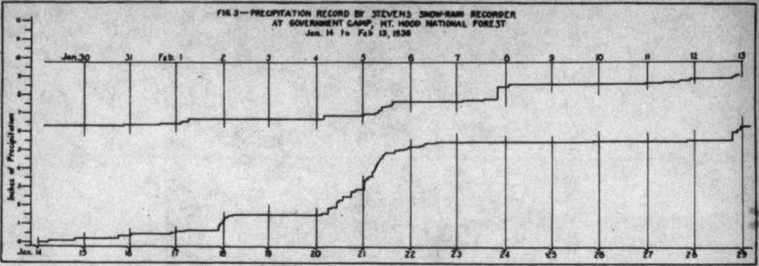


FIG.3—PRECIPITATION RECORD BY STEVENS SNOW-RAIN RECORDER
AT GOVERNMENT CAMP, MT. HOOD NATIONAL FOREST
Jan. 14 to Feb 13, 1938

The recorder positioning-motor receives its power from dry- or storage-batteries in a suitable container. Dry-batteries are now available that have a life of a year. If storage-battery is preferred, it is recharged by the wind-generator. The battery is connected to the weighing motor by a time-switch operated by the clock that makes contact each half hour. The graph therefore is a stepped line, each step showing the precipitation that fell during the preceding half hour. Some of the vertical steps in the accompanying graph (Fig. 3) are longer than the preceding half-hour's precipitation could account for. This is due to wet snow sticking to the inside of the inverted cone leading from the collector to the weighing tank. Several hours' precipitation may thus be temporarily held until it becomes heavy enough to drop off, the total accumulation thus appearing in the next registration.

The value of the recorder is in securing continuous precipitation-data in isolated localities throughout winter and summer. The necessity to visit the recorder at stated intervals is entirely eliminated--once or twice a year is sufficient.

Leupold, Volpel and Company,
Portland, Oregon

DISCUSSION

J. E. CHURCH (University of Nevada, Reno, Nevada)--The tendency of batteries to freeze under continued low temperatures even no lower than -10° F, as experienced on Mount Rose, and the susceptibility of wind-vanes to become encrusted with frost feathers particularly in exposed situations make the present effort problematical but all the more worth while. Could not this type of gage be given prolonged use in connection with the present series of high-level tests of catch-gages and snow-surveys being conducted by the United States Weather Bureau? Thus its dependability could be determined. If weights can not be substituted for batteries, the latter could be placed below the frost-line of the rock or soil.

WINTER SPORTS

ON THE RELATION OF WEATHER- AND SNOW-REPORTS TO WINTER SPORTS

Robert G. Stone

Last year I reprinted in the Bulletin of the American Meteorological Society [January and February, 1937] a classification of the forms of snow and ice by Gerald Seligman of Great Britain; this was quite detailed and seems to have attracted a good deal of practical interest in this country. I believe many of us were somewhat unaware of the complexities of the forms of snow and surprised to find that so much work had been done on them in Europe. In so far as winter sports and especially skiing are concerned, the need for a greater popular working knowledge of the different grades of snow was felt in New England some years ago when the sport first became widely indulged in. Our skiers began importing terms they had picked up in Alpine resorts and applying them here, but these were inevitably rather misunderstood or misapplied by our amateurs, so that the original meanings of some of the terms were lost and quite different ones substituted. Fortunately, so far, only several such conversions are deeply intrenched here, but the trend is evident unless skiers become better educated about snow-craft, as European skiers know it. Park Carpenter, an enthusiastic Boston skier and editor of The Ski Bulletin, in 1932 or so, first mimeographed a classification with descriptions of snow-surface types for skiing which he obviously obtained for the most part from European sources such as Lunn. It was intended for observers who were sending in reports of the skiing conditions from certain resorts. Later he published some revisions which are still widely used in the northeast and to some extent elsewhere for reports sent to The Ski Bulletin (Boston) and perhaps by others.

The kinds of snow-surface are designated therein as follows: Under heading of "Soft-snow conditions" as fluffy, powder, new, heavy, sticky, wet, glazed, and slush; under heading of "Hard-snow conditions" as (crusts) breakable, wind, common, and icy; and under heading of "Spring conditions" as granular. There are brief definitions or descriptions of what is to be understood by these terms, and a few directions for making observations of temperature, depth of snow, and for reporting the data.

Now this probably seems to the skier to serve the present needs fairly well. There is nothing particularly scientific about it, though it does little rank violence to science; however, the term "glazed" is poor since it may confuse between true glaze, sleet, sun-crust, rain-crust, or incipient frozen granular, which are quite different, though all impracticable for skiing, but the description given for "glazed" indicates it refers only to sun-crust, as it is known in Europe. Also "common crust" is out of line with the other terms since it is neither accurately descriptive nor in the vernacular. "Heavy" is too vague and overlaps with "wet", "sticky", the crusts when incipient, etc.

Since our eastern skiers have to, or prefer to, ski principally on narrow forest-trails which soon become hard-packed, if not icy, and they have relatively little opportunity to ski under open-slope and alpine conditions, the above classification would naturally suffice and may even be unnecessary. But the present trend is toward more open-slope skiing and ski-touring, and there are many ski-schools springing up which teach the technique needed for truly alpine snow-conditions. I venture to forecast, therefore, that a better knowledge of the kinds of snow and a more refined classification for the ski-reports will be found desirable in this part of the country.

In the West, true alpine snow-conditions prevail and the same knowledge of snow-craft found necessary in the Alps will sooner or later take root. I believe that it is not too early to begin thinking about the possibilities of more rational and detailed study of snow for winter sports in this country. In the Alps, scientists, Governments, and the mountain clubs have consistently advocated and fostered the education of skiers and climbers in matters of rational snow-craft, as a means to protection of life and greater enjoyment of sport.

Now the specific question as to what classification of the forms of snow-surface should be used this or next winter by the snow-surveys and other agencies interested in giving reports for winter sports to the public has already been precipitated by the appearance in print of the classification by Seligman which I mentioned first. [See also his chapter on snow in "Skiing, the international sport", edited by Roland Palmedo and published by the Derrydale Press, New York, N. Y., in limited editions.] Last autumn Mr. Mitchelson even invited me to formulate a classification for use in California, a task which I did not feel able to undertake, but which stirred up some correspondence in which you may be interested.

In reply to Mr. Mitchelson I wrote as follows: "Frankly, we are unprepared to meet your problem very definitely because, on the one hand, science is still not quite sure what the different kinds of snow mean to the skier, and on the other hand, the skiers and snow-observers need to be educated as to the kinds of snow as far as they are known to concern skiing. The especial difficulty in this case is that snow-conditions are somewhat different in the West than here and we have only local experience (Appalachians); and I doubt if one should indiscriminately borrow terms used elsewhere without giving the matter some research to see if they really apply well. In this country we are only just arriving at the stage where we are getting people interested in observing and studying the character of snow in various places. I should not want to suggest the use of certain terminology and concepts in California, only later to have it turn out they were unwise.

"Unless the snow-reports for skiers are transmitted and broadcast in the space of a couple of hours or less, there is no sense in reporting detailed descriptions of the snow-conditions, because they are very apt to change as to details every few hours. There are characteristic changes that go with the course of the day, due to the warmth of sunshine and chill of night, and in general, I think these should not be reported as of a given hour because it will always be different by the time the skiers get there. Skiers will have to learn what the diurnal behavior of the different primary types of snow is and judge it for themselves from the more general descriptions. A good weather-forecast issued with the snow-reports makes it possible for a snow-wise skier to forecast what will happen to the existing snow by the time he gets there, if the time-interval be not too long. Perhaps, some one should issue a pamphlet describing principles of snow and weather for skiers, which will help or indicate how to interpret the reports and forecasts. Such information could be lectured over the radio too, which some commercial radio advertiser might want to sponsor.

"It is also customary in the East to say only whether the skiing is good, fair, poor, etc., without giving any reason except the depth of snow-cover and some adjective such as slushy, hard crust, powder, etc. Now, different observers and skiers have different ideas of what is good or poor skiing, depending on their experience or training, as well as temperament. Often what looks like good skiing in these parts would be rated decidedly poor by many Western skiers used to more alpine conditions. The possibility of standardizing the ratings is a knotty problem, and perhaps if science were able to do it, commercial interests would sabotage it, for obvious reasons. Again, the tendency here is to report ski-conditions only with respect to limited areas covered by popular trails and practice slopes at certain resorts, which may be sufficient; but, as many skiers here have learned, such reports may be very misleading as to the general distribution of skiing-conditions over a large area, such as a whole State. Without a fairly dense and extensive network of reporting observers, it is hard to obviate this difficulty materially. Perhaps it is of more consequence in the East than in the West.

"How many and what kind and distribution of observers will you have for supplying snow-reports for skiing? How quickly do you expect to be able to assemble and broadcast the reports? Those are just some of the things to be considered."

In writing to Dr. J. E. Church on the same general subject, as well as referring to Mr. Mitchelson's request, I went into more detail as follows: "I am aware that the Western mountains have characteristically somewhat different types of snow-surfaces, as far as skiers are concerned, from the ones we have here. Eastern skiers who have been to the West say that they have a much greater frequency of "heavy" snow-surfaces than here and that it is generally a little harder to make turns in it. Now this may be true only for the spring and late winter snow when the Sun has been working on it quite a bit. We also get the same thing here, of course, in spring. But our skiers do not notice it so much because they are accustomed to ski on trails that are well packed and even icy by spring; whereas, in the West, they go all over the open alpine slopes ever softened by the Sun. It is no wonder, therefore, that the prevailing conception of what constitutes "good skiing" conditions varies from section to section according to the prevailing natural conditions. A Westerner would turn up his nose at what we call "good" in this climate of frequent open winters. This fact makes me wonder if it will ever be possible to get reports of snow-conditions for skiing rated on an absolute scale for national or regional broadcasting, and I am not sure that it would be desirable. Presumably, reports giving an accurate technical description of the snow-surface would be absolutely comparable from one section to another, but I doubt that amateur observers can ever be taught to make such reports correctly and I do not think the majority of skiers will want to bother with them either. Then if we must fall back on such general reports as to whether the snow or skiing is "good", "fair", "poor", etc., as they are now doing here in New England and New York, we must probably resign ourselves to a great deal of subjective and sometimes wilfully misleading reporting.

"Another difficulty is the lack of understanding among even fairly good skiers as to the different major kinds of snow-surface and their skiing qualities. The skiers, as well as the observers, need education in this respect. What few terms the skiers already commonly use to distinguish snow-conditions are sometimes rather unfortunate. Thus they call the new firn snow of spring "corn-snow", a term which on the Continent and in other countries is used for quite a different phenomenon. Furthermore, they call various stages of spring snow having different ski-qualities all "corn". So you see the kind of thing we are up against.

"Finally, unless the snow-reports for skiers can be collected and broadcast very quickly after they were observed, it is rather foolish to give a description of the snow-surface in any technical or detailed terms, because a few hours or a day may change the snow-conditions radically and the skiers will find something quite different by the time they get to the snow-fields. In New England, New York, and Pennsylvania, reports are made Thursday morning and appear in The Ski Bulletin and newspapers by Friday or Saturday, though radio broadcasts in some cases reach the public more promptly, of course.

"But the skiers do not reach the snow-resorts until Saturday or early Sunday. In the meantime a fresh snowfall or a bad thaw often has begun. Under such conditions the present policy of giving only the depth of the snow and the quality of skiing and condition of the trails in very general terms is about all that can be expected. If forecasts of the weather for the time when the skiers will be on the snow-grounds can be made accurately enough, a skier of experience and some understanding of snow-craft can foretell for himself what the condition of the snow-surface will probably be where he intends to go. In view of the marked contrasts between shady and sunny slopes and between night and daytime, as well as other local and regional topographic and climatic differences, it would be manifestly impossible for a central office to issue reports and forecasts that would tell the skier just where, when, and what he will find a day or two hence, and rather impracticable to attempt to even describe the existing conditions in such detail. If these local and diurnal variations are very great, as I believe they are sometimes and in some sections, then it is just a question what can feasibly be done for the skiers. Last winter (1936-37) was very skimpy of snow in this part of the country and skiers were simply wild trying to find out where they might find skiing. The reports were totally inadequate and misleading because they were taken only at a few resort centers, and then usually at some man's house in the valley where the snow was much shallower than on the higher slopes. The amounts of snow on the trails were often reported, too, but they were not always good because there was such a big variation from one part of the trail to another and so much change from Friday to Saturday or Sunday. When reports suggested there was no skiing south of Mount Washington, many skiers were surprised to find deep snow in the woods on north slopes 50 miles south and at much lower elevation where the fields and south slopes were nearly or all bare. Before the Christmas vacations there was too little or no snow for trail-skiing but everyone had to make reservations for quarters in the mountains well in advance. They had no idea whether there would be snow or where was the best chance of its coming. A little snow came but melted off in a few days and the skiers were disappointed by the thousands. Strangely enough, Mount Mansfield, Vermont, turned out to be the only place with much good snow, whereas we would have wagered on Mount Washington and would have lost! Despite the considerable snow they get in northern New England for two months or so in the average year, conditions over a large part of the region remain rather critical for skiing almost the whole winter. Our National Park Service study (supra, pp. 486-492) shows that hardly any place has more than a 50-50 chance of enough snow for skiing--even at the height of the snow-cover season; but, when the individual skier considers the wide area over which he may seek snow at any one date, his chances of getting it are increased to perhaps 80 per cent in midwinter, since it rarely occurs that all places are then deficient in snow. Yet to exercise this choice of the places with the best snow, the skier is often forced to go much further from home and to spend much more money and time than he can afford or wishes. In the West the situation is not nearly so critical as here, though skiers have to travel further on the average to reach snow, and I suspect one would have to go to the Laurentians in Eastern Canada to approach the stability of Western snow-conditions. Northeastern skiers might just as well make up their minds to the bitter fact that even with snow-reports and forecasts they are going to be disappointed about half the time in the long run, and those who depend on them for a living likewise."

Bester Robinson, who is the representative for Western winter sports on the Committee on Snow of the American Geophysical Union, was furnished a copy of the above letter and made the following comments in a letter to me: "In the first place, I do not believe in the desirability of uniformity in recording snow-conditions. The skiers in each section of the country tend to build up a verbiage of their own with regard to snow-conditions, based on the needs which they meet. You in the East, for instance, seem to be very much interested in the condition of trails, whether they are hard-packed, etc. In our Sierra Nevadas, however, we have two principal concerns: First, whether there is an adequate depth of snow to cover rocks and brush; second, whether the new fall of snow has packed sufficiently for cross-country touring. With us, if these two conditions are satisfactory, we know that we will have satisfactory skiing in all of the higher portions of the Sierra Nevadas and auxiliary ranges.

"With regard to weather, our problems undoubtedly are different. Skiing with us is typically a week-end sport. Skiers usually leave principal population-centers Friday night and return Sunday night. Our storms in the Sierras are almost invariably general storms covering either the entire range or a very large section of it, as well as the entire area between the coast and the top of the range. The general meteorological predictions, based largely upon Pacific Ocean observations, are reasonably accurate for two or three days in advance. It is the concensus of opinion among California skiers that on Friday the local Weather Bureau can give a prediction as to conditions in the Sierras, which stands a five-to-one chance of being accurate. I imagine that our local Weather Bureau receives far more telephone calls from skiers than from any other source during the winter season.

"The fact that I do not believe in either the standardization of snow-recording for sport-purposes or in attempting to make the character of recording too technical, does not mean that the skiers should not have whatever information would be of value to them in determining whether or where to go skiing.

"As you have pointed out, attempts to characterize the specific surface-condition are hope-less, because of the differences which would be met on varying exposures and during different parts of the day. Our typical condition in the High Sierras during winter will be frozen snow-crust on southern exposures early in the morning, which will change to wet snow before noon. At the same time, on northern exposures we will have powdered snow, and the tops of the ridges will be covered with wind-crust. There seems to be no sense in going into these infinite variations. A competent skier, if he knows the depth of the snow and the time when the last snow-storm occurred (or the general character of snow on the level), will be in a position to determine conditions which he must anticipate.

"Depth of snow on the level, determined at snow-courses laid out in accordance with the specifications of the Weather Bureau, and also the general character of snow on open level areas, that is, whether it is soft "corn", packed "corn" or powder, or new wet snow, would entirely satisfy the requirements of California skiers. . ."

Lest my letters and that of Mr. Robinson give the impression that there is not much interest nor faith in the science of snow on the part of skiers, I should add that we all feel that a greater knowledge of the physics of snow will be of immense utility in many practical ways, to skiing as well as other matters. It is perhaps too much to expect that skiers turn amateur scientists, as they sometimes do in Europe, and answer the questions for us. But it would be well to encourage any scientifically-minded skier to make a careful study of the friction of different snows on skis and waxes and of the relation of density and structure of the snow to the amount of speed and torque necessary to turn the skis. The question of the classification of snow-forms and their origin is probably better left to serious physicists, but any rough classification used by skiers should at least be scientific to the extent that it recognizes the well-known evolution of snow from a falling flake to eventual spring snow or firm ice, the diurnal effects of Sun and shade, and the ablational modifications from wind, Sun, rain, dust, temperature, and ground. The terms should be descriptive in themselves of the gross appearance and structure of the snow or ice, the genetic relations when known being indicated by the grouping of the terms or by prefixed adjectives. Vernacular terms should be retained only when they fall in line with accurate description and genetic considerations. I am attempting to make some concrete suggestions along that line, which may be ready for publication at a later date, but the various modified forms of snow and ice due to ablational factors will be very difficult to distinguish adequately without a complicated terminology. A multiplicity of terms in a classification need not be looked upon as a drawback in itself, since in practice very few of them are needed in any one section and time of the season, but the classification must at least cover all that are likely to occur in one region during any winter, which includes more than many people suspect.

In closing I would like to mention the value of climatic studies of snow to the winter-sports interests. The National Park Service, as part of its program of technical studies for recreational development carried out under its supervision with Civilian Conservation Corps funds and labor, last year pursued a compilation and summary of the depth-of-snow-on-ground observations made at cooperative stations of the United States Weather Bureau in New York and New England. The results are being prepared by me for publication, but since the Park Service will probably not be able to issue the report for some months yet, the plan of this study has been described in another article in this volume of Transactions (pp. 486-492).

(The creation of National Advisory Committees on Camping, Hiking, and Skiing has been announced by the Secretary of the Interior to assist the National Park Service in formulating policies and practices in connection with these activities.)

Blue Hill Observatory,
Harvard University,
Milton, Massachusetts

CLASSIFICATION OF SNOW-SURFACES

Henry I. Baldwin
(Communicated by letter, December 31, 1937)

I have been trying to look up material for a statement on classification of snow-surfaces to be sent for the meeting at Davis, but have come to the conclusion that I can add nothing to what Mr. Stone has already written. There is enclosed, however, a classification used in reporting the conditions for The Ski Bulletin which has been in use here for about eight years. I agree with Mr. Stone that this is not as scientific a classification as for instance Seligman's, and also that the people who do the reporting rarely are accurate in their estimates of the sur-

FIG. 1—TUCKERMAN RAVINE, MOUNT WASHINGTON, MAXIMUM GRADE ABOUT 50°, SKI-SEASON MARCH TO JULY (MAXIMUM CROWD IN ONE DAY 2000)
PHOTO BY VICTOR BEAUDOIN, BERLIN, N.H.

face-conditions. Here in the East conditions on the surface change practically from hour to hour and are greatly influenced by air-temperature, wind, and Sun. Small obstacles and differences in exposure affect the snow and there is reason to believe that the character of the ground-surface beneath may be quite important. For instance, places where brush has been burned on ski-trails are known as "hot spots" and many people are thoroughly convinced that lye from the wood-ashes becomes dissolved and works upward in the snow. I know of no experimental proof of this and am inclined to think the absorption of solar heat by the charcoal causes the difference. We also have the phenomenon of slush on roads caused by using calcium chloride to keep sand which is spread on the highways from freezing.

A really sound classification of snow-surfaces would have to be worked out by a committee representing the winter-sport, highway, and water-power interests in cooperation with physicists and meteorologists. I do not think I am competent to comment further. I would have been glad to have conferred with Mr. Stone in person, but he was leaving for a couple of weeks' absence at this time, and I really think he has covered the situation regarding snow-reports very well in his communications.

I selected a number of snow-scenes from the beautiful collection of the State Planning and Development Commission (Donald D. Tuttle, Publicity Director). These were taken by professional photographers. Only one showing snow-surface conditions (Fig. 1) has been selected for reproduction from his large collection which includes a great many devoted to winter sports and winter activities.

The new and revised Carpenter snow-classification

(This classification is based chiefly on terms used by Lunn in his "Alpine skiing at all heights and seasons." The more scientifically acceptable of these were adopted by Seligman in his book "Snow structure and ski fields" [see Bull. Amer. Met. Soc., pp. 48-53, February 1937]. For a criticism of the terms and definitions as given by Mr. Carpenter, see article by R. G. Stone "On the relation of weather- and snow-reports to winter sports" [this volume of Transactions, pp. 720-724]. Mr. Stone is preparing a revised classification for winter-sports use which will attempt to combine the genetic relationships and vernacular terminology. The descriptions followed by a plus (+) sign are revisions of those as defined in The Ski Bulletin, 27 Beach Street, Boston, Massachusetts.)

Stations where there is less than four inches of snow send no report.

Observers are requested to measure snow-depth regularly at the same fixed point in some level lot or field unaffected by drifts. If for any reason measurement is taken at any other point, indication of such other point should precede the report.

The terms descriptive of snow-surface employed in these reports are for the most part adopted from Arnold Lunn's "Alpine skiing at all heights and seasons."

The terms used in describing soft, dry snows are:

(1) Fluffy--large, light, feathery flakes which may be blown about like feathers.
(2) Powder--cold, dry, powdery snow.
(3) New--snow which has fallen during the 24 hours preceding the observation.
(4) Heavy--powder-snow so toughened by wind or freezing that, while skis sink in it readily, there is nevertheless a definite resistance to any lateral movement.

The terms used in describing soft, wet snows are:

(1) Sticky--cold, powder-snow, in process of being warmed by air-temperature and just beginning to thaw.
(2) Wet⁺--soggy, "cotton-batting" snow, ideal for making snowballs, forts, and men.
(3) Slush⁺--more water than snow.

Crust-conditions are described as:

(1) Glazed⁺--a light, thin, transparent glaze, which frequently forms over any kind of soft snow but does not materially hinder lateral movement of the skis.
(2) Breakable⁺--any type of crust other than glaze, which will not hold the full weight of a man on a single ski.
(3) Wind-crust⁺--by the action of wind and frost dry-formed from powder-snow; usually ripple-marked.
(4) Common crust--formed by a light thaw and refreezing.
(5) Icy crust--formed by the freezing of a watery surface after a heavy thaw.

Spring snow-conditions are described as:

(1) Granular--an old, wet snow, which has "melted and refrozen night after night" until it has acquired "a crystalline character."
(2) Frozen granular (new term)⁺--granular snow which by a fall in temperature has been frozen into a hard, ice-like base.

New Hampshire Forestry and Recreation Department,
Hillsboro, New Hampshire

SOME OBSERVATIONS ON SNOW-REPORTS IN NEW ENGLAND

Henry I. Baldwin

Snow-reporting in New England is used in this paper to refer to reports of snow-conditions for winter sports primarily, although such reports are also of service to highway engineers, long-distance shippers, and power-companies. Stream-flow and even floods are less dependent on snow-accumulations than on occasional torrential rains, and differ thus from Western conditions. Since snow-depth, its quality, and distribution vary extremely from place to place in New England, as well as from day to day and hour to hour, many agencies such as railroads which run "snow-trains" have been receiving and publishing snow-reports for several years. These reports are carried by most metropolitan and local daily papers, as well as by special publications like The Ski Bulletin published every Friday in Boston.

In discussing this subject I would approach it first from the viewpoint of a reporter of snow-conditions and secondly from that of a user of these reports. The pioneer effort in the East has been The Ski Bulletin, founded and edited by Park Carpenter, an enthusiastic ski-runner of Boston, and credit is due him for developing the system of snow-reporting described below. I have cooperated with The Ski Bulletin since the beginning of its snow-reports, first at Berlin, New Hampshire, and after 1932 at Hillsboro, New Hampshire, in the southern part of the State. According to instructions, I telegraph in the snow-depth, temperature, and a one-word description of the surface each Thursday morning before 9 a.m. For example, "12 powder 5 plus 20" means a total depth of 12 inches at the stake, of which five inches is a powder-surface, and the remainder an old unbreakable crust; "plus 20" is the air-temperature, generally obtained when setting the maximum thermometer at the cooperative Weather Bureau station maintained here at the Fox Research Forest. Now there are certain objections to this system inherent in the method of measurement in addition to the time of reporting, which are often misleading. Let us consider snow-depth alone.

Snow-depth--The influence on depth of exposure, cover, and general location of the point of measurement is very great. The first winter I spent at Hillsboro the ground was often bare on one side of the house while drifts were nine feet high on the other. So a snow-measurement station was selected in an opening in pine-woods, on level ground about a mile from the buildings and 200 feet lower. There was no evidence of drifting here, and snow-reports for The Ski Bulletin have been based on a snow-stake at this station ever since. Since most New England skiing has so far been on wooded trails, it seems logical that the place of snow-measurement should be in such surroundings. After a few years, it became apparent, however, that in relatively snowless winters, snow lay longer in this protected spot than on the wind-swept hills, or on the more or less exposed ski-trails in this section. Frequently the general countryside was almost bare when a foot of snow was registered on this stake, and only patches were to be found at the Weather Bureau station stake. Now the only unique skiing opportunities in this region are high open pastures, usually wind-swept. Obviously it would be misleading to report 12-inch snow present when these were bare. Therefore, no report is sent when conditions do not justify it. The only solution would appear to be a description of average conditions (a) on open hillsides and (b) on sheltered woods trails. Here again very considerable differences will be found on different exposures, degrees of slope, and type of cover. A detailed description would be costly to telegraph in the case of a skiing resort of minor importance such as this. At really prominent skiing centers, snow-depth and quality are reported at several different altitudes when conditions vary sufficiently to warrant this. Paired snow-stakes in and out of the forest have been observed for two years in several locations. There is frequently greater variation between two "open" stations in different exposures than between the open and forest-stations, but during the early winter snow-depths are double to six times as great in the open as in the forest due to interception by and subsequent evaporation from the tree-crowns.

Measurement of snow-depth--Another variable factor in the snow-depth measurements is the technique of measurement. I am not familiar with the methods of measurement at all reporting stations, but I have seen some of the stakes, and suspect others have no special stakes, but the observers perhaps plunge a yard-stick in the snow at various points (usually not exactly the same points each week). We have been using here a wooden stake driven firmly in the ground below frost-line early in the fall, with the zero-point on the inch-scale even with the ground-surface before any snow falls. We always choose a point where the ground is perfectly level at least three to four feet on all sides of the stake. All bushes and high weeds in this area are cut, and a wire is strung around it about three feet high to keep animals and people from treading the snow down about the stake. The effects of the exposure, surrounding slopes, protection from wind, etc., and general topography have been mentioned, which with the absolute altitude are the chief factors determining how much snow will fall and accumulate around that stake. Of minor importance are ground-cover, and composition, shape, size, and color of the stake itself.

It is a common observation that snow lies longer on some objects than others; these are chiefly non-conductors of heat. Experiments have been made with white-painted pine-boards, pine-needles, etc., but with variable results. We now use the natural cover about each stake. It is possible that one variable could be eliminated if the same cover was found about all stakes.

After a thaw or long period of sunshine, a depression forms about the stake, and the stake must be read by sighting along the surrounding flat snow-surface, or laying a straight-edge on the snow. We have not done much work on the type of stake which would best avoid wind-eddies and heat-absorption, but we are now trying a broomstick painted with aluminum paint. The smaller the stake, the better, however, and we shall try one-half inch diameter florists' rattan sticks next year if the graduations can be made readable.

Snow-quality--We have not had much original thought on a scientific classification of snow in New England as a physicist or even a serious alpinist might attempt. Bentley at Jericho, Vermont, did not see the snow for the crystals. The opposite is true of most ski-runners. The winter-sports enthusiast, and ski-runners with scientific background have only a superficial knowledge of different kinds of snow. Since New England snow-environment differs somewhat from alpine conditions, and is not adequately worked out yet, the simpler and more practical the terms used in reporting snow-condition to inexperienced tourists the better. I do not believe that "good," "fair," "poor," etc., are sufficiently descriptive, however. These terms are used, I believe, by the Boston and Maine Railroad to describe conditions for skiing and other sports. I would prefer "hard," "soft," and "breakable crust"--a classification proposed by Caulfeild in 1914 in preference to the more detailed one of Arnold Lunn.

Without going into refinements and running the risk of mistaking (at this stage of the game) the special sub-species proposed by Seligman, would it not be better to use a broader classification since the snow will change slightly before the ski-runner reaches it? First of all, he is interested to know if there is a "base" of hard snow, or if he is liable to scratch through the loose snow onto rocks and bare ground. A stiff but breakable crust is a very unpleasant condition for skiing and should be reported, when present, otherwise assumed "unbreakable." Finally, does not "hard" or "soft" tell the story sufficiently? It ought not to take much experience to tell what conditions will be like, given the snow-depth, this information, and the weather-report. This brings us to the consumer's viewpoint.

Since moving to a point south of the main skiing centers in New Hampshire from a point north of them, I have had to reverse my estimates of what conditions may be like compared to those at my residence. I have learned, however, the relationship which generally prevails at places east and west as well as north and south of where I happen to reside, and make my preparations accordingly. I find this process quite natural and rely on it perhaps as much as on published reports. I think everyone consciously or unconsciously will do this. My case, it should be said, is not representative of the majority of week-end ski-runners. I get The Ski Bulletin Saturday noon, containing reports of conditions as of 9 a.m. Thursday, and, when skiing conditions are not sufficiently attractive locally, I may leave for other ski-country early Sunday morning. Therefore, it is quite necessary to qualify the reports of conditions in the light of weather-changes since the reports were made and from what I know has happened in the past. It is apparent that snow-depth is of chief importance and that surface-conditions are secondary. If depth has been grossly overestimated, as it not infrequently is, conditions may be most disappointing. The ski-runner has less cause to be disgruntled over changes in quality, which are beyond the control of the reporter.

Reports qualifying the snow-data have often been helpful. Also, if in the opinion of the reporter the conditions at the regular observation-point are misleading in that they present a picture which is (a) too optimistic, too rosy, The Ski Bulletin has welcomed comments for important stations such as "slopes bare" or "trails poor," etc., or (b) too pessimistic, in which case an additional (but not a substituted) report may be appended in order that good skiing at higher elevations may not be overlooked. Of course, there is a temptation for a snow-reporter whose livelihood depends upon a "capacity crowd" of ski-runners at his hotel, restaurant, or ski-school to err on the side of optimism.

As for reporting snow-conditions for the whole country, I see little need or possibility of it. Snow west of the Mississippi should be reported by and for people in the West. Western snow-depth data a week old ought to be adequate for Easterners, even for those who are flying out to ski, since the depth is so fabulous. Similarly it can do Westerners little good to know detailed conditions in the East.

In conclusion, I think the most important need in snow-reporting is accuracy in describing depth on the places where depth is a critical factor in skiing. It makes less difference whether there be 2.5 or 3.0 feet on a trail where six inches are sufficient than it does to report 14 inches on a stake somewhere when part of the skiing terrain is bare or cover dangerously thin. Snow-craft is part of the equipment of the experienced ski-runner, just as wind and weather are of the amateur yachtsman. Ski-runners must learn to read their snow as the sailor does his water. Ready-made forecasts will never entirely replace independent judgment of such changeable phenomena, and after all it is part of the game.

New Hampshire Forestry and Recreation Department,
Hillsboro, New Hampshire

SNOW-COVER CONDITIONS FOR WINTER SPORTS IN THE WESTERN STATES

Paul A. Ewing

For several years the Bureau of Agricultural Engineering has been engaged in wide-spread studies of the snow-cover for the purpose of forecasting spring and summer runoff. This service has been primarily beneficial to farmers. Now it has been found that the snow-surveys can readily be extended to benefit the snow-sportsmen, whose numbers and enthusiasm are increasing so remarkably throughout the West. (W. W. McLaughlin says: "It is estimated that about 2,000,000 people take advantage of this sport in this country. With these broadcast efforts of ours we are probably reaching every interested person in the Western part of the United States--the Western one-third.")

The form taken by this extended activity is a series of radio broadcasts addressed to winter sportsmen. At present, four broadcasts are being made weekly: One from Denver for Rocky Mountain areas; one from Salt Lake City for Utah; one from Boise for other Intermountain areas; and one from San Francisco for the Pacific Coast States. In addition to the latter program, a local broadcast for Oregon is made from Medford. As I am most familiar with the San Francisco program, it is the one I propose to describe.

These programs go on the air Friday evenings at 6:15 o'clock. The Bureau's contributions consist essentially of telegraphic reports received by it during the day of the broadcast, describing conditions at most of the winter-sports areas in California, Oregon, and Washington where skiing, tobogganing, bob-sledding, skating, and other recreations are enjoyed. These telegrams are sent us, at our expense, by the Pacific Coast National Parks and Forests, the Calaveras

Big Trees State Park, Los Angeles County Recreation Camps and Playgrounds, the Los Angeles Department of Water and Power, and a few well-known resorts outside the jurisdiction of these agencies.

Contributing also most importantly is the San Francisco District Forecast Center of the Weather Bureau, which supplies us Friday morning with a résumé of the previous week's weather-developments up and down the Coast, giving especial attention to their effects upon the snow-cover; and, for the conclusion of the program, a forecast for the week-end, released only a few minutes before the broadcast and also addressed especially to winter sportsmen.

Following a few sentences of explanatory introduction, the routine of the programs is to read the résumé of the past week's weather, and then to announce three items of information for each sports-area: First, the condition of the roads leading to the area; second, character and depth of the snow on the course or toboggan; third, weather-conditions, including temperature as of about 10 a.m. Concluding each program is the forecast of the weather for the week-end. This weather-forecast refers to the four principal Pacific Coast sections--that is, Washington, Oregon, northern California, and southern California. Unless otherwise specified by reason of the localization of storms, northern California is understood to include Sequoia National Park, and southern California includes Owens Valley.

Finally, if time is available, the program includes announcements of tournaments, ski-races, or other special winter-sports events.

While the order of these recitals is intentionally quite rigid so that sportsmen listening for announcements regarding their favorite courses will know when to expect them, an effort is made to keep them lively and informal. Nevertheless, it is difficult to compress all the information into ten minutes. In order to effect conciseness, therefore, and to make the information understandable without too much repetition, we adopted a set of terms descriptive of snow which would have meaning to sportsmen. We solicited suggestions for such terms before the broadcast series began.

In the search for these suggestions, and for other help, I called upon Dr. Church late in the summer, and thereupon paved the way for the trouble I am in at this moment. Dr. Church was curious to know what character our responses would take; he suggested a symposium to be made of them, and thought that this meeting would be interested too.

Although too few to be dignified by the term "symposium," these responses were of undeniable interest, and from them we obtained the nomenclature now being used in the broadcasts. Let me run through them rapidly for you.

Jerry Carpenter of the Winter-Sports Committee, California State Chamber of Commerce, thought sportsmen are not much interested in the character of the snow--it is depth they care about.

Mr. Marr wrote: "It appears to me that we might rely upon 'poor,' 'fair,' or 'excellent' as sufficiently descriptive terms when used by our cooperators, who presumably will be authorities on winter-sports conditions. It would appear advisable to supplement such description by giving depth of snow, the moisture-condition of the surface-snow such as 'dry' or 'wet,' and condition of the surface-snow as to whether it is 'firm,' 'loose,' or 'crusted.'"

Mr. Work submitted a selection of terms from Seligman, as follows:

New snow: Snow immediately after it has fallen and while it still possesses a fluffy, feathery, or floury nature due to its not having changed materially from the flake condition.

Sand snow (a condition I have never seen): Snow of a type which has fallen at extremely low temperature so that neither ski nor sledge will glide in it.

Wild snow: A collateral form of new snow which has fallen in complete calm at low temperature and is immensely unstable.

Settling snow: Snow intermediate between new snow and settled snow.

Settled snow: Snow which has settled into a close-lying powdery form--the good powder-snow of the skier.

Old snow: Snow which has passed beyond the settled or loose-lying powdery stage. It includes the various types of firm snow, sun-crust, rain-crust, and wind-packed snow.

Firm snow: Snow which has become granular and compacted (high density).

Sun-crust: Any sort of snow which has been superficially melted by heat and refrozen into a crust, etc.

Rain-crust: Rain-crust is formed by the snow-surface becoming wetted by rain and subsequently freezing.

Breakable crust: Denotes any kind of crust which breaks under the weight of the ski.

Telemark-snow: Any hard snow or crust thick enough not to be breakable, but sufficiently softened on top to permit of turns being executed with the ski.

Spring snow: Somewhat drier snow than telemark-snow, but difference not very marked.

Wet snow: Any type of snow which has become altered by wind, rain, or damp air so as to become high in density. Usually sticky or not suitable for fast skiing.

Mr. Jessup's suggested terms are those in use by the Washington Ski Club, as follows:

Crusty: Implies lack of fresh snow and indicates a frozen surface due to melting during the day and freezing at night.

Powdery (or fluffy): Fluffy dry snow--a choice condition.

Cloggy: Wet heavy snow.

Breakable crust: Not as hard as "crusty"--liable to break through in places and very dangerous.

Mr. Parshall suggested, and in the Denver broadcasts is now using, the following terms:

Dry: A light fluffy snow of low density, easily moved by wind--may be referred to as "powder."

Heavy: Wet, high density, will not crumble when shoveled or disturbed.

Crusted: Conditions where freezing has formed a top crust.

Packed: Condition of crusting due to wind-driven snow; the report should mention whether the crust is firm or breakable under the weight of the skier.

Drifted: Snow blown into drifts, irregular with firm surface.

Granular: A condition usually found in old snow during the spring, sometimes referred to as "corn-snow."

Lawrence C. Merriam, Acting Superintendent of Yosemite National Park, reported the expressions commonly used in the Park, as follows:

Powder-snow: The ideal snow.

Wet snow: Results in slow skiing.

Crust-snow: Fast but rather hazardous.

M. H. Davis, Supervisor of the Shasta National Forest, submitted the following list:

Soft: A powdery, cold, fairly fast snow.

Wet fast: Consisting of snow that has lain long enough to be fairly well packed but not crusted.

Clogging or slow snow: Signifying newly fallen soft snow which is fairly wet because of comparatively high temperatures.

Light crusted: Self-explanatory.

Icy: Signifying snow with a heavy crust on the surface or a thin layer of ice.

Finally, we had a list from John C. Preston, Superintendent of Lassen Volcanic National Park, which after some cogitation we decided to use. We have had no criticism of these terms, which encourages me to think they are what was wanted. Mr. Preston's definitions follow:

Packed snow: This is settled snow which will support the weight of skiers. Normal packed snow is snow which will support skiers on skis, allowing them to sink in from one to four inches. Icy packed snow is snow which will completely support skiers on the surface, presenting a hard unbreakable surface to skis.

Soft snow: This is soft, unpacked snow into which skiers will sink more than four inches. Further classification as to consistency should include powder (meaning light, fluffy snow), medium (meaning normal soft snow), and wet (meaning heavy, wet snow).

Variable snow: This is snow which, as implied, may be both soft and packed. Wind often packs snow in spots while other portions of the surface are soft.

Breakable crust: This is snow with a surface-crust which breaks under the weight of the skier. It is treacherous and skiers should be warned in advance of its existence in any area on a particular week-end.

Division of Irrigation,
U. S. Bureau of Agricultural Engineering,
Berkeley, California

DISCUSSION

BESTOR ROBINSON (Chairman of the Winter Sports Committee of the Sierra Club)--In a recent letter Mr. Robinson includes the following, which is added as a discussion of the foregoing papers: "Most winter sports enthusiasts do all of their skiing in two or three areas only and are reasonably familiar with these areas. The most important factor in determining the general condition of the snow is the time elapsed since the last snowfall. I would suggest, accordingly, giving consideration to including this item in addition to the other matters covered in each area."

EASTERN SIERRA SNOW-SPORTS

George Deibert

I cannot help but feel I am a little young in experience to be telling snow-surveyors anything about their business. However, I had one experience on a snow-survey that will long be remembered.

Two of us started out one morning from Bishop for a course over Bishop Pass. The course lay on the other side of a 12,000-foot summit some 16 or 17 miles away. The going was fine up to a mile or two below the summit. There a blizzard hit us full in the face, and tougher going I never hope to see. We had hoped for a two-mile free ride down the other side, but no, with a 40- or 50-mile gale coming head on and visibility only a few feet, going down the hill was as laborious as going up.

We arrived at the approximate location of the shelter-house just as darkness was coming on, and after several circles had been made, we located the top of the man-well, which was just even with the top of the snow, some 15 feet deep. We located the top of the stove-pipe about five feet below the surface of the snow. A fire was started with great difficulty, supper made, and a good night's sleep had. Before breakfast could be had, we had to dig the stove-pipe out again. We finished running the course about one o'clock, and after a hurried meal started back over the summit, arriving a few miles from our car at dark.

During these 48 hours we had covered about 36 miles and scaled about 7,000 feet of altitude, and we did it under the hazardous conditions of rocky cliffs, blizzard, corn-ice, high altitude, and low temperatures. Had we not found the shelter-house when we did, or had the wind quit blowing, the snow would have wiped out all trace of the shelter-house and we would still be over there hunting.

I relate this story for the benefit of those of you who might have to do with the building of shelter-houses. Mark them well and make them comfortable.

We on the eastern slope of the Sierras have to offer some of the finest ski-slopes to be found anywhere. As you know, our side of the mountains breaks off from the summit more abruptly than those on the western slope. Glacier- and water-erosions have formed huge fans and moraines, some of which are miles in extent. They are for the most part free from timber, and usually have a fairly even surface, with a pitch of 15° to 50°

We usually have good skiing snow in December, and it lasts through to April and May. In fact, one could have good skiing all summer on Bishop Pass, 30 miles out of Bishop. We also have some outstanding peaks that will tickle the palate of any skier. Fifteen miles out of Bishop is Mount Tom, 13,649 feet high. The south slope is open, and offers a ten- or twelve-mile run with few interruptions. Dunderberg Mountain, 12,374 feet, starts at Conway Summit, 8,300 feet, seven hours up and one hour back. Dr. Walter Mosauer told me that was the finest run he ever had.

Mammoth Mountain, 11,000 feet. Drive up to Mammoth Lakes, 9,000 feet, a beautiful run, part open and part timber. An "up ski" was installed last year on McGee Mountain, summit 10,800 feet, base 7,000 feet. It extends some 2,500 feet up the Mountain and gains about 1,000 feet in altitude. This is skier's paradise, all open and all degrees of pitch. Our ski-slopes usually have a snow-depth of three to seven feet. Highway No. 395, the section of the Three Flags Highway between Reno and Los Angeles, skirts the foot of the main range, about 25 to 30 miles air-line from the summit, for a distance of some 200 miles. State Highway equipment keeps this scenic highway open all year.

The Eastern Sierra Ski-Club was organized in Bishop last year, and has made fair strides in developing the sport. We have applied for membership in the California Ski-Association. At present we have about 75 active members. Last year we developed about 50 skiers, mostly C and B class. This year we hope to double that and make enough A-class skiers to do all your snow-surveying.

We are also active in promoting skating. There are many high glacier-lakes in our vicinity. The higher ones start freezing in November, and as they are snowed under we drop down to the lower ones. The June Lake Area in Mono Basin affords some very fine skating during December and January.

We are now in the process of laying out several ski-runs, varying in length from one to 18 miles, and are looking forward to the establishing of several "ski-huts" in the back country.

Bishop, California

POSSIBILITIES OF COMMERCIALIZING AND DEVELOPING WINTER SPORTS NEAR CRYSTAL BAY, LAKE TAHOE, NEVADA

Forrest L. Rhodes

My sincere enthusiasm for the development of a winter-sports area at Lake Tahoe, Nevada, is based on actual study and investigation. It has been an honor for me to assist in the study of this project and state the results.

The area under consideration is not only suitable for winter, but ideal for summer vacationists. The beauty of Lake Tahoe itself has been universally praised. The huge magnificence of the high Sierra-Nevada Mountains that encompass the Lake adds materially to the splendor of it. The heavy growth of timber surrounding the Lake and extending far up into the Sierras lends a silent enchantment which grips the spirit of beauty in everyone. On ascending to the highest reaches of the sky-line, one is dwarfed by the panorama which is pictured for more than a hundred miles in all directions. From one spot alone, five lakes--Donner, Tahoe, Gray, Fallen Leaf, and Lahontan--are easily visible, with a wonderful view of Mount Lassen to the far north, and the path of the Truckee River is traceable for miles. The high mountains toward the central part of Nevada, the mountains many miles past the southern end of Lake Tahoe, and those far into the west in California make a bold relief against the sky-line.

Therefore, it could only be a matter of time until such a place came to the attention of able-bodied businessmen with a clear sense of development. Such was the case and now the evolution of this beauty spot is being pushed forward as rapidly as possible. Along with the natural wonders of the region, other appurtenances are to be built to blend in with topography and background which has necessitated the purchase of large tracts of land. A noted architect is now working on a design for a large hotel to be located on the Lake shore--a base around which all activities will be centered. To facilitate the skiing, a number of ski-tows will be built--a nursery school for the ski beginners--instructors will be available, and a number of ski-trails will be cleared. For the skating enthusiasts, an ice-rink will probably be constructed near the main hotel. Skijoring, tobogganing, and a bob-sled run will also be incorporated in the final plans.

In the summer, the pleasure of boating and swimming at Lake Tahoe is familiar to most everyone. The finest beaches on the shores of the Lake are located in the immediate vicinity of the main hotel. One beautiful sandy beach alone is approximately 7,000 feet in length. For the people who enjoy hiking, a number of blazed trails to the higher slopes and lakes will be available. A number of paths along the high Sierras for short horseback trips and also overnight trips will be coursed and marked. This will probably necessitate the building of small mountain lodges to ensure the pleasures of the trips.

In the United States, skiing is estimated to be better than a twenty million dollar industry and it is only a part of winter sports. Therefore, it has become a science in itself and needs the help of water- and snow-science to direct the choice of areas for the public. More people are probably praying for a white winter in order to enjoy a successful winter-sports season than there are people who hope for the same but who seek the benefits to be derived from the water-content. More people everyday wish to get out into the brisk winter air, away from city life and everyday routine, for they are learning that that is the real test of recreation. But the choice of many people in seeking outdoor recreation is controlled by the period of time it takes to reach the various sports-areas. Also the length of the season of almost every one of the present popular winter-sports centers is too short. Both the easy accessibility and the extra long season of the proposed area at Lake Tahoe, Nevada, are tremendous assets in its development (Fig. 3).

Main air-lines land in Reno, Nevada, an easy hour's drive to the Lake. Main highways from Oregon, Washington, Los Angeles, Salt Lake City, and New York intersect at Reno. Those driving from San Francisco can reach the Lake from Truckee, California. The Southern Pacific Railroad and the Western Pacific Railroad, both serve Reno, as well as do two main stage-lines.

Excellent skiing is to be had from early November to the latter part of May and during some years the season has been known to be still longer. At the present time, two nationally known skiers are stationed at the highest slopes. A comfortable, well-equipped cabin has been built from which all kinds of observations are being taken. Wind, temperature, snow-depth, storm-conditions, and so forth are duly noted and recorded. These two men stay up in the mountains for five days and come into Reno for two days, the trip to the cabin taking around six hours from Floriston and the trip out taking around one hour. The maximum elevation in the territory covered is very close to 10,000 feet with the cabin located near the 9,200 foot contour and the beginning

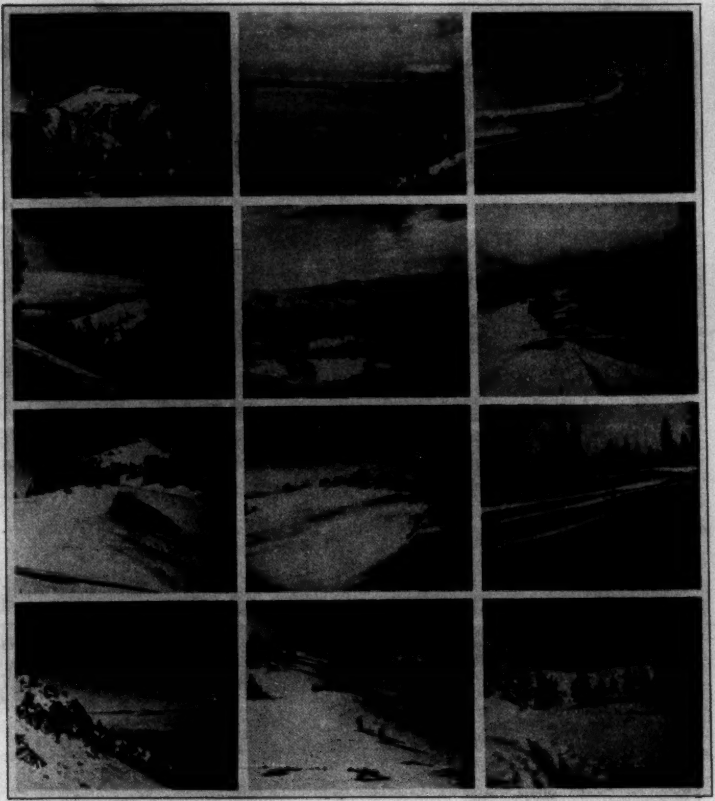


FIG. 1—GENERAL VIEWS: 1, MOUNT ROSE; 2, GABLE PEAK; 3, RIFLE PEAK; 4, INCLINE; 5, STATE-LINE POINT; 6, CRYSTAL BAY; 7–12, GENTLE SLOPES AND LONG DESCENTS (PHOTOGRAPHS BY HALVER MIKKELSEN AND WAYNE POULSEN)

of the trip to the cabin from Tahoe is 6,225 feet above sea-level.

The two skiers have already made several interesting discoveries which will be of value to both the winter-sports science and the sport-loving public. Needless to say, these valuable points that are being revealed every day, lend tremendous encouragement to the backers of the project.

At the present time there are four feet of snow on the ground while many other sports-areas have hardly enough for fair skiing. The two men skied in to the cabin on Thanksgiving and found conditions wonderful for all types of skiing. As time passed their enthusiasm increased because, out of the first 40 days, only three days presented poor skiing. Another extraordinary factor was the condition of the snow under different temperatures. Since their first trip in, when the thermograph was started, the lowest temperature recorded was 7° F and the highest 30° F. But the snow appeared to remain unchanged from its original state, a so-called "powder-snow".

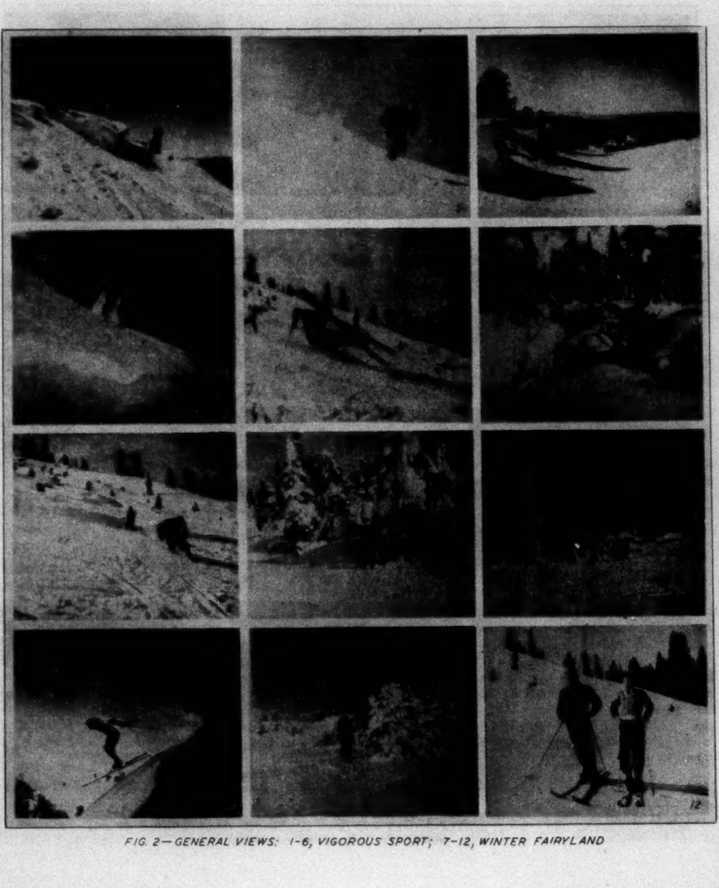


FIG. 2—GENERAL VIEWS: 1-6, VIGOROUS SPORT; 7-12, WINTER FAIRYLAND

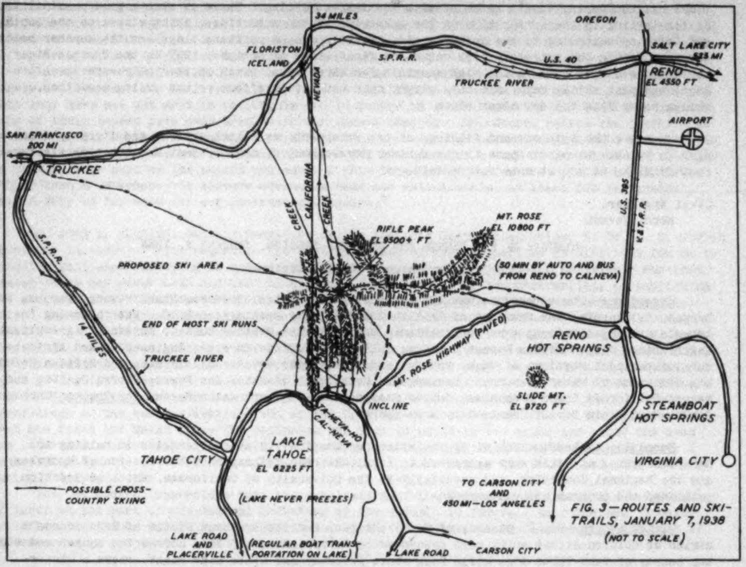

34 MILES
OREGON
FLORISTON
ICELAND
S.P.R.R.
U.S. 40
SALT LAKE CITY
535 MI
RENO
EL 4550 FT
TRUCKEE RIVER
NEVADA
AIRPORT
SAN FRANCISCO
200 MI
TRUCKEE
CREEK
CALIFORNIA
CREEK
U.S. 395
V.&T.R.R.
S.P.R.R.
RIFLE PEAK
EL 9500+ FT
MT. ROSE
EL 10800 FT
PROPOSED SKI AREA
(50 MIN BY AUTO AND BUS
FROM RENO TO CALNEVA)
END OF MOST SKI RUNS
14 MILES
MT. ROSE HIGHWAY (PAVED)
RENO
HOT SPRINGS
TRUCKEE RIVER
SLIDE MT.
EL 9720 FT
INCLINE
TA-NEVA-HO
CAL-NEVA
STEAMBOAT
HOT SPRINGS
LAKE
TAHOE
EL 6225 FT
TAHOE CITY
VIRGINIA CITY
POSSIBLE CROSS-
COUNTRY SKIING
(LAKE NEVER FREEZES)
TO CARSON CITY
AND
450 MI—LOS ANGELES
LAKE ROAD
AND
PLACERVILLE
(REGULAR BOAT TRANS-
PORTATION ON LAKE)
LAKE ROAD
CARSON CITY

FIG. 3—ROUTES AND SKI-TRAILS, JANUARY 7, 1938
(NOT TO SCALE)

The latest report from the men when they returned from the cabin Thursday past (January 6, 1938) added further beneficial knowledge. They found clear skies and warm weather for most of the past week while Reno and the valley of the Truckee River were blanketed by a high cloud-bank. The temperatures were also higher than they had been previously, varying from 22° F to 50° F, and the snow still remained in the desired "powdered" form. There was also six inches of new snow.

Many areas do not offer any suitable slopes for skiing during a snow-storm, but in this locality there are a number of slopes which are protected from the severest part of the storm. The prevailing winds seem to come from the southwest and the high ridges running north and south offer protection to the eastern slopes. The maximum wind-velocity noted so far was 60 miles per hour, but was of comparatively snort duration. This may even be exceeded, but such high velocities generally occur in sudden gusts. Due to the numerous slopes to choose from, at no time did the skiers find the wind too severe for skiing. The strongest winds seem to occur just before and at the beginning of the storms.

In many resorts, the runways for slalom and long easy ski-runs are hampered by a heavy, thick growth of timber, while other localities are practically treeless. The first place offers a severe handicap for the outdoors sportsmen while the second presents a bleak, uninteresting view. The proposed locale is beautified by tall stately trees of mostly second growth. Most of the very old and dead trees have been logged off, giving slopes of clear timber with open paths making possible easy skiing. The skiers have already spotted a number of long trails varying from six to ten miles in length, with some of these six-mile trails presenting the possibility of averaging 40 miles per hour for the entire run. As a matter of fact, the two men ventured the opinion that there are enough slopes to accommodate one thousand people without the slightest appearance of being crowded.

As for a brief discussion of the directional slopes, the eastern slopes are protected by the ridges running north and south as the prevailing wind is from the southwest. The northern side of the eastern and western slopes are all "powder-snow" after the storms with some ice-spots on the very top. The southeast and generally all the southern slopes are granular snow, or as sometimes called "spring snow", which permits the fastest skiing. There is also a good possibility of fine skiing in the spring time on the Lake side of the main ridge, which slopes to the south, and later by switching to the opposite side of the ridge--a northern slope--as the weather becomes warmer. During the extremely high water and flood-stage of December 1937 on the Truckee River there was a slight rain on the high mountain for three days, which incidentally were the aforementioned poor skiing days, but this slight rain had little effect on the skiing condition, skiing being fine the day after storm.

Such are the opinions and findings of two prominent, excellent skiers, and it is my sincere wish to be able to participate in the further development of this project and to bring its current findings to you at some future date.

Civil Engineer,
Reno, Nevada

MINUTES OF THE DINNER AND BUSINESS SESSION, JANUARY 8, 1938

Dean George D. Clyde, Chairman

Attendance--The attendance was 32, comprising the States of Idaho, Utah, Nevada, Washington, Oregon, California, the Province of British Columbia, and Washington, D. C. The following interests and organizations were represented: United States Weather Bureau; Bureau of Agricultural Engineering; United States Forest Service; Colleges of Agriculture and Engineering and Agricultural Experiment Stations of Utah, Nevada, and California; Nevada, California, and British Columbia Divisions of Water Resources; Los Angeles Department of Water and Power; Sierra Pacific and Nevada-California Power Companies; United States District Court Water Master for Truckee-Carson Basin; California Harbor Commission; snow-sports.

Resolutions--Resolutions of appreciation for hospitality and cooperation in holding the Snow-Conference at Davis were expressed to local Chairman Veihmeyer of the Section of Hydrology and the Regional Committee, and especially to the University of California, which in addition published the program without expense.

Winter sports--Dr. F. Howard, of San Francisco, and the Brockman Studio of Reno loaned a series of colored slides which were presented by Forrest Rhodes. They showed the sunset and winter beauty of Lake Tahoe from Rifle Peak above Brockway and State Line Point, where a Winter-Sports Syndicate is planning a winter resort similar to Sun Valley in Idaho. The climate of Tahoe is modified by the never-freezing Lake and the elevation of Rifle Peak is such that the snow-season is long and the snow-fields are scarecely affected by abnormal rains such as largely denuded the Sierra Nevadas of their covering the present winter. Accessibility is assured by auto road to the base of Rifle Peak or by train to Tahoe City and steamer thence to Brockway. From the Lake edge to the mountain rim, every variety of ski-field is available.

Views upon Tahoe and winter scenes along its shores rival those from Tamalpais and Rigi at Lake Lucerne, Switzerland. The browns and greens of the forest-trees against the fluffy white of the snow and the deep blue of the Lake and the rich colors of the sunset lift winter sports into the realm of dreams. Professional skiers and enthusiastic amateurs are already frequenting these grounds. H. I. Snider, United States Forest Ranger of the Tahoe National Forest, considers this area for sports and beauty both by winter and by summer, as a "Little Switzerland", and firmly believes that it will attain this reputation.

Future plans of the Conference--Chairman Clyde reported that a questionnaire sent to the Executive Committee revealed that three members favored dissolution and joining some other organization and four were non-committal. The other four did not reply.

Vice-Chairman Church of the Section of Hydrology stated that the Regional Committee for the South Pacific Coast area, where the present meeting was being held, felt that the present affiliation between the Conference and the Section of Hydrology of the American Geophysical Union was anomalous because the former retained practical independence while merging its publications with those of the latter; consequently that the Conference should come closer or retire farther.

To determine the attitude of the national officers of the Section of Hydrology, Dr. Church sent an inquiry to the past Chairman and present Executive Committee of the Section, and presented the following correspondence as a permanent foundation for cooperation, if acceptable to the Conference:

Chairman LE ROY K.SHERMAN of the Section of Hydrology, to Dr. Robert E. Horton, Executive

Committee of the Section (December 11, 1937)--"Thank you for posting me on the action of the American Society of Agricultural Engineers in appointing a Committee on Hydrology. . . We should take note of this Committee in a friendly way. I believe they have certain functions that call for their existence in applied hydrology as a separate unit from the American Geophysical Union.

"Along this same line, Dr. Church has written me concerning the Western Interstate Snow-Survey Conference. You will recall they have an affiliation with the Section of Hydrology in that they have met out west in California and in Denver in connection with our Committee on Snow. Many of their papers have been printed in our annual reports. Dr. Church raises the question as to whether this Western Interstate Snow-Survey Conference should be amalgamated with our American Geophysical Union or whether it should continue its existence as a sort of associate. My thought is that a large part of the papers and work of this Snow-Survey Conference relates to technique rather than to science. It appears advisable that the relationship, at least for the present, should stay as is, both for our interest and theirs."

Dr. JOHN A. FLEMING, General Secretary of the American Geophysical Union, to Dr. J. E. Church (December 11, 1937)--"With regard to the last paragraph of your letter, it is difficult for me to conceive that snow-surveying is not one of the basic features of hydrologic activity. For that reason there has never been any serious question in my mind as to the desirability of publishing the proceedings of the Snow-Survey Conferences in the Transactions of the American Geophysical Union. My only disappointment has been that it has been necessary, because of the small funds available for publication in the Union, to call upon you and your colleagues for financial assistance. It seems to me that, on the whole, it would be unwise to make an entirely separate and distinct publication of the Snow-Survey Conferences since this would mean a lack of unity as regards geophysical endeavor in our Union. So far as the comments that have been received regarding the Transactions are concerned, they seem unanimously favorable to the publication of the proceedings of the various sections and of the Snow-Survey Conferences. One of the real problems that has faced the Union since the beginning has been to unite in one organ and under one head the interests and contacts of scientific investigators in geophysical fields. In such unity I am certain that the interest of geophysical research in our country has been distinctly advanced during the life of the Union.

"You, of course, appreciate that the publication of the Transactions involves a large amount of labor on the part of the General Secretary of the Union. So far as I am concerned, I have felt that this labor was fully compensated for by the advantages which have accrued to those of us concerned in geophysics through the Union."

Dr. O. E. MEINZER, Past Chairman, Section of Hydrology, to Dr. J. E. Church (December 30, 1937)--"Dr. Fleming talked with me some time ago in regard to your problem as to the relation of the Snow-Survey Conference to the Section of Hydrology, and he has, I believe, written to you on that subject. I believe he and I are in agreement that it is desirable to continue the contact between the two organizations, and to include the papers of the Snow-Survey Conference in the Transactions, as has been done heretofore. You doubtless will be best able to judge if, in the future, it seems desirable to merge the Snow-Survey Conference completely with the Section of Hydrology."

Vice-Chairman J. E. CHURCH sent the following questionnaire to other members of the Executive Committee, Section of Hydrology (December 30, 1937), through Chairman Sherman (other members are Secretary Beij, Dr. Horton, and Dr. Lowdermilk):

"Dear Chairman Sherman:

"To help us round out more fully your opinion and a letter received from Secretary Fleming, we are asking you to obtain, if possible, for presentation at the Davis meeting, a formal opinion of the membership of the Executive Committee of the Section of Hydrology regarding the desirability of continuing the present affiliation between the Section of Hydrology and the Western Interstate Snow-Survey Conference.

"The question of discussion at the dinner meeting of the Western Interstate Snow-Survey Conference will be the future organization of that body. The point has been raised that the Snow-Survey Conference, whether directed by the Western Interstate Snow-Survey Conference or by the Regional Committee of the Section of Hydrology, should either be more closely organized as a part of the Section of Hydrology or should be more independent of it, the reason advanced being that the present relationship of joint meetings and joint publication in the Transactions of the American Geophysical Union creates an anomaly.

"The possible reason against the closer organization is the more popular character of snow-survey papers caused in part by the pioneer-period through which snow-surveying is now passing and the desire to stress technical points that pertain more to perfecting methods than to underlying principles of snow-hydrology. It seems particularly desirable to increase morale through giving the men on the snow-survey courses an opportunity to present their experiences and their problems. It is, therefore, questionable whether a Regional Committee could serve this broader purpose while pursuing the scientific phases of hydrology.

"On the other hand, it seems desirable for the Snow-Survey Conference to be held in close affiliation with the Section of Hydrology in order to obtain the inspiration and leadership of the latter and also to have the advantage of having its proceedings printed by the American Geophysical Union and circulated among its members and to the many libraries and organizations that form part of its mailing list. There is also the further advantage in this plan of publication, namely, that means can readily be procured from supporting organizations for meeting the cost of publishing the Snow-Survey Proceedings--a thing which might be impossible if the Snow-Survey Conference were merged into the American Geophysical Union.

"It has been suggested that both points of view might be more nearly satisfied by having a member on each of the regional committees of the Section of Hydrology whose particular interest would be in developing a Snow-Survey Conference at each of the regional meetings. This can be done through conference and cooperation with the Western Interstate Snow-Survey Conference or, in case the Conference fails to act, a separate conference can be called, as was done at Denver by the Regional Committee itself. An organization of this kind can readily be extended to the Eastern States so that a Snow-Survey Conference can be held either in connection with any regional meetings that may be established along the Appalachians or a conference can be called in affiliation with the national meeting at Washington. Word has now been received that a Snow-Survey Conference has already been held at Boston to organize the snow-survey interests in the New England States.

"This problem seems to be an urgent one at the present time, and its final solution would seem to lie in a gradually increasing organic union with the Section of Hydrology of the American Geophysical Union. This latter idea has been suggested by General Secretary Fleming of the American Geophysical Union, who is seeking to draw into one organization all interests concerned with the geophysics of the Earth."

To this questionnaire the following answers were received:

Dr. W. C. LOWDERMILK, Member of Executive Committee, Section of Hydrology, to Dr. Church (January 4, 1938)--"Your letter of December 30 has just arrived and I hasten to reply, sending the letter to Davis by air-mail and a copy to Mr. Sherman. Apparently there is some advantage for the Interstate Snow-Survey Conference to have its meetings and proceedings independent of the Regional Committee of the Section of Hydrology in the interests of support for publication of proceedings and for greater attention to technique of snow-surveying during the developmental phase of the work. On the other hand, it is highly desirable that the Snow-Survey Conference should be closely affiliated with the Section of Hydrology. Accordingly, I favor having a member of each of the regional committees of the Section of Hydrology responsible for developing Snow-Survey Conferences at regional meetings. As these Conferences become more technical, they may become incorporated into the meetings of the Section of Hydrology."

K. HILDING BEIJ, Secretary, Section of Hydrology, to Dr. Church (January 4, 1938)--"The copy of your letter of December 30 to Mr. Sherman has just arrived. The question you raised seems to me to be a very important one and I wish I felt better qualified to make suggestions as to its solution.

"The organization of the Section of Hydrology is rapidly becoming so involved and diffuse that the Secretary is even now finding it difficult to keep the proper records of activities. In addition to ten research-committees, we have special committees, regional committees, and cooperating committees, as well as the Executive and the Program committees. And yet we have no direct tie-up with the seven associations of the International Union. Therefore, as a possible means of coordination of some of the Section's activities as well as a solution of your problems, I venture the following suggestion. I believe that right now, while you are head of the International Commission on Snow, the time is favorable for such an attempt at coordination; and perhaps the remaining activities of the Section may be coordinated in a similar manner. Briefly, the suggestion is this:

"(1) Form the American Commission on Snow as an integral part of the Section of Hydrology. The Executive Committee of the Section should appoint a Chairman, a Vice-Chairman, and a Secretary, these three officers to act in an executive capacity and to represent the United States on the International Commission.

"(2) The American Commission on Snow should be composed of: (a) The Research Committee on Snow, whose function is the scientific study of the hydrology of snow; (b) the Western Interstate Snow-Survey Conference, whose function remains as at present; (c) other interstate snow-survey conferences; (d) special committees in the field of the Commission.

"There remains the problem of financing meetings and publications. It is my opinion that dues should be raised to five dollars per year, but the Section has no power to do this. Hence, we must, for the time being, seek help from cooperating organizations and individuals as in the past. However, I believe we should work towards the goal of financial independence with dues sufficient to cover expenses and commensurate with the benefits received."

LE ROY K. SHERMAN, Chairman, Section of Hydrology, to Dr. Church (January 4, 1938)--"Your letter of December 30, 1937, raises an important and rather far-reaching question: Should there be an amalgamation, or at least a much closer relation, between the Section of Hydrology and the Western Interstate Snow-Survey Conference? May I consider first a few fundamentals.

"The Section of Hydrology was formed, as I understand, for several reasons: First, because there was no association for the meeting of those interested in hydrology as a science; second, there was no medium for the publication of scientific investigations and studies on hydrology, unless they were disguised as engineering or technical applications; and third, there was a recognition by scientists, as well as engineers, that hydrology heretofore considered on the basis of empirical rules and direct applications was failing to meet the requirements of modern technical progress.

"The engineering and technical societies, such as the American Society of Civil Engineers, the American Society of Agricultural Engineers, associations for reclamation, water-supply, forestry, and drainage are all rendering valuable contributions to hydrology. We should recognize and encourage their work; we should hold joint meetings with them, as portions of our work and theirs overlap. But we should not take over their functions and make the Section of Hydrology another engineering or technical society. There are plenty of technical societies now. Let us not forget that we primarily are interested in hydrology; their interest is incidental. We are a part of the International Association of Scientific Hydrology. It is difficult and, I think, undesirable to draw a sharp line between papers on hydrology and papers on applications of hydrologic principles. The terminal fringe of our realm, however, is well defined.

"At the present time, there is a trend that favors the popularizing of science and the free inclusion of technical applications. I refer, for example, to the comments before the recent Indianapolis meeting of the American Association for the Advancement of Science to the effect that science should cultivate the general public. Dr. Bowie, in a good letter to me, said: 'We should not take a high-brow attitude in regard to geophysics and stress only the research phase of it. We should, in our lectures and writings, stress the practical importance of developing geophysics in all of its phases as a means of making this world a better place in which to live. Dr. Bowie and I probably agree as to what constitutes high-brow stuff, but the general public would undoubtedly include in this category much more of our work.

"The record of the Section of Hydrology, in the work of its special advisory committees, shows that we are not self-centered, that we are glad to cooperate, and that we wish to aid the technician in placing his structures upon a firm foundation.

"This leads me to the following conclusions. I refer to the Section of Hydrology and not necessarily to the American Geophysical Union.

"(1) There should be cooperation and joint meetings with any organizations having any common interests with us or with any Committee of our Section.

"(2) The publication of selected outside papers on hydrology by the American Geophysical Union is desirable, provided such papers are approved by our Publication Committee and provided the publication-cost thereof is borne by the outside organization.

"(3) A permanent working federation, with associations interested only incidentally in hydrology or interested mainly in the technical applications of hydrology, is not desirable for our Section.

"The work of the Snow-Survey Conference is closely linked with our Committee on Snow. There should be cooperation between the two. It is my opinion that it would be better for the Snow-Survey Conference to keep its own organization and not depend upon us for the publication of all of its papers. The same remarks apply to such as the American Society of Agricultural Engineers and others.

"In my opinion, it is desirable to keep up our affiliations with the Western Interstate Snow-Survey Conference, subject to the limitations above-mentioned. This letter reflects only my individual opinions. I am sending copies of it to each member of the Executive Committee and to Dr. Bowie and Dr. Fleming."

Dr. R. E. HORTON, Member of Executive Committee, Section of Hydrology, to Dr. Church (January 7, 1938)--"There are some marked advantages in continuing the doing of some things in the same way in which they have been done in the past, under the name of the Interstate Snow-Survey Conference.

"As long as the meetings are held under the auspices of any one of the regional committees of the Section of Hydrology and papers are presented and published in accordance with the established rules of the Section of Hydrology, there cannot be, as I see it, any valid objection to continuing the present procedure of the Interstate Snow-Survey Conference and calling it that name. It might be well to add 'Interstate Snow-Survey Conference of the Section of Hydrology.'"

Dr. C. S. SCOFIELD, Past Chairman, Section of Hydrology, to Dr. Church (January 8, 1938)--"I think you can shape up the problem this way, namely, strengthen the activities and the scope of the Committee on Snow of the American Geophysical Union and maintain it. Concentrate in the Snow-Survey Conference all the popular and applied features and practical phases, such as the power-companies, Government bureaus, irrigation-companies, and agencies are willing to pay for.

"Maintain the Conference as a separate unit. Present to the meetings of the American Geophysical Union the technical and scientific papers.

"There would be no difficulty in arranging for publication by the American Geophysical Union if there is financial need.

"You should develop rather than diminish the mechanical and practical side of the Conference. The man who does the sampling should feel that he has a place to come to present his problems.

"Better develop this Committee on Snow for the technical stuff and let the Snow-Survey Conference go in the direction toward which it would naturally go. Maintain this affiliated association as long as it is desirable for the Snow-Survey Conference to do so. Do not be influenced by the American Geophysical Union. Make the well-being of the Snow-Survey Conference the main point on which you base your decision."

Dean CLYDE, however, urged that the Conference act independently of the Section because of the financial support implied in joint publications with it. However, it was pointed out that the Section provided editorial and publication conveniences but without actual outlay of money.

Furthermore, Mr. BERNARD, representing one of the supporting organizations of the Conference, pointed out that the high rating acquired by the Conference through its close affiliation with the International Union of Geodesy and Geophysics was its best assurance of continued support. Mr. ELGES pointed out that the present cost of reprints could not possibly be borne by membership dues. In fact, Mr. DUKES felt that the members of the Conference were not members in their own personal interest but in that of their organizations.

The following letter written after the meeting by Major MacDONALD of British Columbia urged affiliation with the Section of Hydrology for the sake of perpetuity: "I cannot see how the Snow-Survey Conference can well be made a part of the Section of Hydrology of the Union. There is a large personnel which should take part in the Conference which cannot well be members of the Union. On the other hand, the success of such an informal gathering as a Snow-Survey Conference depends to a very large extent on one or two keen and active individuals. If they disappear or become indifferent, the Conference may very well die. It should be tied in some way with some more permanent organization which will take the responsibility of keeping the informal organization alive. I do not see how you can well change the present situation. It appears to me that the Section of Hydrology is the logical body to foster the activities of the Conference. If it does not assume that responsibility, the Conference and the important work it is doing may very easily die out."

It was finally proposed and adopted that the Conference remain fluid and independent as at present with opportunity to meet with any organization that might provide a focal point for the gathering of snow-survey interests for discussion.

Membership requirements were merely interest in the work, and all who attended the Conference were considered members, without necessary registration or dues.

The present Executive Committee was expanded to 15 in order to include British Columbia and Alberta among the Western States. It was also decided to scatter the members of the Committee as widely as possible throughout the area to provide a nucleus in each of the four regions for the organization of conferences when opportunity was afforded; the feeling also prevailed that all interests should be represented. Since the Committee was widely scattered, it was agreed that five should constitute a quorum and that discussions must be carried on mainly by correspondence.

To afford close membership where desired with the American Geophysical Union and the Section of Hydrology--one of its subdivisions--it was pointed out that membership was open to all who had an interest in hydrology, and that at least associate membership would be granted to non-residents of the United States in case they desired particularly to join.

To clear up confusion regarding the various organizations in snow, it was stated that the Research Committee on the Hydrology of Snow was one of the research-groups of the Section of Hydrology and the International Commission of Snow is a similar research-group in the International Association of Scientific Hydrology. The International Commission, the American Committee, and the Snow-Survey Conference are completely independent of each other, despite the fact that some individuals are on all three.

The Conference represents the practical problems arising from the pioneer nature of the work. The Committee on Snow and the International Commission of Snow represent the research-endeavor and are identical except geographically. All are concerned with the human aspects of snow.

Election of officers--The former Executive Committee of eleven was reelected with the exception of Harlowe M. Stafford, who withdrew in favor of Fred Paget, his successor in the California Cooperative Snow-Surveys. To complete the Committee, the following members were nominated and elected: Fred Paget, California; Richard C. Farrow, British Columbia; O. H. Hoover, Alberta; H. T. Gisborne, Montana; E. B. Debler, Colorado, or whoever should be suggested by him.

The members of the Committee present elected Professor H. P. Boardman of Nevada as Chairman, and the choice was confirmed by the meeting. On motions of Mr. Jones and Mr. Lewis, it was voted to continue affiliation as at present with the Section of Hydrology until the Executive Committee has had opportunity to ascertain "what other means for affiliation with any other similar organization there may be to accomplish present ends," and to report back to the Conference. It was also voted to instruct the Executive Committee to "investigate means of financing future operations and meeting expenses that may occur."

Personnel list of Executive Committee--To aid in the interchange of problems and experiences between members of the Committee and snow-survey interests, the following detailed address-list is given:

Professor H. P. Boardman, University of Nevada, Reno, Nevada
James E. Jones, Los Angeles Department of Water and Power, Los Angeles, California
Dean George D. Clyde, State Agricultural College, Logan, Utah
Professor Joseph Kittredge, Jr., University of California, Berkeley, California
James E. Marr, U. S. Bureau of Agricultural Engineering, Boise, Idaho
R. L. Parshall, U. S. Bureau of Agricultural Engineering, Fort Collins, Colorado
R. A. Work, U. S. Bureau of Agricultural Engineering, Medford, Oregon
J. C. Alter, U. S. Weather Bureau, Salt Lake City, Utah
Lynn Crandall, U. S. Geological Survey, Idaho Falls, Idaho
F. H. Paget, Department of Public Works, Sacramento, California
R. C. Farrow, Department of Lands, Victoria, British Columbia, Canada
O. H. Hoover, Dominion Water and Power Bureau, Calgary, Alberta, Canada
H. T. Gisborne, U. S. Forest and Range Experiment Station, Missoula, Montana
E. B. Debler, U. S. Bureau of Reclamation, Denver, Colorado

Mailing list--To provide a background for a more complete mailing list of proceedings and other reprints, Mrs. Alice White, Secretary of the Department of Meteorology, Nevada Agricultural Experiment Station, provided the Conference with the following analysis of the mailing list now being built up for the joint use of the Committee on Snow and the Snow-Survey Conference, and other interests in snow and ice.

A--Federal	
Bureau of Agricultural Engineering, particularly Division of Irrigation	19
Bureau of Agricultural Economics and Livestock Estimates	4
Soil Conservation Service	40
Weather Bureau	81
Forest Service (general)	171
Forest and Range Experiment Stations	21
Division of Grazing	1
National and State Parks	11
Bureau of Reclamation	68
Irrigation-districts and Government projects	23
Geological Survey (Water-Resources Branch)	28
National Resources Committee	4
Coast Guard	1
Federal Power Commission	3
War Department (River and Harbor Division)	4
Unclassified	2

B--State and city	
State Engineers and Water Commissions	49
City Engineers (including hydrographers and snow-surveyors not classified)	9
Irrigation-Districts	11
Lock and canal companies	3
Power-companies	49
Recreation	6
Highway engineers	3
State Planning Board	2
C--Education and research	
Agricultural Extension Service	8
Agricultural Experiment Stations	16
Equipment and instrument-makers	4
Universities and research-institutes	47
International Commission of Snow	126
Editors and authors	14
Libraries	10
D--General	
Farmers, ranchers, and others	10
Total	844

Classification of mailing list by regions and States

West		
North Pacific Coast (Lower Columbia and Fraser rivers)		77
Alaska	2	
British Columbia	5	
Washington	27	
Oregon	43	
South Pacific Coast (Great Valley and Lower Colorado Watershed)		218
North Continental Divide (Missouri and Columbia watersheds)		78
Idaho	31	
Washington (eastern)	10	
Montana	35	
Alberta (Canada)	2	
South Continental Divide (Arkansas, Rio Grande, and Colorado watersheds)		134
Utah	42	
Wyoming	23	
Colorado	50	
New Mexico	19	
Administrative		63
Washington, D. C.	62	
Ottawa, Canada	1	
Northeastern Canada (Montreal)		2
Mississippi Basin		
South Central States		19
Arkansas	2	
Oklahoma	2	
Texas	5	
Kansas	2	
Missouri	3	
Mississippi Basin--Concluded		
South Central States--Concluded		
Kentucky	2	
Mississippi	1	
Louisiana	2	
North Central States		49
South Dakota	10	
North Dakota	4	
Nebraska	12	
Iowa	1	
Minnesota	3	
Wisconsin	8	
Michigan	3	
Ohio	5	
Illinois	2	
Winnipeg (Canada)	1	
Atlantic Seaboard		
Northern Appalachians		48
Maine	6	
New Hampshire	4	
Massachusetts	10	
Connecticut	4	
New York	15	
Pennsylvania	7	
New Jersey	2	
Southern Appalachians and South		12
Virginia	2	
West Virginia	1	
North Carolina	4	
Tennessee	1	
Georgia	2	
Maryland	2	

Acknowledgment--The Snow-Survey Conference is under deep obligation to Mrs. Alice White of the Nevada Agricultural Experiment Station and to Darrel B. Crook of the Los Angeles Department of Water and Power for generous assistance in recording the meeting.

Similar obligation should be expressed to the Section of Hydrology, the Regional Committee of the South Pacific Coast Area, and the University of California for generous cooperation in holding the Conference, and to the American Geophysical Union for including the Proceedings in the *Transactions*.

The following organizations, through bearing the cost of publication or purchasing reprints, have aided greatly in extending interest in the work: Nevada, Utah, and Colorado Agricultural Experiment Stations; Nevada State Engineer; Arizona State Water Commissioner; California Division of Water Resources; British Columbia Water Rights Branch; Los Angeles Department of Water and Power; Southern California Edison Company; Nevada-California Electric Corporation; The Metropolitan Water District of Southern California; Los Angeles County Flood Control District; Sierra Pacific Power Company; Pacific Gas and Electric Company; U. S. Weather Bureau; Division of Irrigation, U. S. Bureau of Agricultural Engineering; U. S. Bureau of Reclamation; U. S. Forest Service; National Park Service; Engineers, U. S. War Department.

(Signed) J. E. Church,
For the Executive Committee

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